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Articles and Essays by Marvin Zetterbaum, Thomas Schwartz, Neal E. Cutler, Lester M. Salamon and John J. Siegfried, George Von der Muhll, Peter H. Merkl, Richard M. Merelman

Richard F. Fenno, Jr.

U.S. House Members in Their Constituencies

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Corrupt Politicians and Their Electoral Support

Abraham H. Miller, Louis H. Bolce, and Mark Halligan

The J-Curve Theory and the Black Urban Riots

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Provocative News

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224 pages; \$6.50/paper. 1976. ISBN 0-06-041267-4.



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ARTICLES

U.S. House Members in Their Constituencies: An Exploration. The paper addresses itself to two questions left underdeveloped in the literature on representative-constituent relations. First, what does the representative see when he or she sees a constituency? Second, what consequences do these perceptions have for his or her behavior? The paper reverses the normal Washington-oriented view of representative-constituent relations and approaches both questions by examining the representative in his or her constituency. The paper's observations are drawn from the author's travels with seventeen U.S. House members while they were working in their districts. Member perceptions of their constituency are divided into the geographical, the reelection, the primary and the personal constituencies. Attention is then given to the home style of House members. Home style is treated as an amalgam of three elements — allocation of resources, presentation of self, explanation of Washington activity. An effort is made to relate home style to the various perceived constituencies. Some observations are made relating constituency-oriented research to the existing literature on representation.

By RICHARD F. FENNO, JR., Professor of Political Science, University of Rochester.

Partisan Patterns of House Leadership Change, 1789-1977. This study of 364 leadership selections in the U.S. House from 1789 through 1977 discovered that Democrats have a higher proportion of appointed leaders than Republicans; their leaders move between posts in an ordered succession; their appointed leaders are often "removed from above" by their elected ones; and their leaders are subjected to infrequent and unsuccessful caucus challenges. Republicans rely upon election to choose their leaders; their leaders' rate of interpositional mobility is very low; their appointed leaders were never removed by their elected ones; and their leaders face the contests at the same rate as the Democrats do, but the incidence of successful challenges is much greater. They are "removed from below."

Majority vs. minority status had little statistically significant impact upon leadership contests and what variation appeared indicated that challenges were more frequent in the majority party where the stakes are higher and the rewards are greater than in the minority. Regardless of electoral consequences, however, Republican leaders are more vulnerable to caucus defeat than Democratic ones, which lends further support to the contention that party identity is more important than party status.

By GARRISON NELSON, Associate Professor of Political Science, University of Vermont.

Party Realignment and the Transformation of the Political Agenda: The House of Representatives, 1925–1938. According to Walter Dean Burnham, party realignments "result in significant transformation in the general shape of policy." Through the analysis of House roll-call data, the New Deal realignment is examined to determine whether, in fact, a significant transformation took place and, if so, what its characteristics were. It was hypothesized that if a new political agenda emerged at that time, at least some of the stable policy dimensions which Aage Clausen finds as characterizing the modern Congress should have developed during the New Deal period. In terms of content and level of partisan voting evoked, the government management and the agricultural policy dimensions do take their modern form during the New Deal. A social welfare dimension developed but had not, by the late 1930s, taken its modern shape. It is argued that a major transformation of policy did take place and that, in the process, the ideological distance between the parties increased. This realignment, however, did not immediately change regional voting patterns within each party.

By BARBARA DECKARD SINCLAIR, Associate Professor of Political Science, University of California, Riverside.

954 Corrupt Politicians and Their Electoral Support: Some Experimental Observations. This paper concerns the relationship between voters and corrupt politicians. An explanation is suggested for why voters would discount even credible information that a candidate is corrupt. Then the results of an experiment designed to test a necessary condition in this explanation are reported. The principal implication of this exploratory study is that corrupt elected officials are immune from electoral reprisal because voters rather easily trade off the information that a candidate is corrupt in return for other things they value in the candidate.

By BARRY S. RUNDQUIST, Visiting Associate Professor of Political Science, University of Illinois at Chicago Circle; GERALD S. STROM, Assistant Professor of Political Science, University of Illinois at Chicago Circle; and JOHN G. PETERS, Assistant Professor of Political Science, University of Nebraska-Lincoln.

The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation
 Theory. A time-series analysis of individual level, perceptual data disconfirms the J-curve theory of the
 black urban riots (i.e., that they arise because a period of progress was followed by a sharp decline) and

suggests that ambiguities surrounding black people's perceptions of their economic situation probably led to the frustration that culminated in urban violence.

the frustration that culminated in urban violence.

The methodological component of the research deals with such problems of relative deprivation-based research as: (1) the substitution of aggregate, objective-level indicators for perceptual theoretical concepts; (2) the correspondences between objective and perceptual data on both a point-by-point basis and across time-series patterns; (3) the empirical implications of failing to look at important subgroup distinctions; and (4) the crucial assumption of all forms of relative deprivation theory that future expectations of need fulfillment, especially in the period of rising satisfactions, are a function of current levels of need fulfillment. The research calls for modifications in the structure and application of relative deprivation theory in light of the findings in these areas.

By ABRAHAM H. MILLER, Professor of Political Science, University of Cincinnati; LOUIS H. BOLCE, Assistant Professor of Political Science, Fordham University, Lincoln Center; and MARK HALLIGAN, Law Student, Northwestern University Law School.

Equality and Human Need. This paper has the twofold purpose of exploring how and whether it may be said that value arises from human need and, in particular, how the value of equality may arise from an alleged human need for recognition. It traces two opposite dispositions toward recognition, one seeing it as destructive of the minimum conditions for political life, the other viewing it as the principal agency through which men achieve their humanity. The concept of a basic human need is then exposed to the criticism of "social apperception," which apparently renders meaningless the concept altogether. Nevertheless, two "faces" of recognition are explored — one affirming that common human nature in virtue of which all men are said to be equal, and the other, affirming the concrete specificity of each individual. The paper concludes by arguing that this second aspect of the drive for recognition, which is viewed by some as the primary political obligation, is actually not a legitimate aspiration of political life.

By MARVIN ZETTERBAUM, Professor of Political Science, University of California, Davis.

Collective Choice, Separation of Issues and Vote Trading. In legislatures and committees, a number of issues are voted on separately, leading to an outcome consisting of positions on each of these issues. I investigate the effects this separation of issues has on collective choices, assuming a very abstract collective choice model, whose assumptions are presupposed by many less abstract models, notably spatial models. Assuming the model, if there exists an undominated outcome (one to which no winning coalition prefers any other feasible outcome), it must be chosen in the absence of vote trading, although vote trading can (perversely) lead to a very different outcome. But vote trading does not necessarily lead to a "voting paradox" situation, contrary to several recent papers. The model enables us to define a natural solution concept for the case where every feasible outcome is dominated. Variations on this concept are explored. The effects of weakening the model are investigated.

By THOMAS SCHWARTZ, Associate Professor of Government, University of Texas at Austin.

1011 Demographic, Social-Psychological, and Political Factors in the Politics of Aging: A Foundation for Research in "Political Gerontology." "Political gerontology" is the study of the political aspects of aging and the aged. Although psychology and sociology have research subfields concerning aging, this interest is just beginning to develop systematically within political science. Consequently, this article describes theory and research from several disciplines which together provide a foundation for research in "political gerontology." Demographic analysis suggests that old people constitute a continuously growing component of the American population. Social-psychological analysis indicates that the aged are likely to engage in substantial political activity. Political analysis suggests that old people are likely to make increasing demands upon the political system. This multidisciplinary knowledge base, combined with the predicted increasing political salience of the aging population, suggests the contours of a research agenda for "political gerontology."

By NEAL E. CUTLER, Associate Professor of Political Science and Chief, Social Policy Laboratory, University of Southern California.

Economic Power and Political Influence: The Impact of Industry Structure on Public Policy. Political scientists have devoted considerable attention to the ways in which economic power can be translated into political influence. Yet there has been little empirical research capable of confirming or denying general hypotheses about the political implications of various aspects of economic structure. This article seeks to begin filling this gap by first identifying five aspects of economic structure likely to affect an industry's political influence (firm size, industry size, market concentration, profitability, and geographic dispersion) and then testing these aspects by analyzing how well they account for variations among industries in their success at securing public policies of benefit to them, especially in two policy arenas: federal corporate income taxes and state excise taxes. What emerges most clearly from this analysis is an empirical confirmation of the popular hypothesis linking firm size to political influence with respect to both federal corporate tax policy and state excise tax policy. Beyond that, we find reasonably strong negative relationships between political influence and market concentration, profitability, and industry

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size — the latter lending interesting support to Mancur Olson's argument about the political disabilities of large groups. In the process, the article suggests a potentially fruitful new way to get beyond the case study approach in studying the impact of economic power on political influence, and thus a way to bring to bear more powerful methodological tools on this central issue of modern democracy.

By LESTER M. SALAMON, Associate Professor of Political Science and Institute of Policy Sciences, Duke University (currently on leave serving as Deputy Associate Director, Office of Management and Budget, Washington, D.C.), and JOHN J. SIEGFRIED, Associate Professor of Economics, Vanderbilt University.

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By GEORGE VON DER MUHLL, Professor of Political Science, University of California, Santa Cruz.

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By PETER H. MERKL, Professor of Political Science, University of California, Santa Barbara.

On Social Psychological Handy Work: An Interpretive Review of The Handbook of Social Psychology, Second Edition. This paper reviews the new Handbook of Social Psychology, with a special eye towards its utility for political scientists. The review focuses on theory, methodology, substantive areas of social psychological research, and political applications of social psychological findings. Special attention is paid to Handbook articles of particular merit and application to political science. These include articles on cognitive theory, experimentation, observational analyses and sociometry, as well as articles which add to our knowledge of such politically important problems as reasoning, compliance, and decision making. Throughout, important findings relevant to the operations of politics are spotlighted. These include, interalia, cognitive biases towards the perception of unequal influence, the "risky shift," constraints on selective perception, and characteristics of leadership behavior. Omissions, theoretical flaws, and errors due to the "datedness" of findings are also discussed.

By RICHARD M. MERELMAN, Professor of Political Science, University of Wisconsin-Madison.

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U.S. House Members in Their Constituencies: An Exploration*

RICHARD F. FENNO, JR. University of Rochester

Despite a voluminous literature on the subject of representative-constituency relationships, one question central to that relationship remains underdeveloped. It is: what does an elected representative see when he or she sees a constituency? And, as a natural follow-up, what consequences do these perceptions have for his or her behavior? The key problem is that of perception. And the key assumption is that the constituency a representative reacts to is the constituency he or she sees. The corollary assumption is that the rest of us cannot understand the representative-constituency relationship until we can see the constituency through the eyes of the representative. These ideas are not new. They were first articulated for students of the United States Congress by Lewis Dexter. 1 Their importance has been widely acknowledged and frequently repeated ever since. But despite the acceptance and reiteration of Dexter's insights, we still have not developed much coherent knowledge about the perceptions members of Congress have of their constituencies.

A major reason for this neglect is that most of our research on the representative-constituency linkage gets conducted at the wrong end of that linkage. Our interest in the constituency relations of U.S. senators and representatives

*My I.O.U.'s lie scattered all over the United States among my friends in and out of academia. They are too numerous to list here. But Theodore Anagnoson, Viktor Hofstetter, John Kingdon, and Herbert McClosky deserve special thanks. So do the Russell Sage Foundation and the University of Rochester for their financial help. A slightly different version of this article was delivered under the title "Congressmen in Their Constituencies: An Exploration," at the American Political Science Association Convention in San Francisco, September, 1975. It is a part of a larger study, Home Style: U.S. House Members in their Constituencies (Boston: Little Brown, forthcoming).

¹Dexter's seminal article was "The Representative and His District," Human Organization 16 (Spring, 1957), 2-14. It will be found, revised and reprinted, along with other of Dexter's works carrying the same perspective in: Lewis Dexter, The Sociology and Politics of Congress (Chicago: Rand McNally, 1969).

has typically been a derivative interest, pursued for the light it sheds on some behavior - like roll call voting - in Washington. When we talk with our national legislators about their constituencies, we typically talk to them in Washington and, perforce, in the Washington context. But that is a context far removed from the one in which their constituency relationships are created, nurtured, and changed. And it is a context equally far removed from the one in which we might expect their perceptions of their constituencies to be shaped, sharpened or altered. Asking constituency-related questions on Capitol Hill, when the House member is far from the constituency itself, could well produce a distortion of perspective. Researchers might tend to conceive of a separation between the representative "here in Washington" and his or her constituency "back home," whereas the representative may picture himself or herself as a part of the constituency - me in the constituency, rather than me and the constituency. As a research strategy, therefore, it makes some sense to study our representatives' perceptions of their constituencies while they are actually in their constituencies - at the constituency end of the linkage.2

Since the fall of 1970, I have been traveling with some members of the House of Represen-

²Political science studies conducted in congressional constituencies have been few and far between. The most helpful to me have been: On Capitol Hill, ed. John Bibby and Roger Davidson (New York: Holt, Rinehart and Winston, 1967); On Capitol Hill, ed. John Bibby and Roger Davidson (Chicago: Dryden, 1972); John Donovan, Congressional Campaign: Maine Elects a Democrat (New York: Holt, Rinehart and Winston, 1957); Charles Jones, "The Role of the Campaign in Congressional Politics," in The Electoral Process, ed. Harmon Zeigler and Kent Jennings (New York: Prentice-Hall, 1966); John Kingdon, Candidates for Office: Beliefs and Strategies (New York: Random House, 1966); David Leuthold, Electioneering in a Democracy (New York: Wiley, 1968). And no one interested in this subject should miss Richard Harris's superb "How's It Look?", New Yorker, April 8, 1967. The most recent study is: The Making of Congressmen, ed. Alan Clem (North Scituate: Duxbury, 1976).

tatives while they were in their districts, to see if I could figure out - by looking over their shoulders - what it is they see there. These expeditions, designed to continue through the 1976 elections, have been totally open-ended and exploratory. I have tried to observe and inquire into anything and everything the members do. Rather than assume that I already know what is interesting or what questions to ask, I have been prepared to find interesting questions emerging in the course of the experience. The same with data. The research method has been largely one of soaking and poking or, just hanging around. This paper, therefore, conveys mostly an impressionistic feel for the subject - as befits the earliest stages of exploration and mapping.

As of June 1976, I had accompanied fourteen sitting House members, two House members-to-be and one House member-elect in their districts - for a minimum of two, a maximum of ten, and an average of five days each sometimes at election time, sometimes not. In eleven cases I have accompanied the member on more than one trip; in six cases I have made only one trip. Since I am a stranger to each local context and to the constellation of people surrounding each member, my confidence in what I see and hear increases markedly when I can make similar observations at more than one point in time. In ten cases I have supplemented my trips to the district with a lengthy interview in Washington. In the district, I reconstruct my record from memory and from brief jottings, as soon after the event as is feasible. In Washington, I take mostly verbatim notes during the interview and commit them to tape immediately thereafter.

I have tried to find a variety of types of members and districts, but I make no pretense at having a group that can be called representative, much less a sample. The seventeen include nine Democrats and eight Republicans. Geographically, three come from two eastern states; six come from five midwestern states; three come from three southern states; five come from three far western states. Since I began, one has retired, one has been defeated and one has run for the Senate. There is some variation among them in terms of ideology, seniority, ethnicity, race, sex, 3 and in terms of safeness

³The group contains sixteen men and one woman. In the title of the paper and in the Introduction, I have deliberately employed the generic language "House member," "member of Congress," "Representative," and "his or her" to make it clear that I am talking about men and women. And I have tried to use the same language wherever the plural form appears in the paper. That is, I have tried to stop using the word

and diversity of district. But no claim is made that the group is ideally balanced in any of these respects.

Perceptions of the Constituency

The District: The Geographical Constituency. What then do House members see when they see a constituency? One way they perceive it the way most helpful to me so far - is as a nest of concentric circles. The largest of these circles represents the congressman's broadest view of his constituency. This is "the district" or "my district." It is the entity to which, from which, and in which he travels. It is the entity whose boundaries have been fixed by state legislative enactment or by court decision. It includes the entire population within those boundaries. Because it is a legal entity, we could refer to it as the legal constituency. It captures more of what the congressman has in mind when he conjures up "my district," however, if we label it the geographical constituency. We retain the idea that the district is a legally bounded space and emphasize that it is located in a particular place.

The Washington community is often described as a group of people all of whom come from somewhere else. The House of Representatives, by design, epitomizes this characteristic; and its members function with a heightened sense of their ties to place. There are, of course, constant reminders. The member's district is, after all, "the Tenth District of California." Inside the chamber, he is "the gentleman from California"; outside the chamber he is Representative X (D. California). So, it is not surprising that when you ask a congressman, "What kind of district do you have?", the answer often begins with, and always includes, a geographical, space-and-place, perception. Thus, the district is seen as "the largest in the state, twenty-eight counties in the southeastern corner" or "three layers of suburbs to the west of the city, a square with the northwest corner

[&]quot;congressmen." In the body of the paper, however, I shall frequently and deliberately use "congressman" and "his" as generic terms. Stylistically, I find this a less clumsy form of the third person singular than "congressperson," followed always by "his or her." This usage has the additional special benefit, here, of camouflaging the one woman in the group. Where necessary, I have used pseydonyms for these seventeen members in the text.

⁴Additional evidence will be found in: Donald R. Matthews and James A. Stimson, Yeas and Nays: Normal Decision Making in the U.S. House of Representatives (New York: Wiley, 1975), pp. 28-31.

cut out." If the boundaries have been changed by a recent redistricting, the geography of "the new district" will be compared to that of "the old district."

If one essential aspect of "the geographical constituency" is seen as its location and boundaries, another is its particular internal make-up. And House members describe their districts' internal makeup using political science's most familiar demographic and political variables socioeconomic structure, ideology, ethnicity, residential patterns, religion, partisanship, stability, diversity, etc. Every congressman, in his mind's eye, sees his geographical constituency in terms of some special configuration of such variables. For example,

Geographically, it covers the northern onethird of the state, from the border of (state X) to the border of (state Y), along the Z river twenty-two counties. The basic industry is agriculture - but it's a diverse district. The city makes up one-third of the population. It is dominated by the state government and education. It's an independent minded constituency, with a strong attachment to the work ethic. A good percentage is composed of people whose families emmigrated from Germany, Scandinavia and Czechoslovakia. I don't exactly know the figures, but over one-half the district is German. And this goes back to the work ethic. They are a hardworking, independent people. They have a strong thought of 'keeping the government off my back, we'll do all right here.' That's especially true of my out-counties.

Some internal configurations are more complex than others. But, even at the broadest level, no congressman sees, within his district's boundaries, an undifferentiated glob.4 And we cannot talk about his relations with his "constituency" as if he did.

All of the demographic characteristics of the geographical constituency carry political implications. But as most Representatives make their first perceptual cut into "the district," political matters are usually left implicit. Sometimes, the question "what kind of district do you have?" turns up the answer "it's a Democratic district." But much more often, that comes later. It is as if they first want to sketch a prepolitical background against which they can later paint in the political refinements. We, of course, know – for many of the variables – just what those political refinements are likely to be. (Most political scientists would guess that the district just described is probably more Republican than Democratic - which it is.) There is no point to dwelling on the general political relevance of each variable. But one summary characterization does seem to have which to understand political perceptions and their consequences. And that characteristic is the relative homogeneity or heterogeneity of the district.

As the following examples suggest, members of Congress do think in terms of the homogeneity or heterogeneity of their districts - though they may not always use the words.

It's geographically compact. It's all suburban no big city in the accepted sense of the word and no rural area. It's all white. There are very few blacks, maybe 2 per cent. Spanish surnamed make up about 10 per cent. Traditionally, it's been a district with a high percentage of home ownership.... Economically, it's above the national average in employment ... the people of the district are employed. It's not that it's very high income. Oh, I suppose there are a few places of some wealth, but nothing very wealthy. And no great pockets of poverty either. And it's not dominated by any one industry. The X County segment has a lot of small, clean, technical industries. I consider it very homogeneous. By almost any standard, it's homogeneous.

This district is a microcosm of the nation. We are geographically southern and politically northern. We have agriculture - mostly soy beans and corn. We have big business - like Union Carbide and General Electric. And we have unions. We have a city and we have small towns. We have some of the worst poverty in the country in A County. And we have some very wealthy sections, though not large. We have wealth in the city and some wealthy towns. We have urban poverty and rural poverty. Just about the only thing we don't have is a good sized ghetto. Otherwise, everything you can have, we've got it right here.

Because it is a summary variable, the perceived homogeneity-heterogeneity characteristic is particularly hard to measure; and no metric is proposed here. Intuitively, both the number and the compatibility of significant interests within the district would seem to be involved. The greater the number of significant interests - as opposed to one dominant interest - the more likely it is that the district will be seen as heterogeneous. But if the several significant interests were viewed as having a single lowest common denominator and, therefore, quite compatible, the district might still be viewed as homogeneous. One indicator, therefore, might be the ease with which the congressman finds a lowest common denominator of interests for some large proportion of his geographical constituency. The basis for the denominator could be any of the prepolitical variables. We do not think of it, however, as a political characteristic - as the equivalent, for special usefulness as a background against instance, of party registration or political safe-

ness.⁵ The proportion of people in the district who have to be included would be a subjective judgment - "enough" so that the congressman saw his geographical constituency as more homogeneous than heterogeneous, or vice versa. All we can say is that the less actual or potential conflict he sees among district interests, the more likely he is to see his district as homogeneous. Another indicator might be the extent to which the geographical constituency is congruent with a natural community. Districts that are purely artificial (sometimes purely political) creations of districting practices, and which pay no attention to preexisting communities of interest are more likely to be heterogeneous.⁶ Pre-existing communities or natural communities are more likely to have such homogenizing ties as common sources of communication, common organizations, and common traditions.

The Supporters: The Re-election Constituency. Within his geographical constituency, each congressman perceives a smaller, explicitly political constituency. It is composed of the people he thinks vote for him. And we shall refer to it as his re-election constituency. As he moves about the district, a House member continually draws the distinction between those who vote for him and those who do not. "I do well here"; "I run poorly here." "This group supports me"; "this group does not." By distinguishing supporters from nonsupporters, he articulates his baseline political perception.

House members seem to use two starting points — one cross sectional and the other longitudinal — in shaping this perception. First, by a process of inclusion and exclusion, they come to a rough approximation of the upper and lower ranges of the re-election constituency. That is to say, there are some votes a member believes he almost always gets; there are other votes he believes he almost never gets. One of the core elements of any such distinc-

⁵Marginal districts probably tend to be heterogeneous. Safe districts probably are both heterogeneous and homogeneous. On the relationship of electoral conditions to homogeneity and heterogeneity, I owe a lot to my conversations with Morris Fiorina. See his Representatives, Roll Calls and Constituencies (Boston: Lexington, 1974).

⁶On the usefulness of the distinction, see Donald Stokes and Warren Miller, "Party Government and the Saliency of Congress," Public Opinion Quarterly 26 (Winter, 1962), 531-546. In M. Barone, G. Ujifusa, and D. Matthews, Almanac of American Politics (Boston: Gambit, 1974), the authors often describe congressional districts as artificial and containing no natural community of interest. See for example, pages 86, 133, 146, 162, 376, 402, 419, 373, 616.

tion is the perceived partisan component of the vote — party identification as revealed in registration or poll figures and party voting. "My district registers only 37 per cent Republican. They have no place else to go. My problem is, how can I get enough Democratic votes to win the general election." Another element is the political tendencies of various demographic groupings.

My supporters are Democrats, farmers, labor — a DFL operation — with some academic types.... My opposition tends to be the main street hardware dealer. I look at that kind of guy in a stable town, where the newspaper runs the community — the typical school board member in the rural part of the district — that's the kind of guy I'll never get. At the opposite end of the scale is the country club set. I'll sure as hell never get them, either.

Starting with people he sees, very generally, as his supporters, and leaving aside people he sees, equally generally, as his nonsupporters, each congressman fashions a view of the people who give him his victories at the polls.

The second starting point for thinking about the re-election constituency is the congressman's idea of who voted for him "last time." Starting with that perception, he adds or subtracts incrementally on the basis of changes that will have taken place (or could be made by him to take place) between "last time" and "next time." It helps him to think about his re-election constituency this way because that is about the only certainty he operates with he won last time. And the process by which his desire for re-election gets translated into his perception of a re-election constituency is filled with uncertainty. At least that is my strong impression. House members see re-election uncertainty where political scientists would fail to unearth a single objective indicator of it. For one thing, their perceptions of their supporters and nonsupporters are quite diffuse. They rarely feel certain just who did vote for them last time. And even if they do feel fairly sure about that, they may perceive population shifts that threaten established calculations. In the years of my travels, moreover, the threat of redistricting has added enormous uncertainty to the make-up of some re-election constituencies. In every district, too, there is the uncertainty which follows an unforeseen external event recession, inflation, Watergate.

Of all the many sources of uncertainty, the most constant — and usually the greatest — involves the electoral challenger. For it is the challenger who holds the most potential for altering any calculation involving those who voted for the congressman "last time." "This

time's" challenger may have very different sources of political strength from "last time's" challenger. Often, one of the major off-year uncertainties is whether or not the last challenger will try again. While it is true that House members campaign all the time, "the campaign" can be said to start only when the challenger is known. At that point, a redefini-.tion of the re-election constituency may have to take place. If the challenger is chosen by primary, for example, the congressman may inherit support from the loser. A conservative southern Republican, waiting for the Democratic primary to determine whether his challenger would be a black or a white (both liberal), wondered about the shape of his re-election constituency.

It depends on my opponent. Last time, my opponent (a white moderate) and I split many groups. Many business people who might have supported me, split up. If I have a liberal opponent, all the business community will support me.... If the black man is my opponent, I should get more Democratic votes than I got before. He can't do any better there than the man I beat before. Except for a smattering of liberals and radicals around the colleges, I will do better than last time with the whites.... The black vote is 20 per cent and they vote right down the line Democratic. I have to concede the black vote. There's nothing I can do about it.... [But] against a white liberal, I would get some of the black vote.

The shaping of perceptions proceeds under conditions of considerable uncertainty.

The Strongest Supporters: The Primary Constituency. In thinking about their political condition, House members make distinctions within their re-election constituency - thus giving us a third, still smaller concentric circle. Having distinguished between their nonsupporters and their supporters, they further distinguish between their routine or temporary supporters and their very strongest supporters. Routine supporters only vote for them, often merely following party identification; but others will support them with a special degree of intensity. Temporary supporters back them as the best available alternative; but others will support them regardless of who the challenger may be. Within each re-election constituency are nested these "others" - a constituency perceived as "my strongest supporters," "my hard core support," "my loyalists," "my true believers," "my political base." We shall think of these people as the ones each congressman believes would provide his best line of electoral defense in a primary contest, and label them the primary constituency. It will probably include the earliest of his supporters — those who recruited him and those who tendered identifiably strong support in his first campaign — thus, providing another reason for calculating on the basis of "last time." From its ranks will most likely come the bulk of his financial help and his volunteer workers. From its ranks will least likely come an electoral challenger.

A protected congressional seat is as much one protected from primary defeat as from general election defeat. And a primary constituency is something every congressman must have.

Everybody needs some group which is strongly for him — especially in a primary. You can win a primary with 25,000 zealots.... The most exquisite case I can give you was in the very early war years. I had very strong support from the anti-war people. They were my strongest supporters and they made up about 5 per cent of the district.

The primary constituency, I would guess, draws a special measure of a congressman's interest; and it should, therefore, draw a special measure of ours. But it is not easy to delineate - for us or for them. Asked to describe his "very strongest supporters," one member replied, "That's the hardest question anyone has to answer." The primary constituency is more subtly shaded than the re-election constituency, where voting provides an objective membership test. Loyalty is not the most predictable of political qualities. And all politicians resist drawing invidious distinctions among their various supporters, as if it were borrowing trouble to begin classifying people according to fidelity. House members who have worried about or fought a primary recently may find it somewhat easier. So, too may those with heterogeneous districts whose diverse elements invite differentiation. Despite some difficulty, most members - because it is politically prudent to do so - make some such distinction, in speech or in action or both. By talking to them and watching them, we can begin to understand what those distinctions are.

Here are two answers to the question, "who are your very strongest supporters?"

My strongest supporters are the working class — the blacks and labor, organized labor. And the

⁷See Donald B. Johnson and James R. Gibson, "The Divisive Primary Revisited: Party Activists in Iowa," *American Political Science Review* 68 (March, 1974), 67-77.

⁸The term is that of Leo Snowiss, "Congressional Recruitment and Representation," *American Political Science Review* 60 (September, 1966), 627-639.

people who were in my state legislative district, of course. The fifth ward is low-income, working class and is my base of support. I grew up there; I have my law office there; and I still live there. The white businessmen who are supporting me now are late converts — very late. They support me as the least of two evils. They are not a strong base of support. They know it and I know it.

I have a circle of strong labor supporters and another circle of strong business supporters.... They will 'fight, bleed and die' for me, but in different ways. Labor gives you the manpower and the workers up front. You need them just as much as you need the guy with the two-acre yard to hold a lawn party to raise money. The labor guy loses a day's pay on election day. The business guy gets his nice lawn tramped over and chewed up. Each makes a commitment to you in his own way. You need them both.

Each description reveals the working politician's penchant for inclusive thinking. Each tells us something about a primary constituency, but each leaves plenty of room for added refinements.

The best way to make such refinements is to observe the congressman as he comes in contact with the various elements of his re-election constituency. Both he and they act in ways that help delineate the "very strongest supporters." For example, the author of the second comment above drew a standing ovation when he was introduced at the Labor Temple. During his speech, he spoke directly to individuals in the audience. "Kenny, it's good to see you here. Ben, you be sure and keep in touch." Afterward, he lingered for an hour drinking beer and eating salami. At a businessman's annual Christmas luncheon the next day, he received neither an introduction nor applause when the main speaker acknowledged his presence, saying, "I see our congressman is here; and I use the term 'our' loosely." This congressman's "circle of strong labor supporters" appears to be larger than his "circle of strong business supporters." And the congressman, for his part, seemed much more at home with the first group than he did with the second.

Like other observers of American politics, I have found this idea of "at homeness" a useful one in helping me to map the relationship between politicians and constituents — in this

⁹For example, Theodore H. White, The Making of the President: 1960 (New York: Atheneum, 1961), pp. 276–278; Marshall Frady, Wallace (New York: New American Library, 1969), pp. 2–4, 38–39; Theodore H. White, The Making of the President: 1972 (New York: Atheneum, 1973), pp. 147, 414–415, 456–457.

case the perception of a primary constituency. House members sometimes talk in this language about the groups they encounter.

I was born on the flat plains, and I feel a lot better in the plains area than in the mountain country. I don't know why it is. As much as I like Al [whom we had just lunched with in a mountain town], I'm still not comfortable with him. I'm no cowboy. But when I'm out there on that flat land with those ranchers and wheat farmers, standing around trading insults and jibes and telling stories, I feel better. That's the place where I click.

It is also the place where he wins elections — his primary constituency. "That's my strong area. I won by a big margin and offset my losses. If I win next time, that's where I'll win it — on the plains." Obviously, there is no one-to-one relationship between the groups with whom a congressman acts and feels most at home and his primary constituency. But it does provide a pretty good unobtrusive clue. 10 So I found myself fashioning a highly subjective "at homeness index" to rank the degree to which each congressman seems to have support from and rapport with each group.

I recall, for example, watching a man whose constituency is dominantly Jewish participating in an afternoon installation-of-officers ceremony at a Young Men's Hebrew Association attended by about forty civic leaders of the local community. He drank some spiked punch, began the festivities by saying, "I'm probably the first tipsy installation officer you ever had," told an emotional story about his own dependence on the Jewish "Y," and traded banter with his friends in the audience throughout the proceedings. That evening as we prepared to meet with yet another and much larger (Democratic party) group, I asked where we were going. He said, "We're going to a shitty restaurant to have a shitty meal with a shitty organization and have a shitty time." And he did - from high to low on the "at homeness index." On the way home, after the meal, he talked about the group.

Ethnically, most of them are with me. But I don't always support the party candidate, and they can't stand that.... This group and half the other party groups in the district are against me. But they don't want to be against me too strongly for fear I might go into a primary and

¹⁰On recent trips, wherever possible, I have also asked each congressman at the end of my visit, to rank order the events of the visit in terms of their "political importance" to him and in terms of the degree to which he felt "at home" or "comfortable" in each situation.

beat them. So self-preservation wins out....
They know they can't beat me.

Both groups are Jewish. The evening group was a part of his re-election constituency, but not his primary constituency. The afternoon group was a part of both.

The Intimates: The Personal Constituency. Within the primary constituency, each member perceives still a fourth, and final, concentric circle. These are the few individuals whose relationship with him is so personal and so intimate that their relevance to him cannot be captured by their inclusion in any description of "very strongest supporters." In some cases they are his closest political advisers and confidants. In other cases, they are people from whom he draws emoţional sustenance for his political work. We shall think of these people as his personal constituency.

One Sunday afternoon, I sat in the living room of a congressman's chief district staff assistant watching an NFL football game with the congressman, the district aide, the state assemblyman from the congressman's home county, and the district attorney of the same county. Between plays, at halftime and over beer and cheese, the four friends discussed every aspect of the congressman's campaign, listened to and commented on his taped radio spots, analyzed several newspaper reports, discussed local and national personalities, relived old political campaigns and hijinks, discussed their respective political ambitions. Ostensibly they were watching the football game; actually the congressman was exchanging political advice, information, and perspectives with three of his six or seven oldest and closest political associates.

Another congressman begins his weekends at home by having a Saturday morning 7:30 coffee and doughnut breakfast in a cafe on the main street of his home town with a small group of old friends from the Rotary Club. The morning I was there, the congressman, a retired bank manager, a hardware store owner, a high school science teacher, a retired judge, and a past president of the city council gossiped and joked about local matters — the county historian, the library, the board of education, the churches and their lawns - for an hour. "I guess you can see what an institution this is," he said as we left. "You have no idea how invaluable these meetings are for me. They keep me in touch with my home base. If you don't keep your home base, you don't have anything."

The personal constituency is, doubtless, the most idiosyncratic of the several constituencies. Not all members will open it up to the outside observer. Nine of the seventeen did, however; and in doing so, he usually revealed a side of his personality not seen by the rest of his constituencies. "I'm really very reserved, and I don't feel at home with most groups - only with five or six friends," said the congressman after the football game. The relationship probably has both political and emotional dimensions. But beyond that, it is hard to generalize, except to say that the personal constituency needs to be identified if our understanding of the congressman's view of his constituency is to be complete.

In sum, my impression is that House members perceive four constituencies — geographical, re-election, primary, and personal — each one nesting within the previous one.

Political Support and Home Style

What, then, do these perceptions have to do with a House member's behavior? Our conventional paraphrase of this question would read: what do these perceptions have to do with behavior at the other end of the line - in Washington? But the concern that disciplines the perceptions we have been talking about is neither first nor foremost a Washington-oriented concern. It is a concern for political support at home. It is a concern for the scope of that support — which decreases as one moves from the geographical to the personal constituency. It is a concern for the stability of that support — which increases as one moves from the geographical to the personal constituency. And it ultimately issues in a concern for manipulating scopes and intensities in order to win and hold a sufficient amount of support to win elections. Representatives, and prospective representatives, think about their constituencies because they seek support there. They want to get nominated and elected, then renominated and re-elected. For most members of Congress most of the time, this electoral goal is primary. It is the prerequisite for a congressional career and, hence, for the pursuit of other goals. And the electoral goal is achieved – first and last – not in Washington but at home.

Of course, House members do many things in Washington that affect their electoral support at home.¹¹ Political scientists interpret a

¹¹ Two of the best studies ever on the point are David Mayhew, Congress: The Electoral Connection (New Haven: Yale University Press, 1974), and John Kingdon, Congressmen's Voting Decisions (New York: Harper and Row, 1973).

great deal of their behavior in Washington in exactly that way — particularly their roll-call votes. Obviously, a congressman's perception of his several constituencies will affect such things as his roll-call voting, and we could, if we wished, study the effect. Indeed, that is the very direction in which our conditioned research reflexes would normally carry this investigation. But my experience has turned me in another — though not, as we shall see an unrelated — direction. I have been watching House members work to maintain or enlarge their political support at home, by going to the district and doing things there.

Our Washington-centered research has caused us systematically to underestimate the proportion of their working time House members spend in their districts. As a result, we have also underestimated its perceived importance to them. In all our studies of congressional time allocation, time spent outside of Washington is left out of the analysis. So, we end up analyzing "the average work week of a congressman" by comparing the amounts of time he spends in committee work, on the floor, doing research, handling constituent problems - but all of it in Washington.¹² Nine of my members whose appointment and travel records I have checked carefully for the year 1973 – a nonelection year - averaged 28 trips to the district and spent an average of 101 working (not traveling) days in their districts that year. A survey conducted in 419 House offices covering 1973, indicates that the average number of trips home (not counting recesses) was 35 and the number of days spent in the district during 1973 (counting recesses) was 138.13 No fewer than 131, nearly one-third, of the 419 members went home to their districts every single weekend. Obviously, the direct personal cultivation

12The basic research was done by John Saloma, and is reported in his Congress and the New Politics (Boston: Little Brown, 1969), Chapter 6; in Donald Tacheron and Morris Udall, The Job of the Congressman (Indianapolis: Bobbs Merrill, 1966), pp. 280−288; and in Guide to the Congress of the United States (Washington: Congressional Quarterly, 1971), pp. 532ff. But no one has expanded Saloma's work. A pioneer work, which would have given us a wider perspective, but which seems to have been neglected is Dorothy H. Cronheim, "Congressmen and Their Communication Practices" (Ph.D. dissertation, University of Michigan, Department of Political Science, 1957).

13These surveys will be described in the next section of the paper. We shall not, however, again use the figures on total number of days spent in the district. They seem less reliable than the others, when checked against the few cases in which I have the complete record. Also, it should be noted that the number of cases for which the total number of days was collected was 401.

of their various constituencies takes a great deal of their time; and they must think it is worth it in terms of winning and holding political support. If it is worth *their* time to go home so much, it is worth *our* time to take a commensurate degree of interest in what they do there and why.

As they cultivate their constituencies, House members display what I shall call their home style. When they discuss the importance of what they are doing, they are discussing the importance of home style to the achievement of their electoral goal. At this stage of the research, the surest generalization one can make about home style is that there are as many varieties as there are members of Congress. "Each of us has his own formula — a truth that is true for him," said one. It will take a good deal more immersion and cogitation before I can improve upon that summary comment. At this point, however, three ingredients of home style appear to be worth looking at. They are: first, the congressman's allocation of his personal resources and those of his office; second, the congressman's presentation of self; and third, the congressman's explanation of his Washington activity. Every congressman allocates, presents, and explains. The amalgam of these three activities for any given representative constitutes (for now, at least) his home style. His home style, we expect, will be affected by his perception of his four constituencies.

Home Style: Allocation of Resources

Every representative must make a basic decision with regard to his home style: "How much and what kinds of attention shall I pay to home?" Or to put it another way: "Of all the resources available with which to help me do my job, which kinds and how much of each do I want to allocate directly to activity in the district?" There are, of course, many ways to allocate one's resources so that they affect the district. Our concern is with resources allocated directly to the district. Of these, we propose to look first, at the congressman's time and second, at the congressman's staff. The congressman's decision about how much time he should spend physically at home and his decision about how much of his staff he should place physically in the district are decisions which give shape to his home style.

Of all the resources available to the House member, the scarcest and most precious one, which dwarfs all others in posing critical allocative dilemmas, is his time. Time is at once what the member has least of and what he has the most control over. When a congressman divides up his time, he decides by that act what kind of congressman he wants to be. He must divide his time in Washington. He must divide his time at home. The decision we are concerned with here is the division of his time between Washington and home. When he is doing something at home, he must give up doing some things in Washington, and vice versa. So he chooses and he trades off; and congressmen make different allocative choices and different allocative trades. In this section, we shall focus on the frequency with which various congressmen returned to their districts in 1973.

"This is a business, and like any business you have to make time and motion studies," said one member. "All we have is time and ourselves, so we have to calculate carefully to use our time productively." It is not true, of course, that "all" the congressman has is time and himself. The office carries with it a large number of ancillary resources - a staff, office space, office expense allowances, free mailing privileges, personal expense allowances, etc. all of which draw attention when the advantages of incumbency are detailed. Each congressman chooses how he will utilize these resources. The most important of these choices are choices about how to use his staff. And among the key choices about staff is how to allocate them between Washington and the district. In this section we shall focus particularly on one indicator of that decision - the percentage of his total expenditures on staff salaries allocated by him to the salaries of his district staff.14

The information on trips home and staff allocation was collected on Capitol Hill in June 1974. Six students, each of whom had just finished working for four months, full-time in a congressman's office, conducted a survey by

14Other kinds of data were collected which might also be useful as an indication of district staff strength. Three of them correlated very highly with the indicator being used, so that it does not appear we are missing much by relying on one indicator. The measure we are using in the article — per cent of staff expenditures allocated to district staff — showed a correlation of .861 with number of people on the district staff, of .907 with the per cent of total staff members allocated to the district, and of .974 with the dollar amounts spent on district staff. Also recorded were the rank, in the total staff hierarchy, of the highest paid person on the district staff, as another indicator of district staff allocation practices. That indicator has not been used in the article, but it might be noted that the range is from first (i.e., the highest paid district staffer is the highest paid of all the congressman's staffers) to more than ninth (anything above nine was not recorded).

visiting each member's office and talking to his or her administrative assistant or personal secretary. The question about trips home usually produced an educated estimate. 15 The questions about staff vielded more precise answers. Each student presented the Clerk of the House's Report for 1973, with its list of each representative's staff members and their salaries; and the respondents simply designated which staff members were located in the district. A briefer, follow-up survey was conducted by four students with similar "Hill experience" in May 1975. This survey added to the store of information on those 1973 members who were still in Congress. 16 For 1973, it should be noted, each member was allowed a maximum of sixteen staff members and a maximum payroll of \$203,000. And for the two-year period 1973-1974 (the 93rd Congress) each member was reimbursed for thirtysix round trips to his district. Members were not, of course, required to use any of these allowances to the maximum.

On the matter of trips home, there is evidence of personal attentiveness to the district and of variation in that attentiveness. The average number of 1973 trips per member was thirty-five, and the median was thirty. The range went from a low of four trips to a high of three hundred and sixty-five.¹⁷ One can ask: "for which kinds of House members is their frequent physical presence in the district an important part of their home style and for which kinds is it less so?" At this early stage, we can present only a few suggestive relationships. In order to do so, we have categorized the frequency of trips home into low (less than 24), medium (24-42), and high (more than 42). The categories are based on the responses to the question and to the appearance of reasonable cutting points in the data.18 These

¹⁵See footnote 18.

16The 1974 interviewers were Larry Fishkin, Nancy Hapeman. Bruce Pollock, Kenneth Sankin, Fred Schwartz, and Jacob Weinstein. The 1975 interviewers were Joel Beckman, Joanne Doroshow, Arthur Kreeger, and Susan Weiner. Sandra Bloch, Fishkin, Hapeman, and Schwartz helped with the analysis. During the later stages of the analysis, I have leaned particularly heavily on Viktor Hofstetter.

17There were eleven members who went home every night — eight from Maryland, two from Virginia, and one from Pennsylvania. In computing averages, they were coded at ninety-eight trips (more than anyone else) rather than at 365, so as to minimize distortion. Also, so as to minimize distortion, caused by these cases, we have used the median number of trips in the analysis of this section rather than the average number of trips.

¹⁸The most common replies were "every week," "once a month," "twice a month," "every other

categories have been cross-tabulated with a number of variables that should be expected to correlate with the frequency of home visits.

One standard supposition would be that representatives in electoral jeopardy will decide to spend more of their time at home than will representatives whose seats are well protected. As a generalization, however, this supposition receives no confirmation when our conventional measures of electoral safeness are used. As Table 1 shows, the frequency of trips home does not increase as electoral margins decrease. Indeed, there is just not much of a relationship at all.¹⁹ It might be noted, in this connection, that objective measures of marginality have not fared particularly well in producing consistent findings whenever they have been used.20 My own experience leads me to believe that only subjective measures of electoral safety are valid. House members feel more uncertainty about re-election than is captured by any arbitrary electoral margin figures. Furthermore, uncertainty about their primary election situation is totally untouched by such figures. The point is that subjective assessments of electoral safeness might be more strongly correlated with trips home.

week," "between once and twice a month," "three or four times a month," etc. Congressmen placed in the "low" category were those whose staffers were unwilling to go as high as "twice a month." Congressmen whose trips were reported as "once a week" or more fell into the "high" category. But some respondents said "every week except for a few" or "every week, but maybe he missed one or two here or there." So, we decided to try to capture that sense by including in the "high" category people who were reported to have made somewhat less than fifty-two trips. (Doubtless, those who said they went home every week missed a few too.) Since a sizeable group had forty trips and none had forty-one or forty-two, the cut was made at that point, which made 43+ the "high category. The middle category were those who remained — people who went home at least twice a month (twenty-four trips) but not as often as forty-three times in 1973.

¹⁹The frequency of 1973 home visits does not bear any relationship to whether the member's electoral margin declined, increased, or remained the same between 1972 and 1974.

²⁰See Fiorina, Representatives, Roll Calls and Constituencies, Chapter One. For an analysis of why House members ought to worry, see Robert Erikcson, "A Reappraisal of Competition for Congressional Office: How Careers Begin and End," paper presented at Conference on Mathematical Models of Congress, Aspen, Colorado, 1974. The one piece of research which dovetails best with my research is Warren Miller, "Majority Rule and the Representative System of Government" in Cleavages, Ideologies and Party Systems, ed. Erik Allardt and Yrjo Littunen (Helsinki, 1964), Chapter 10. Miller uses subjective marginality as the measure of competitiveness and relates it to the policy attitudes of the congressman and his re-election constituency.

A related hunch would be that the longer a congressman is in office, the less time he will spend at home. Part of the argument here overlaps with the previous one - the longer in office, the more secure the seat. But the more important part of the reasoning would be that with seniority comes increased influence and responsibility in the House and, hence, the need to spend more time in Washington. These suppositions are supported by our data - but not as strongly and as consistently as we had imagined would be the case. A simple correlation between terms of service and number of trips home shows that as seniority increases, home visits decrease - as we would expect. But the correlation coefficient (Pearson's r) is an exceedingly weak -.235. When the data are grouped, however, the nature and strength of the relationship becomes clearer.

Table 2 pictures the relationship between the three categories of personal attentiveness and three levels of seniority. The summary. statistics continue to be unimpressive, because for the middle levels of seniority no allocative pattern is evident. But looking at the lowest and highest levels of seniority, it is clear that the frequency of home visits is much greater for the low seniority group than it is for the high seniority group. The relationship between length of service and trips home, we conclude. is not a consistent, linear relationship. But for those at the beginning of their House careers and those farthest along in their House careers, their longevity is likely to be one determinant of their decisions on time allocation. Congressional newcomers appear to be more singleminded in pursuing of the electoral goal than are the veterans of the institution.

A third reasonable guess would be that the more time-consuming and expensive it is to get to his district, the less frequently a congressman will make the trip. Leaving money aside (but recalling that for 1973-1974, each member was provided with a "floor" of thirty-six trips), we would expect to find that as distance from Washington increases, the number of trips home decreases. It is not easy to get a measure of distance that captures each member's travelling time. For now, we shall use region as a surrogate for distance, on the theory that if any relationship is present, it will show up in a regional breakdown. And it does. Table 3 indicates that the members nearest Washington. D.C. (East) spend a good deal more time at home than do the members who live farthest away from the Capitol (Far West). The less of his Washington time a member has to give up in order to get home, the more likely he is to go

Table 1. Trips Home and Electoral Margin

	Frequency of Trips Home (1973)			
Election Margin (1972)	Low (0-23)	Medium (24-42)	High (43+)	Total
Less than 55%	21 (29%)	28 (38%)	24 (33%)	73 (100%)
55-60%	18 (32%)	18 (32%)	20 (36%)	56 (100%)
6165%	22 (27%)	17 (20%)	44 (53%)	83 (100%)
More than 65%	68 (33%)	66 (32%)	73 (35%)	207 (100%)
•	129	129	161	419

Note: Gamma = -.03

home – at least at the extremes of distance. If distance is a factor for the other three regional groups, it does not show up here. Our guess is that the distance is a far more problematical factor in those cases. We shall, however, return to the regional variable shortly.

A persistent dilemma facing every member of Congress involves the division of time between work and family. And one of its earliest manifestations comes with the family decision whether to remain at home or move to Washington. If (for whatever reason) the family decides to remain in the district, we would expect the House member to go home more often than if the family moves to Washington. Table 4 shows that this is very much the case.21 Five times the percentage of representatives whose families remain at home fall into the high category of trips home as do represen-

²¹The number of cases is lower here than for the other parts of the analysis because the data were collected in 1975 – after a number of the 1973 congressmen were no longer available for questioning.

Table 2. Trips Home and Seniority

	Fr			
Seniority	Low (0-23)	Medium (24-42)	High (43+)	Total
Low (1-3 terms)	34 (22%)	44 (28%)	78 (50%)	156 (100%)
Medium (4-7 terms)	43 (28%)	59 (38%)	52 (34%)	154 (100%)
High (8+ terms)	52 (48%)	26 (24%)	31 (28%)	189 (100%)
	129	129	161	419
Mean seniority	7.0 terms	5.0 terms	4.7 terms	

Note: Gamma = -.30

Table 3. Trips Home and Region

		Frequency of Trips Home		
Region	Low (0-23)	Medium (24 <i>-</i> 42)	High (43+)	Total
East	5 (5%)	20 (20%)	76 (75%)	101 (100%)
South	36 (35%)	32 (30%)	36 (35%)	104 (100%)
Border	9 (26%)	10 (29%)	16 (45%)	35 (100%)
Midwest	29 (28%)	47 (44%)	30 (28%)	106 (100%)
Far West	50 (69%)	20 (27%)	3 (4%)	73 (100%)
	129	129	161	419

East: Conn., Me., Mass., N.H., N.J., N.Y., Pa., R.I., Vt. South: Ala., Ark., Fla., Ga., La., Miss., N.C., S.C., Tenn., Texas, Va.

Border: Del., Ky., Md., Mo., Okla., W.Va.

Midwest: Ill., Ind., Iowa, Kans., Mich., Minn., Neb., N.D., Ohio, S.D., Wisc.

Far West: Alaska, Ariz., Calif., Colo., Hawaii., Idaho, Mont., Nev., N.M., Oregon, Utah, Wash., Wyo.

Table 4. Trips Home and Family Residence

	Frequency of Trips Home			
Family Residence	Low (0-23)	Medium (24–42)	High (43+)	Total
Washington area	87 (41%)	89 (42%)	37 (17%)	213 (100%)
District Unmarried	3 (4%) 5 (14%)	6 (8%) 12 (32%)	69 (88%) 20 (54%)	78 (100%) 37 (100%)
	95	107	126	328

tatives whose families are in Washington – 88 per cent to 17 per cent. To put the finding somewhat differently, the average number of 1973 trips for members with families in Washington was twenty-seven, for members with families in the district it was fifty-two, and for unmarried members it was forty-four trips. Whether family decisions produce the home style or whether a home style decision produces the family decision remains an unanswered question. Either process seems perfectly plausible. It is clear, however, that some decisions about home style are family-related decisions.

To sum up, a House member's decision on how to allocate his time between home and Washington is affected: (1) by his seniority, if it is very low or very high; (2) by the distance from Washington to home, if that distance is very long or very short; and (3) by the place where his family is located, whether his family moves to Washington or remains in the district. A congressman's electoral margin, objectively measured, has little effect on his time allocations. How, if at all, these factors are interrelated, and how strongly each factor contributes to the allocative pattern are matters for later analysis.

Members of Congress also decide what kind of staff presence they wish to establish in the district. Here, too, we find great variation. On the percentage of total staff expenditure ellocated to district staff, the range, in 1973, went from 0 to 81 per cent. We might think that a member who decides to give a special degree of personal attention to "home" would also de-

cide to give a special degree of staff attentiveness to "home." But the relationship between the two allocative decisions does not appear to be strong. Using percentage of total staff expenditure on district staff (as we shall throughout this section) as the measure of district staff strength, we find a very weak correlation (Pearson's r = .20) between a congressman's decision on that matter and the number of trips he takes home. Table 5 clusters and cross-tabulates the two allocative decisions. For our measure of district staff strength we have divided the percentage of expenditures on district staff into thirds. The lowest third ranges from 0-22.7 per cent; the middle third ranges from 22.8-33.5 per cent; the highest third ranges from 33.6-81 per cent. The crosstabulation also shows a pretty weak overall relationship. For now, therefore, we shall treat the two decisions as if they were made independently of one another and, hence, are deserving of separate examination.

What kinds of members, then, emphasize the value of a large district staff operation? Once again, it turns out, they are not members in special electoral trouble. Table 6 displays the total lack of any discernible impact of electoral situation (objectively measured) on district staff strength. Nor, as indicated in Table 7, does seniority make any difference in staff allocative decisions. That it does not adds strength to the idea that the relationship between seniority and home visits discovered earlier is accounted for — as we have suggested — by career-and-goal factors rather than by electoral factors.

Table 5. Trips Home and District Staff Expenditures

		Frequency of Trips Hon	ne	
District Staff Expenditures	Low (0-23)	Medium (24–42)	High (43+)	Total
Lowest 1/3	53 (39%)	42 (31%)	40 (30%)	135 (100%)
Middle 1/3	42 (30%)	55 (40%)	41 (30%)	138 (100%)
Highest 1/3	31 (23%)	32 (23%)	75 (54%)	138 (100%)
•	126	129	156	411

Note: Gamma = .28

Table 6. District Staff Expenditures and Electoral Margin

Electoral Margin	District Staff Expenditures (1973)			
1972	Lowest 1/3	Middle 1/3	Highest 1/3	Total
Less than 55%	20 (27%)	28 (38%)	26 (35%)	74 (100%)
55-60%	20 (36%)	20 (36%)	16 (28%)	56 (100%)
61-65%	24 (29%)	29 (35%)	29 (35%)	82 (100%)
More than 65%	72 (36%)	61 (30%)	68 (34%)	201 (100%)
	136	138	139	413

Note: Gamma = -.04

Table 7. District Staff Expenditures and Seniority

	District Staff Expenditures			
Seniority	Lowest 1/3	Middle 1/3	Highest 1/3	Total
Low (1-3 terms)	44 (29%)	54 (35%)	56 (36%)	155 (100%)
Medium (4-7 terms)	44 (29%)	56 (37%)	51 (34%)	151 (100%)
High (8+ terms)	47 (44%)	28 (26%)	32 (30%)	107 (100%)
	136	138	139	413

Note: Gamma = -.13

Table 8. District Staff Expenditures and Family Residence

	District Staff Expenditures			
Family Residence	Lowest 1/3	Middle 1/3	Highest 1/3	Total
Washington area	85 (40%)	77 (37%)	48 (23%)	210 (100%)
District	21 (28%)	17 (23%)	37 (49%)	75 (100%)
Unmarried	8 (22%)	10 (28%)	18 (50%)	36 (100%)
	114	104	103	321

The other variables discussed earlier – family residence and distance - do not have the obvious implications for staff allocation that they have for the member's own time dilemmas. It might be that if his family is in the district, and if he plans to be home a lot, a congressman might decide to have a big district staff operation to work with him there. That supposition receives some support in Table 8. Another possibility is that members might decide to use a strong district staff to compensate for their lack of personal attention via trips home. However, this idea is not supported in Table 8, nor in Table 9, which seeks to uncover regional and distance patternings of district staffs. Representatives who live nearest to Washington (East) and who tend to go home the most do tend to have large district staffs. But representatives who live farthest away and tend to go home the least show only a slight tendency to compensate by allocating heavy expenditures to their district staffs.

Table 9, however, does reveal some regional allocation patterns that did not appear when we looked for regional patterns in home visits.

Region, it appears, captures a good deal more than distance, particularly in relation to staff allocations. The southern and border regions emerge with distinctive patternings here. To a marked degree, House members from these two areas eschew large staff operations in their districts. Scanning our two regional tabulations (Tables 3 and 9), we note that every region save the Midwest reveals a noteworthy pattern of resource allocation. In the East we find high frequency of home visits and large district staffs; in the Far West, we find a low frequency of home visits; in the southern and border regions, we find small district staffs. Again, the two types of allocative decisions appear to be quite distinct and independent. Region, we tentatively conclude, has a substantial affect on home style.

But regions are composites of several states; and while regional regularities often reflect state regularities, they can also hide them. Both situations have occurred in this instance. Figure 1 displays state-by-state allocation patterns of personal and district staff attentiveness. For each state delegation, we have computed the

Table 9. District Staff Expenditures and Region

		District Staff Expenditures		
Region	Lowest 1/3	Middle 1/3	Highest 1/3	Total
East	16 (16%)	31 (31%)	52 (53%)	99 (100%)
South	47 (46%)	36 (35%)	19 (19%)	102 (100%)
Border	18 (55%)	8 (24%)	7 (21%)	33 (100%)
Midwest	39 (37%)	34 (32%)	33 (31%)	106 (100%)
Far West	16 (22%)	29 (40%)	28 (38%)	73 (100%)
	136	138	139	413

Regions:

East: Conn., Maine, Mass., N.H., N.J., N.Y., Pa., R.I., Vt.

South: Ala., Ark., Fla., Ga., La., Miss., N.C., S.C., Tenn., Texas, Va.

Border: Del., Ky., Md., Mo., Okla., W.Va.

Midwest: Ill., Ind., Iowa, Kans., Mich., Minn., Neb., N.D., Ohio, S.D., Wisc.

Far West: Alaska, Ariz., Calif., Colo., Hawaii, Idaho, Mont., Nev., N.M., Oregon, Utah, Wash., Wyo.

mean number of trips home made by its members in 1973; and we have divided the state delegations into those whose averages fell above and below the median number of trips for all House members, i.e., 30 trips, Also, for each state delegation, we computed an average of the percentage of staff expenditures allocated to the district staff by its members; and we have divided the state delegations into those whose averages fell above and below the median percentage for all House members, i.e., 29 per cent. The result is a crude fourfold classification of states according to their combined personal and staff resource allocations to "home." The underlined states fall strongly into their particular patterns; the others display weaker tendencies. Each state is identified, also, by its regional classification.

There are, as Figure 1 shows, distinctive state allocative patterns. Some were fore-shadowed in the regional patterns discussed earlier. For example, the eastern states cluster in the high-trips home category; and the far western states cluster in the low-trips home category. Southern and border states cluster in the weak-district staff category. Other state patterns, however, appear here for the first time. The large number of states clustering in the low-trips-home/small-district-staff category, for example were totally obscured in our regional data.

Explanations for these various state clusters are more difficult; and only a few guesses can be made here. The sharp separation between eastern and far western states in trips home is doubtless a function of distance. And — it now appears more strongly — some far western state representatives do compensate for the infrequency of their home visits by maintaining a relatively large staff presence in the district. Yet if California members invest heavily in this

compensatory allocative strategy,²² why don't the members from Washington and Oregon do likewise?

The decision of most southern and border state representatives to spend relatively little on their district staff operations may be explainable by a tradition of nonbureaucratized, highly personalized politics in those areas. Northeasterners may be accustomed to coping with bureaucrats - legislative or otherwise - whereas southern and border state residents would expect to deal directly with the elective officeholder. Yet, in terms of the amount of personal attentiveness to their districts, southern delegations vary widely. At first glance, that variation seems to be related to distance - with states in the near South receiving more attention than states in the far South. But the marked difference among, say, Kentucky, Tennessee, and Alabama would seem to require a more complex explanation. And, speaking of complexity, the similar disposition of resources by the unexpected mix of delegations in the low-tripshome/small-district-staff category (in the lower right hand corner of Figure 1) defies even an explanatory guess at this stage of our study.

The allocative elements of home style vary across regions and among states within regions. The relevance of state delegations to patterns of resource allocation at home will come as no surprise to students of Congress. For there is virtually no aspect, formal or informal, of the legislative process on Capitol Hill that has not already revealed the importance of the state

²²An excellent study of district staff operations in California, containing many stimulating comments on the general subject is: John D. Macartney, "Political Staffing: A View From the District" (Ph.D. dissertation, University of California, Los Angeles, Department of Political Science, 1975).

Personal Attentiveness

District Staff Attentiveness	Above the Median in Trips Home	Below the Median in Trips Home
Above the Median in District Staff Expenditures	Connecticut (E) Massachusetts (E) New York (E) Tennessee (S) Illinois (MW) Maine (E) New Hampshire (E) Pennsylvania (E) Rhode Island (E) South Carolina (S) Vermont (E)	California (FW) Colorado (FW) Hawaii (FW) Idaho (FW) Iowa (MW) Kansas (MW) New Mexico (FW) Wyoming (FW)
Below the Median in District Staff Expenditures	Kentucky (B) Maryland (B) North Carolina (S) Virginia (S) West Virginia (B) Delaware (B) Indiana (MW) Montana (FW) Ohio (MW)	Alabama (S) Arizona (FW) Florida (S) Louisiana (S) Minnesota (MW) Oklahoma (B) Oregon (FW) Washington (FW) Wisconsin (MW) Arkansas (S) Michigan (MW) Nebraska (MW) South Dakota (MW) Texas (S) Utah (FW)

N.B. States which fall five trips or more above or below the median and whose district staff expenditures fall 5 per cent or more above or below the median are *italicized*. States not listed fall on the median in one or both instances. Regional classifications are in parentheses.

Figure 1. Allocation Patterns: By State

delegation.²³ How much that importance is the product of extensive communication among delegation members and how much the product of similar expectations emanating from similar districts, has not been definitively answered. Nor can it be here. All we can say is that both are probably involved. State delegation members probably talk to one another about their allocative practices and follow one another's example and advice. Also, certain expectations and traditions probably develop within states, or sections of states, so that members feel constrained to make resource allocations that are not too far out of line with those expectations.

23For example, Aage Clausen, How Congressmen Decide: A Policy Focus (New York: St. Martin's, 1973); Barbara Deckard, "State Party Delegations in the U.S. House of Representatives: A Study in Group Cohesion," Journal of Politics, 34 (February, 1972), 199-222; John Ferejohn, Pork Barrel Politics (Stanford University Press, 1974); John Kessel, "The Washington Congressional Delegation," Midwest Journal of Political Science, 8 (February, 1964), 1-21; Matthews and Stimson, Yeas and Nays.

The ambiguity of this discussion raises one of the broadest questions concerning home style. Is a congressman's home style something he chooses and then imposes upon his district or is it something that is imposed upon him by the kind of district he represents? We shall worry that question and work our way toward an answer as we proceed. At this point it appears that either or both patterns can hold: for, while state regularities testify to district influences on home style, some states display no regularities; and all states display enough idiosyncratic behavior to testify to the presence of individual choice.

Home style is, then, partly a matter of place — i.e., it is affected by the nature of the congressman's geographical constituency. That constituency is, after all, the closest thing to a "given" in his nest of perceptions. But home style is also partly a matter of individual choice. And in this respect, it can be affected by his perception of his other three constituencies. That, indeed, is what we expect to find as we move to discuss the other elements of home style.

Home Style: Presentation of Self

Most House members spend a substantial proportion of their working time "at home." Even those we placed in the "low frequency" category return to their districts more often than we would have guessed - over half of them go home more than once (but less than twice) a month.²⁴ What, then, do they do there? Much of what they do is captured by Erving Goffman's idea of the presentation of self.²⁵ That is, they place themselves in "the immediate physical presence" of others and then "make a presentation of themselves to others." A description of all the settings in which I have watched members of Congress making such presentations or "performances" as Goffman calls them, would triple the size of this article. But, surely, I have logged - during my thirty visits and ninety-three days in seventeen districts - nearly every circumstance concocted by the mind of man for bringing one person into the "immediate physical presence" of another.

In all such encounters, says Goffman, the performer will seek to control the response of others to him by expressing himself in ways that leave the correct impressions of himself with others. His expressions will be of two sorts - "the expression that he gives and the expression that he gives off." The first is mostly verbal: the second is mostly nonverbal. Goffman is particularly interested in the second kind of expression - "the more theatrical and contextual kind" - because he believes that the performer is more likely to be judged by others according to the nonverbal than the verbal elements of his presentation of self. Those who must do the judging, Goffman says, will think that the verbal expressions are more controlable and manipulable by the performer; and they will, therefore, read his nonverbal "signs" as a check, on the reliability of his verbal "signs." Basic to this reasoning is the idea that, of necessity, every presentation has a largely "promissory character" to it. Those who listen to and watch the presentation cannot be sure what the relationship between them and the performer really is. So the relationship must be sustained, on the part of those watching, by inference. They "must accept the individual on faith." In this process of acceptance, they will rely heavily on the inferences they draw from his nonverbal expressions — the expressions "given off."

Goffman does not talk about politicians; but politicians know what Goffman is talking about. Goffman's dramaturgical analogues are appropriate to politics because politicians, like actors, perform before audiences and are legitimized by their audiences. The response politicians seek from others is political support. And the impressions they try to foster are those that will engender political support. House member politicians believe that a great deal of their support is won by the kind of individual self they present to others, i.e., to their constituents. More than most people, they believe that they can manipulate their "presentation of self." And more than most other people, they consciously try to manipulate it. Certainly, they believe that what they say, their verbal expression, is an integral part of their "self." But, like Goffman, they place special emphasis on the nonverbal, "contextual" aspects of their presentation. At least, the nonverbal elements must be consistent with the verbal ones. At most, the expressions "given off" will become the basis on which they are iudged. Like Goffman, members of Congress are willing to emphasize the latter because, with him, they believe that their constituents will more readily discount what they say than how they say it or how they act in the context in which they say it. In the member's own language, constituents want to judge you "as a person." The comment I have heard most often from the constituents of my representatives is: "He's a good man," or "She's a good woman," unembossed by qualifiers of any sort. Constituents, say House members, want to "size you up" or "get the feel of you" "as a person," or "as a human being." And the largest part of what members mean when they say "as a person" is what Goffman means by "expressions given off."

So members of Congress go home to present themselves "as a person" — and to win the accolade, "He's a good man," "She's a good woman." With Goffman, they know there is a "promissory character" to the presentation. And their object is to present themselves "as a person" in such a way that the inferences drawn by those watching will be supportive ones. The representative's word for these supportive inferences is trust. It is a word they use a great deal. If a constituent trusts a House member, the constituent says something like: "I am willing to put myself in your hands temporarily; I know you will have opportunities to hurt me — though I may not know when

²⁴Fifty-six per cent, or 72 or 129.

²⁵Erving Goffman, *The Presentation of Self in Everyday Life* (New York: Doubleday, 1959). The language I have quoted appears in the Introduction and Conclusion.

those opportunities occur; I assume that you will not hurt me and I'm not going to worry about your doing so until it is proven beyond any doubt that you have betrayed that trust.' The ultimate response members of Congress seek is political support; but the instrumental response they seek is trust. The presentation of self - what is "given" in words and "given off" as a person - will be calculated to win trust. "If people like you and trust you as an individual," members often say, "they will vote for you." So trust becomes central to the congressmanconstituent relationship. Constituents, for their part - as Goffman would emphasize - must rely on trust. They must "accept on faith" that the congressman is what he says he is and will do what he says he will do. House members, for their part, are quite happy to emphasize trust. It helps to allay the uncertainties they feel about their support relationship with their various constituencies. If they are uncertain about how to work for support directly, they can always work indirectly to win a degree of personal trust that will increase the likelihood of support, or decrease the likelihood of opposition.

Trust is, however, a fragile relationship. It is not an overnight or a one-time thing. It is hard to win; and it must be constantly renewed and rewon. So it takes an enormous amount of time to build and maintain constituent trust. That is what House members believe. That is why they spend so much of their working time at home. Much of what I have observed in my travels can be explained as a continuous and continuing effort to win (for new members) and to maintain (for old members) the trust of their various constituencies. Most of the communication I have heard and seen is not overtly political at all. It is, rather, part of a ceaseless effort to reinforce the underpinnings of trust in the congressman or congresswoman "as a person." Viewed from this perspective, the archetypical constituent question is not "what have you done for me lately" but "how have you looked to me lately." House members, then, make a strategic calculation that helps us understand why they go home so much. Presentation of self enhances trust; enhancing trust takes time; therefore, presentation of self takes time.

Of the "contextual," "expressions given off" in the effort to win and hold constituent trust, three seem particularly ubiquitous. First, the congressman conveys to his constituents a sense of his qualification. Contextually and verbally, he gives them the impression that "I am qualified to hold the office of United States

Representative." "I understand the job and I have the experience necessary to do a good job." "I can hold my own — or better — in any competition inside the House." All members try to convey their qualifications. But it is particularly crucial that any nonincumbent convey this sense of being "qualified." For him, it is the threshold impression — without which he will not be taken seriously as a candidate for Congress. Qualification will not ensure trust, but it is at least a precondition.

Second, the congressman conveys a sense of identification with his constituents. Contextually and verbally he gives them the impression that "I am one of you." "I think the way youdo and I care about the same things you do." "You can trust me because we are like one another." The third is a sense of empathy conveyed by the congressman to his constituents. Contextually and verbally, he gives them the impression that "I understand your situation and I care about it." "I can put myself in your shoes." "You can trust me because although I am not one of you - I understand you." Qualification, identification, and empathy are all helpful in the building of constituent trust. To a large degree, these three impressions are conveyed by the very fact of regular personal contact at home. That is, "I prove to you that I am qualified," or "I prove to you that I am one of you," or "I prove to you that I understand you" by coming around frequently "to let you see me, to see you, and to meet with you." Contrariwise, "if I failed to come home to see and be seen, to talk and be talked to, then you would have some reason to worry about trusting me." Thus do decisions about the allocation of resources affect the frequency of and opportunity for the presentation of self.

Once he is home, what kind of a presentation does he make there? How does he decide what presentation to make? How does he allocate his time among his perceived constituencies? How does he present himself to these various constituencies? What proportion of competence, identification, or empathy (or other expressions) does he "give off"? In short, what kinds of home styles are presented; and how do they differ among House members? I shall work toward an answer to these questions by discussing the styles of two representatives.

Presentation of Self: A Person-to-Person Style. While it is probably true that the range of appropriate home styles in any given district is large, it is also probably true that in many geographical constituencies there are distinct limits to that range. Congressman A believes

there is a good "fit" between his kind of district and his kind of home style. He thinks of his geographical constituency as a collection of counties in a particular section of his state — as southern, rural, and conservative. And he believes that certain presentations of self would not be acceptable there. "I remember once," he told a small group at dinner before a college lecture,

when I was sitting in the House gallery with a constituent listening to Congressman Dan Flood speak on the floor. Dan is a liberal from Wilkes Barre, Pennsylvania. He is a former Shakespearian actor and his wife is a former opera singer. Dan was wearing a purple shirt and a white suit; and he was sporting his little waxed moustache. My constituent turned to me and asked 'what chance do you think a man like that would have of getting elected in our district?' And I said, 'exactly the same chance as I would have of getting elected in Wilkes Barre, Pennsylvania.'

The expressions "given off" by a former actor with a purple shirt, a white suit, and a waxed moustache would be suicidal in Congressman A's district. Indeed, two days earlier as we got out of the car in one of his county seats, Congressman A said apprehensively, "see my brown shirt? This will be the first time that these people have ever seen me in anything but a white shirt." Brown — possibly; purple — never.

Congressman A sees his geographical constituency as a homogeneous, natural community. And he thinks of himself as totally at one with that community — a microcosm of it. Three generations of his family have lived there and served as its leaders and officeholders. He himself held two elective offices within the district before running for Congress. He has been steeped in the area he represents.

I should write a book about this district—starting with the Indians. It's a very historic district and a very cohesive district—except for Omega County. Nobody knows it like I do.

One thing that ties the district together is the dominance of the textile industry and the dependence of the people of the district — employer and employee — on the textile industry... If I were hostile to the textile industry, it would be fatal. But that could never happen because I feel so close to the textile industry.

I represent a district in which my constituents and I have total mutual confidence, respect and trust -95 per cent, nearly 100 per cent.

Congressman A feels a deep sense of identification with his constituents. It is this sense of identification that he conveys — verbally and

there is a good "fit" between his kind of nonverbally — when he presents himself to district and his kind of home style. He thinks of them.

"In my state," he says, "only a person-to-person campaign will work." So, when he goes home, he "beats the bushes," and "ploughs the ground," in search of face-to-face contact with the people of his district. From county to county, town to town, up and down main street, in and out of county courthouses, through places of business, into homes and back yards, over country roads and into country stores, from early morning till late at night: ("Anyone who hears a knock on the door after 11:00 p.m. knows it's me") he "mixes and mingles" conveying the impression that he is one of them. In each encounter, he reaches (if the other person does not provide it) for some link between himself and the person he is talking with - and between that person and some other person. There is no conversation that does not involve an elaboration of an interpersonal web and of the ties that bind its members one to the other. In the forefront, always, are ties of family: Congressman A possesses an encyclopedic memory for the names and faces, dates and places of family relations, and for the life-cycle events of family - birth, marriage, moving, sickness, and death. His memory and his interest serve him equally well in finding other common ground — be it rivers, plants and trees, farms, crops and businesses, hunting, fishing and football, land. buildings and automobiles, home, church, and country. He devours the district's history; on one trip he was absorbed in a county history and genealogy, on another the memoirs of U.S. Grant. He continually files, sorts, arranges and rearranges his catalogues of linkages - personto-person, place to place, event to event, time to time.

The Congressman muses a lot about the keys to success in person to person relationships.

Do you remember Miss Sharp back in the post office? She had never met me before, but she called me Sam. That's the way people think of me. No person will ever vote against you if he's on a first name basis with you. Did you know that?

When I'm campaigning I sometimes stop in a country store and buy some salmon and crackers and share them with everyone there — and buy more if need be. Do you know that a man who eats salmon and crackers with you will vote for you? And if a man takes a bite of your chewing tobacco — or better still if he gives you a bite of his chewing tobacco — he'll not only vote for you, he'll fight for you.

People feel they can talk to me. When they are talking, they feel that I'm listening to what

they have to say. Some people have the ability to make others feel that way and some don't. They feel that if they come to me with a problem, I'll do everything I can to help them.

The expression he tries to give off in all his person to person dealings is that he knows them, that they know him "as a person," that they are all part of the same community, and that his constituents, therefore, have every reason to make favorable inferences about him. "They know me," he says, "and they trust me."

Since he perceives his geographical constituency as a group of counties, it is natural to find him conveying this sense of identification in terms of counties. In three different counties — none of them his place of residence — he verbalized his relationship with the people who lived there as follows: Chatting with a group of businessmen riding to lunch after a meeting with the officials of Alpha County on water and sewer problems, he said,

Did you know that an Alpha County man saved my grandfather's life in the Civil War? In the battle of Williamsville, my grandfather was badly wounded and Lieutenant Henry from Henryville picked him up and carried him off the field – just a bloody uniform with pieces of bone sticking out. An orderly stopped him and said, 'What are you doing carrying that corpse?' Lieutenant Henry said, 'That's no corpse; that's Captain McDonough; and so long as there's a spark of life in him, I'm going to try to save him.' He did and my grandfather lived. My roots go deep in Alpha County.

Giving an after dinner speech to the Women's Business and Professional Club of Beta County, he said,

I feel as much at home in Beta County as I do anyplace on earth. I can't begin to describe to you the frustrations I feel when I see these crazy social experiments (in Washington).... These frustrations would make me a nervous wreck or worse if I could not come back home to be with you, my friends and neighbors, my supporters and my constituents. I come home to refresh my spirit and renew my strength, here in the heart of our district, where my family's roots go deep. To me, this truly is 'holy ground.'

Speaking to a sesquicentennial celebration in adjacent Gamma County, he began,

I have never recognized the artificial boundaries that separate our two counties. I have felt as much at home in Gamma County — our county — among my friends and neighbors as if I had been born here, raised here, and lived here every day of my life.

Later that evening, he reflected on his appearance at the sesquicentennial, which he ranked as the most important event of my four-day visit. "When Marvin introduced me today and said that there weren't five people out there, out of 4000, who didn't know me, he was probably right. And those who don't know me think they do."

His repeated use of the term "at home" suggests that Congressman A perceives the people whom he meets as his primary constituency. When asked to describe "his very strongest supporters," he explained: "My strongest supporters are the people who know me and whom I have known and with whom I have communicated over the years ... in my oldest counties, that means 30-40 years." He does not perceive his primary constituency in demographic terms but in terms of personal contacts. In a district seen as homogeneous there are few benchmarks for differentiation. And the one clear benchmark - race - is one he never mentions in public and only rarely in private. His primary constituents are the rural and small town whites who know him (or feel that they know him) personally. He seems to be, as he was once introduced, "equally at home with blue denim and blue serge, with rich folks and po' folks" - so long as the blue denim is nonunion. Standing around in a dusty, brown field swapping hunting jokes with a group of blue-collar friends and sitting in an antiquefilled livingroom talking business with the president of a textile company rank equally high on his "at homeness" index. His primary constituency, as may be the case in homogeneous districts, is quite amorphous - as demographically amorphous as V. O. Key's classic "friends and neighbors" victory pattern. But it is sizeable enough and intense enough to have protected Congressman A, for a considerable number of terms, from any serious primary challenge.

Congressman A does not come home a lot, falling into our low-frequency category (0-23) of trips home. He spent eighty working days there in 1973. When he does, he spends most of his time where it is strategically profitable (and personally comfortable) — with his primary constituency. There, he reinforces his ties to the group of greatest importance to him in his traditionally one-party district. He explained, for example, why he took time out of a crowded Washington work week to fly home to the installation of officers of a Boy Scout Council.

I wanted to make it because of who they were. They were Boy Scout leaders from six of my counties — the men who make scouting here a viable movement. They have given me some of my strongest support since I have been in politics. And they have never asked anything of me but to give them good government. So when they ask, I sure don't want to pass up the opportunity to meet with 90–100 of them. I knew about 90 per cent of them. And the other 10 per cent I know now. Some of those I hadn't met were sons of men I had known.

The scout leaders, of course, want more from him than "good government." They want his time and his personal attention. And he, believing these to be the essence of his home style, happily obliges.

When he mentioned that he had also left Washington once to speak at a high school graduation, I asked whether a high paid staff assistant in the district might relieve him of some of these obligations. (Some members in my group have just such an assistant who attends meetings "in the name of the congressman" when he cannot come.) Congressman A answered,

It wouldn't work. People want to see the congressman — me. At the high school commencement exercises, I could have sent the most scholarly person I could find, to make a more erudite, comprehensive and scholarly exposition than I made. If I had done so, the people there wouldn't have enjoyed one bit of anything he said. And they would never have foreiven me for not being there.

He has a small district staff — three people, one full-time office, and one-half time office — and when he is home, he is as apt to pick up someone's personal problems and jot them down on the back of an envelope as he tours around as he is to find out about these problems from his district aides. Congressman A, at home, is a virtual one-man band. His home style is one of the hardest to delegate to others; and he has no inclination to do so.

He allocates relatively little of his time to his larger re-election constituency. Omega County, singled out earlier as out of the district's mold, is not rural, is populated heavily by out-of-staters, and has experienced rapid population growth. Congressman A admits he does not feel "at home" there. Yet he still gets a sizeable percentage of Omega County's votes — on grounds of party identification. He explained why he didn't spend time among these re-election constituents.

It is so heterogeneous, disorganized, and full of factions.... I don't spend very much time there. Some of my good friends criticize me and say I neglect it unduly. And they have a point. But I can get 50 per cent of the vote without campaigning there at all; and I couldn't

get more than 75 per cent if I campaigned there all the time. If I did that, I would probably lose more votes than I gained, because I would become identified with one of the factions, and half the people would hate me. On top of that, I would lose a lot of my support elsewhere in the district by neglecting it. It's just not worth it.

There is another reason besides time costs and political benefits. It is that Congressman A's home style is totally inappropriate for Omega County, and he avoids the personal unpleasantness that would be involved in trying to campaign there. Strategically, Congressman A will accept any increment of support he can get beyond his primary constituency. ("The black people who know me know that I will help them with their problems.") But he allocates very little of his time to the effort.

Congressman A's presentation of self places very little emphasis on articulating issues. The Congressman's own abilities and inclinations run to cultivating personal, face-to-face relationships with individuals. The greater the social, psychological, and physical distance between himself and others, the less he is at home, regardless of the situation. And he was clearly least "at home" at a college, in a lecture-plus-question-and-answer format. He accepts invitations of this sort to discuss issues. But he does nothing to generate such engagements; nor does he go out of his way to raise issues in his dealings with others at home. On the single occasion when he broke this pattern, he tested out his potentially controversial position with his primary constituents (i.e., the American Legion post in his home town), found it to be acceptable, and articulated it often thereafter. Congressman A's home style does, however, take place within an issue context. There is widespread agreement in the district, and very strong agreement within his primary constituency, on the major issues of race, foreign aid, government spending and social conservatism. The district voted for George Wallace in 1968. Thus while Congressman A's home style is apparently issueless, it may depend for its very success on an underlying issue consensus.

There are, therefore, strategic reasons as well as personal reasons for Congressman A not to focus heavily on specific issues. To do so would be unnecessary and potentially divisive. Congressman A is protective of his existing constituency relations and will not want to risk alienating any of his support by introducing or escalating controversy of any kind. He is a stabilizer, a maintainer. And so, when asked to speak formally, he often responds with com-

munitarian homilies. "I believe if ever there was a promised land, that land is America; and if ever there was a chosen people, those people are Americans." "If a man isn't proud of his heritage, he won't leave a heritage to be proud of. And that goes for his family, his community and his country." These utterances are not the secret of his success. But they do testify, again, to his continuing efforts to articulate a sense of community, to construct and reconstruct a web of enduring personal relationships and to present himself as totally a part of that web. If he gets into electoral difficulty, Congressman A will resort not to a discussion of "the issues," but to an increased reliance on his person-toperson home style. And, so long as his strategic perceptions are accurate, he will remain a congressman.

Presentation of Self: An Issue-Oriented Style. If Congressman B's geographical constituency places any constraints on an appropriate home style, he is not very aware of them. He sees his district as heterogeneous.

It is three worlds; three very different worlds.... It has a city — which is an urban disaster. It has suburbs — the fastest growing part of the district.... It has a rural area which is a place unto itself.

We spent all afternoon talking to the Teamsters in the city; and then we went to a cocktail party in a wealthy suburb. That's the kind of culture shock I get all the time in this district — bam! bam! bam!

The "three worlds" are not just different. They are also socially and psychologically separated from one another.

Actually the people in the three worlds don't know the others are even in the district. They are three separate worlds. In the city, they call it the city district; in the rural area, they call it their district. And both of them are shocked when they are told that they each make up only one-quarter of the district.

The other half are the suburbs — which are, themselves very disparate. A few suburbs are linked to the city; most are not. Some are blue collar; others are affluent. Some are WASP; others are ethnic. The district is, then, perceived not only as diverse and artificial, but as segmented as well. The possibilities for an acceptable presentation of self would seem to be limitless.

Congressman B's past associations in the district do not incline him toward a style peculiar to any one of "the three worlds." His district ties are not deep; he is a young man who went to college, worked and got his

political feet wet outside his district and his state. Nor are the ties strong; he grew up in a suburb in which he probably feels less "at home" ("We lost that stupid, friggin' town by 1000 votes last time.") than anywhere in the district. When he first thought about running. he knew nothing about the district. "I can remember sitting in the livingroom here, in 1963, looking at the map of the district, and saying to myself, 'X? Y? I didn't know there was a town called X in this district. Is there a town called Y?' I didn't know anything about the district." Furthermore, he didn't know any people there. "We started completely from scratch. I was about as little known in the district as anyone could be. In the city, I knew exactly two people. In the largest suburb, I didn't know a single person." He has (unlike Congressman A) absolutely no sense that "only a person like myself" can win in his district. Indeed, he thinks the opponent he first defeated was better suited to the district and should have won the election. "If I were he, I'd have beaten me." In terms of a geographical constituency and an individual's immersion in it, it is hard to imagine two more different perceptions of me-in-the-constituency than those of Congressman A and Congressman B.

Congressman B has not been in office very long. Not only did he begin from scratch, but he has been scratching ever since. He lost his first race for Congress; he succeeded in his second; and he now represents an objectively (and subjectively) marginal district. His entire career has been spent reaching out for political support. As he has gone about identifying and building first a primary and then a re-election constituency, he has simultaneously been evolving a political "self" and methods of presenting that "self" to them.

His earliest campaign promises were promises about the allocation of resources. He pledged to return to the district every week and to open three district offices, one in each of the "three worlds." These commitments about home style were contextually appropriate, if not contextually determined. For a candidate who neither knew nor was known in the district, pledges of attentiveness would seem almost mandatory. Furthermore, they allowed him to differentiate his proposed style from that of the incumbent - who was not very visible in the district and who operated one office there staffed by two people. Also, these pledges allowed him to embroider his belief that "a sense of distance has developed between the people and the government," necessitating efforts to "humanize" the relationship. And finally, his pledges gave him a lowest common

denominator appeal based on style to a district with palpably diverse substantive interests. In 1973, Congressman B made thirty trips home, spent 109 working days there, operated three district offices and assigned one-half of his total staff of fourteen to the district. Promises have turned into style. "We have given the impression of being hardworking — of having a magic carpet, of being all over the place. It's been backbreaking, but it's the impression of being accessible."

Congressman B's actual presentation of self, i.e., what he does when he goes home, has evolved out of his personal interests and talents. He was propelled into active politics by his opposition to the Vietnam War. And his political impulses have been strongly issue-based ever since. He is severely critical of most of what has gone on in American public life for the last ten years. And he espouses a series of programmatic remedies - mostly governmental - for our social ills. He is contemptuous of "old line" politicians who are uninterested in issues and who campaign "by putting on their straw hats and going to barbeques." Riding to a meeting at which he was to address one of his aging town committees, he shouted, "We don't want any old pols or town committees. Give me housewives who have never been in politics before." Whereupon, he rehearsed the opening lines of "the speech I'd like to give" to the town committee. "It will be a stirring speech. 'My fellow political hacks. We are gathered together to find every possible way to avoid talking about the issues." This comment, together with his running mimicry of the "old pols," exemplifies what Goffman calls a performance in the "back region," i.e., behind the scenes where the individual's behavior is sharply differentiated from, and serves to accent, his presentation of self to the audience in the "front region."26

Congressman B presents himself in the "front region," i.e., in public, as a practitioner of an open, issue-based, and participatory politics. It was his antiwar stand particularly, and his issue-orientation generally that attracted the largest element of his primary constituency. These were the antiwar activists — young housewives, graduate students, and professionals — who created, staffed, and manned the large volunteer organization that became his political backbone. In the end, his volunteers

became skilled in the campaign arts - organizing, coordinating, polling, canvassing, targeting, mailing, fund raising, scheduling, advancing, leafleting - even "bumper-stickering." "We organized and ran a campaign the likes of which people in this district had never seen. Neither party had done anything like it." Lacking a natural community to tie into and lacking any widespread personal appeal (or basis for such), Congressman B turned to the only alternative basis for building support - an organization. The "strongest supporters" in his organization did not support him because they knew him or had had any previous connection with him. The bond was agreement on the central issues and on the importance of emphasizing the issues. That agreement was the only "qualification" for the office that mattered to them. Within this group, the sense of identification between candidate and supporters was nearly total. He was "one of them." They trusted him. And they, with some trade union help (especially financial), gave him a victory in his initial primary.

In reaching for broader electoral support, Congressman B has been guided, in addition to his commitment to "the issues," by a personal penchant for talking about them. That is, he is an exceptionally verbal person; and he has evolved a suitably verbal home style. He places special emphasis on articulating, explaining, discussing, and debating issues. In each campaign (whether he be challenger or incumbent) he has pressed for debates with his opponent; and his assessment of his opponents focuses on their issue positions and their verbal facility. ("He's very conservative and, I understand, more articulate than the last guy. I felt sorry for him; he was so slow.")

In his first two campaigns the main vehicle for presenting himself to his prospective election-re-election constituency was "the coffee." He would sit in a living room or a yard, morning, afternoon, and evening (sometimes as often as eight or ten times each day) with one or two dozen people, stating his issue positions, answering their questions, and engaging in give and take. At the verbal level, the subject was substantive problems. But Congressman B knew that expressions "given off" were equally important.

People don't make up their minds on the basis of reading all our position papers. We have twenty-six of them, because some people are interested. But most people get a gut feeling about the kind of human being they want to represent them.

Thus, his display of substantive knowledge and his mental agility at "the coffees" would help

²⁶Goffman, Chapter 3, especially p. 128. Often comments made in the "back regions" provide clues for "at homeness" estimates. "We're having breakfast tomorrow with a businessman's group. They're really a bunch of hoodlums."

convey the impression that "as a human being" he was qualified for the office. And, not relying wholly on these expressions given off, he would remind his listeners, "No congressman can represent his people unless he's quick on his feet, because you have to deal with 434 other people - each of whom got there by being quick on his feet." Coffees were by no means the only way Congressman B presented himself. But it was his preferred method. "The coffees are a spectacular success. They are at the heart of the campaign." Strategically, they were particularly successful in the suburban swing area of the district. But he tried them everywhere - even in the city, where they were probably least appropriate.

Once in office, he evolved a natural extension of the campaign coffee - a new vehicle which allowed him to emphasize, still, his accessibility, his openness and his commitment to rational dialogue. It is "the open meeting," held twice a year, in every city and town in the district - nearly 200 in each session of Congress. Each postal patron gets an invitation to "come and 'have at' your congressman." And, before groups of 4 to 300, in town halls, schools, and community centers, he articulates the issues in a question and answer format. The exchanges are informative and wide-ranging; they are punctuated with enthusiasm and wit. The open meetings, like the coffees, allow Congressman B to play to his personal strengths - his issue interests and his verbal agility. In the coffees, he was concerned with conveying threshold impressions of qualification, and his knowledge and status reinforce that impression in the open meetings. But in the open meetings, he is reaching for some deeper underpinnings of constituent trust. He does this with a presentation of self that combines identification and empathy. "I am not exactly one of you," he seems to tell them, "but we have a lot in common, and I feel a lot like you do." He expresses this feeling in two ways.

One expression "given" and "given off" in the open meetings is the sense that the giveand-take format requires a special kind of congressman and a special kind of constituency and involves them, therefore, in a special kind of relationship. In each meeting I attended, his opening remarks included two such expressions.

One of the first pieces of advice I got from a senior member of my party was: 'Send out lots of newsletters, but don't mention any issues. The next thing you know, they'll want to know how you vote.' Well, I don't believe that.

My colleagues in Congress told me that the questionnaires I sent you were too long and too complicated and that you would never answer

it. Well, 5000 have been filled out and returned already — before we've even sent them all out.

At the same time that he exhibits his own ability to tackle any question, explain any vote, and debate any difference of opinion, he massages the egos of his constituents by indicating how intelligent, aware, and concerned they are to engage with him in this new, open, rational style of politics. At the conclusion of an emotional debate with a group of right-tolifers, whose views he steadfastly opposed, he summed up: "I don't want to pat myself on the back, but there aren't too many congressmen who would do what I am doing here today. Most of them dig a hole and crawl in. I respect your opinions and I hope you will respect mine." The "pat on the back" is for them as well as him. And the expression "given off" is that of a special stylistic relationship. From that relationship, he hopes, will flow an increasing measure of constituent trust.

A second, related, expression "given off" is the sense that Congressman B, though he is a politician, is more like his constituents than he is like other politicians. It is not easy for him to convey such an impression, because the only thing his potential re-election constituents know about him is that he is a politician. They do not know him from any prior involvement in a community life. So he works very hard to bind himself to his constituents by disassociating himself from "the government" and disavowing his politician's status. He presents himself as an antipolitician, giving off the feeling that, "I'm just as fed up with government and the people who run it as you are." Since he is a congressman-politician, he is unrelentingly harsh in his criticism of Congress and his fellow legislators.

As you know, I'm one of the greatest critics of Congress. It's an outrageous and outmoded institution.

All Congress has ever done since I've been in Congress is pass the buck to the president and then blame him for what goes wrong.... Congress is gutless beyond my power to describe to you.

Most members of Congress think that most people are clods... Most of the guys down there are out of touch with their districts.... We aren't living in the 1930s anymore. Of course some members of Congress are.... I could never understand the lack of congressional sensitivity to the problems of the elderly. There are so many of them there.

A politician seeking to convey the impression that he is not a politician, Congressman B hopes to build constituent trust by inviting them to blend their cynicism with his.

The presentation of self - an accessible, issue-oriented, communicative antipolitician at the open meetings is a lowest common denominator presentation. It can win support in each of "the three worlds" without losing support in any. For it is the style, not the issue content that counts most in the re-election constituency. Congressman B is completely comfortable in the setting. "That was fun," says after each open meeting. And, oc-casionally, "it's more fun when there's some hostility." But it is the format more than the audience that makes him feel really "at home." He is not a person-to-person campaigner. "Two of my friends in Congress hold office hours and see people one at a time. That would be a horribly inefficient use of my time. I can see fifty at once. Besides, they don't want to get involved in a give and take." He, on the other hand, keeps his distance from the personal problems of his constituents, inviting them to talk with the staff members who accompany him to the open meetings. Of course, he meets people face-to-face - all the time. But he does not know or seek out much about them as individuals, not much that would build anything more than a strictly political connection. An aptitude for names and faces, a facility with tidbits of personal information and small talk, an easy informality in face-to-face relations these are not his natural personal strengths. But they are not the keys to his success with his re-election constituency. He has evolved a home style that does not call for person-to-person abilities in large supply.

The open meetings remain the centerpiece of his home style. "They are the most extraordinary thing we've ever done, and the most important." He sees them as vehicles which help him reach out to and expand his re-election constituency. For he remains a builder instead of a stabilizer in his constituency relations.

Politically, these open meetings are pure gold. Fifty may come, but everybody in town gets an invitation.... I do know that none of our loyalists come to the meetings. They know the meetings are nonpartisan. Maybe one or two of them will show up, but mostly they are new faces.

They have given him entree into the least supportive, rural areas of his district, where he recruits support and neutralizes the more intense opposition. At first, he says, "in some of these towns they didn't know what to say to a Democrat. They probably hadn't met one except for people who fixed their toilets." Yet at the open meetings, "we've had better turnouts, proportionately, in the rural area." "And

we get a lot of letters from people there who say they disagree with us but respect our honesty and independence." In time — but only in time — interest and respect may turn into the supportive inferences that connote trust.

But as Congressman B spends more and more of his time at home cultivating an expanding re-election constituency, his oldest and strongest supporters have felt neglected. So Congressman B has a more complex strategic problem, in terms of allocating his time, than Congressman A.

When we began, we had the true believers working their hearts out. It was just like a family. But the more you gain in voters, and the more you broaden your constituency, the more the family feels hurt. Our true believers keep asking me, 'Why don't you drink with us?' 'Why don't you talk to me personally anymore?' I have to keep talking to them about the need to build a larger majority. I have to keep telling them that politics is not exclusive; it is inclusive. It is not something that can be done in the living room.

The true believers are not threatening a total loss of support; but declining enthusiasm would present a serious support problem. One way Congressman B may deal with the problem is to come home more, so that he can give the necessary time to the true believers. He does come home more than Congressman A, perhaps partly because his strategic problems at home require it. Still, Congressman B emphasizes identifying and building support beyond the primary constituency in "the three worlds." And he finds the open meetings the most effective (and most comfortable) vehicle for him. "What more could anyone ask," he says, "than to have the congressman come to their town personally?" His primary constituents do ask something more. And, so long as he gives it to them, he will remain a congressman.

Presentation of Self: Constituency Constraints and Constituency Careers. Our description of the person-to-person and the issue-oriented styles is exemplary, not exhaustive.²⁷ Speculatively, however, presentation of self would

27The two styles are the most common among my group. They may be the most common among all representatives. In his study of representation, Paul Peterson uses two very similar analytical categories, "particularistic representation" (person-to-person) and "universalistic representation" (issue-oriented) Paul Peterson, "Forms of Representation: Participation of the Poor in the Community Action Program," American Political Science Review, 64 (June, 1970), 491-501. Other styles will be elaborated in a later, lengthier study, of which this paper is a part.

seem to be explainable by three kinds of factors – contextual, personal, and strategic.

Contextually, a representative thinks about his constituency relations in terms of me-inthe-constituency. That perception predates his service in Washington and cannot be understood by drawing inferences from his Washington behavior. Part of the content of that perception involves a sense of fit - a good fit as in the case of Congressman A, a nonfit as in the case of Congressman B, and a bad fit as in the case of one congressman (not in my group) who refers to his district as "outer Mongolia." A congressman's sense of fit will, in turn, be affected by whether he sees the district as homogeneous or heterogeneous. Good fits are more likely in homogeneous districts. But the reverse side of the coin is that home styles are more likely to be imposed upon the congressman in homogeneous districts. If Congressman A did not represent his district, someone who performed similarly at home probably would. In a heterogeneous district - Congressman B's case - home style is much more a matter of individual choice, and is more likely to be imposed by the congressman on his district. Thus, upon further analysis of the state-by-state resource allocation data, we would expect to find the most idiosyncratic patterns appearing in the most heterogeneous districts. Homogeneous districts, in sum, impose more stylistic constraints on a congressman than do heterogeneous districts.

A second contextual impact on the presentation of self, however, may produce contrary tendencies. Once a congressman has imposed a particular presentational style upon his district, his successors may feel constrained to continue that style. That is, a congressman's home style may be influenced by the previous congressman's home style. Congressman B deliberately chose a style that contrasted with his predecessor's in order to help develop an identifiable political self. It is equally plausible (and it happened in my group) that expectations about style could be so strongly implanted in the district by a predecessor, that the new congressman dare not change. Similarly, a choice of home style by imitation or by contrast can occur with reference to a neighboring congressman - if the congressman choosing a style has reason to believe that some of his constituents are likely to compare him to that neighbor. Regardless of district make-up, then, under certain conditions one congressman's style may be shaped by the style of another congressman, past or present.

From the cases of representatives A and B, it seems clear that the presentation of self is

also shaped by each individual's inclinations and talents. Every congressman has some latitude in deciding how to present himself "as a person." That is not to say that House members like to do all the things they do at home. More members than I would have expected described themselves as "shy," "reserved," or "not an extrovert." But they go home and present themselves anyway. And they try to do what they are most comfortable doing and try not to do that which they are most uncomfortable doing. Congressman A seeks out person-to-person relationships, but does not encourage issueoriented meetings. Congressman B seeks out issue-oriented meetings, but does not encourage person-to-person relationships. Experience, interest, abilities - all the personal attributes of a congressman's self - help shape his presentation of that self to others.

Strategically, each congressman must decide how he will allocate his time when he is at home. And it is of some help to think of this strategic problem in terms of his perceived constituencies. From our two cases, we might generalize that a man in a homogeneous district will spend most of his time with his primary constituency. Homogeneous districts are most likely to be perceived as protected in the general election, so that the strategic problem is to hold sufficient primary constituency support to ward off a primary challenger. By the same token, the primary constituency in a homogeneous district is probably more amorphous and less easily defined than it is in a heterogeneous district. Thus a concentration of effort in the primary constituency does not mean that any less time will be required to cultivate it.

By contrast, in a district perceived to be both heterogeneous and electorally unprotected, the congressman will spend relatively more time in his re-election constituency. But he faces a problem of balance. He will play "the politics of inclusion" by spending time expanding his re-election constituency, partly on the assumption that his strongest supporters have no inclination to go elsewhere. Yet he cannot neglect the primary constituency unduly, since their loyalty and intensity of commitment are necessary to sustain a predictably difficult election campaign. He may, of course, be able to allocate resources other than his time, i.e., votes, to keep his primary constituency content. 28 But there is every evidence from my

²⁸See Morris Fiorina, "Electoral Margins, Constituency Influence and Policy Moderation: A Critical Assessment," *American Politics Quarterly*, 1 (October, 1973), 479–498.

experience that the congressman's strongest supporters are more — not less — demanding of the congressman's time than his other constituencies.

A strategic problem in allocating time, alluded to briefly in discussing Congressman B. involves the presentation of self to one's strongest opponents - to the people each congressman believes he "will never get." House members handle the problem differently. But most of them will accept (and some will solicit) opportunities to present themselves before unfriendly constituents. The strategic hope is that displaying themselves "as a person" may reduce the intensity of the opposition, thus neutralizing their effect within the district. Intense opposition is what every congressman wants least of. Any time spent cooling the ardor of the opposition is time usefully spent, for it may mean less intense support for the challenger. Functionally, the same accents used in presenting one's self to supporters apply to a presentation to opponents - the emphasis on qualification, the effort at identification, the projection of empathy. In other words, the process of allaying hostility differs little from the process of building trust. That makes it easier for House members to allocate some time - probably minor - to a strategy of neutralization.

Students of Congress are accustomed to thinking about a congressman's career in the House - his early adjustments, his rise in seniority, his placement on the ladders of committee and party, the accumulation of his responsibilities, the fluctuations of his personal "Dow Jones Average." But House members also pursue a career in the constituency. Congressman B's evolution from "scratch" to a concern with his primary constituency to a concern with his re-election constituency gives evidence of such a constituency career. He was as much a newcomer in the district as newcomers are (or were) purported to be in the House. He had to work out an appropriate home style there just the way each new House member adapts to the House as an institution. Congressman B has been, and is, in the expansionist phase of his career, continually reaching out for increments of support. Congressman A, by comparison, is in a more protectionist phase of his constituency career. He believes that he has, over a considerable period of time, won the trust of his constituents (i.e., his primary constituents). He is working mainly to reinforce that trust; to protect the support he already has. Congressman B does not talk about constituent trust; he never says, "my constituents trust me." His presentation of self is designed to build trust, but, as we have said, it takes time.

The idea of a career in the constituency helps to highlight an important fact about the congressman as an elective politician. As any textbook treatment of incumbency tells us, the congressman is a particularly long-lived political species. He has been making or will make presentations of self to his constituents for a long time. And they have been looking at or will look at him "as a person" for a long time. Relative to politicians with briefer constituency careers - like presidents - a congressman's political support will depend especially heavily upon his presentation of self. That, of course, is precisely what House members themselves tell us whenever we have asked. They tell us that their "personal record and standing" or their "personalities" are more important in explaining their election than "issues" or "party identification."29 They tell us, in other words, that their home style - especially their presentation of self - is the most important determinant of their political support. The idea of a lengthy constituency career helps us understand why this might be true. For it makes home style into a durable, consistent long term factor in congressional electoral politics. In any congressional electoral analyses patterned after our presidential electoral analyses, home style may have to be elevated to a scholarly status heretofore reserved only for party identification and issue voting.30

29See Stokes and Miller, "Party Government," p. 542; and Charles S. Bullock, III, "Candidate Perceptions of Causes of Election Outcome," paper delivered at American Political Science Association Convention, New Orleans, 1973, Tables 7, 8. In his analysis of ten congressional campaigns in 1962, David Leuthold found that "probably more than half the appeals (of the candidates) . . . were based on the qualities of the candidate or his opponent." Leuthold, Electioneering in a Democracy, p. 113. He believes that voters are looking for ability, concern, and similarity to themselves. That — in the somewhat different words, qualification, empathy, and identification — is what my representatives think the voters want. See Leuthold, pp. 23–24.

30 The first people to view congressional incumbency, in the light of SRC analyses, as a long-term force were Robert Arseneau and Raymond Wolfinger, "Voting Behavior in Congressional Elections," paper delivered at American Political Science Association Convention, New Orleans, 1973. Observations consistent with those here, made in different contexts are David Mayhew's emphasis on "the expected incumbent differential" and Charles Jones's conclusion that House campaigns are less likely to be "issue-oriented" than "image-oriented" and "issue-involved." Home style contributes to the congressman's "incumbent differential" and to his "image." Mayhew, The Electoral Connection; Jones, "The Role of the Campaign in Congressional Politics." From an election analysis perspective Walter Dean Burnham has recently emphasized the "office specific" nature of elections and the need to develop ways of looking at congressional

Home Style: Explanation of Washington Activity

When members of Congress are at home, they do something that is closely allied with, yet separable from, the presentation of self to their constituencies. They explain what they have done in Washington. For some House members, their Washington activities are central to their presentation of self. One congressman, for example, routinely began every speech before every district group as follows:

I have represented this district for the last twenty years. And I come to you to ask for a two-year renewal of my contract. I'm running because I have a twenty-year investment in my job and because I think you, as my constituents, have an investment in my seniority. In a body as large as the House of Representatives with 435 elected, coequal members, there has to be a structure if we're ever going to get anything done. And it takes a long time to learn that structure, to learn who has the power and to learn where to grease the skids to get something done. I think I know the structure and the people in the House better than any newcomer could. And I believe I can accomplish things for you that no newcomer could.

He wants his constituents to see him "as a person" in terms of his importance in Washington. By contrast, neither Congressman A nor Congressman B makes his Washington activity central to his presentation of self. But whether or not his behavior in Washington is central to his presentation of self, every House member spends some time at home explaining and justifying his Washington behavior to his various constituencies. He tells them what he has done and why. What he says, how he says it, and to whom can be viewed as a distinctive aspect of his home style.

The objective of every congressman's explanations — our usage of explanation incorporates the idea of justification as well — is political support. And just as a congressman chooses, subject to constraints, a presentational style, so too does he choose, subject to constraints, an explanatory style. When most people think of explaining what goes on in Washington to constituents, they think of explaining votes. But we should conceptualize the activities subject to explanation more broadly than that. A House member will explain any part of his

elections that are not simply imitative of what we have done for presidential elections. Walter Dean Burnham, "Insulation and Responsiveness in Congressional Elections," *Political Science Quarterly*, 90 (Fall, 1975), 411–435.

activity in Washington if he thinks that part of his activity is relevant to the winning and holding of support at home. Just what kinds of behavior he thinks his various constituencies want or need to have explained to them is an empirical matter; but one which bulks especially large among my representatives is their effectiveness (or lack of it) inside the House on behalf of their constituencies. Often this explanation of one's internal influence also entails a more general explanation of the workings of Congress.

In the case of our Congressman A, for example, press reports that he had lost a committee assignment of importance to the district because of his lack of power within the House, posed a major explanatory problem.

Nothing is more damaging to a congressman in his district than to have his constituents believe that he doesn't have the power to get something he wants of that nature.... It might have been the only issue in the next campaign.... That would have been all that was needed—and only that—to defeat me.... No one in the world would believe my explanation, so I had to try for the next vacancy on the committee and I had to win. [Which he did, before the next election.]

For Congressman A, that is, explaining his internal House influence (or lack of it) might be more crucial to protecting his home support than explaining his votes. "I worked my head off to get that building," he said, as we drove by a new federal building in one of his county seats. "The people here were fixing to run someone against me if I hadn't produced it. People think you just have to wave a magic wand to get an appropriation, when most of it is just standing in line waiting your turn." Obviously, such an explanation of "how Congress really works" would not have satisfied his constituents in that county. So he had to produce.

The range of possible activities requiring a home explanation extends well beyond voting. Still, voting is the Washington activity we most easily recognize; and we can make most of our comments in that context. From John Kingdon's splendid discussion of "explaining," we know that, at the time they decide to vote, House members are very aware that they may be called upon to explain their vote to some of their constituents. Moreover, says Kingdon, the anticipated need to explain influences their decision on how to vote. They may cast a certain vote only if and when they are con-

³¹Kingdon, Congressmen's Voting Decisions, pp. 46-53.

vinced that they have a satisfactory explanation hard to see him adopting any other explanatory in hand. Or they may cast a certain vote because it is the vote least likely to require an explanation. Kingdon is interested in finding out why members of Congress vote the way they do. But along the way, he helps make the case for finding out why members of Congress explain the way they do. For, if the anticipated need to explain has the effect on voting that Kingdon suggests - i.e., if it makes voting more complicated and more difficult than it otherwise would be - then the act of explaining must be as problematical for House members as the act of voting. House members believe that they can win and lose constituent support through their explanations as well as through their votes. To them, therefore, voting and explaining are interrelated aspects of a single strategic problem. If that is the way House members see it, then it might be useful for political scientists to look at it that way, too and to spend a little less of our time explaining votes and a little more of our time explaining explanations.

Members are, of course, called upon to vote much more often than they are called upon to explain. That is, they are never called upon to explain all their votes. Their uncertainty about which votes they will have to explain, however, leads them to prepare explanations for more votes than they need to, the need being enforced on them by dissatisfied constituents and, primarily, by the electoral challenger. The challenger, particularly, controls the explanatory agenda - or, better, tries to do so. All the uncertainty that the challenger produces for the perception of constituency, the challenger also brings to the problem of explanation.

Representatives will strike different postures regarding the need to explain. Some will explain their votes only when they feel hard pressed by constituents and/or challenger to do so. They will follow the congressional adage that "if you have to explain, you're in trouble." And their explanatory practices, if not their voting practices, will be calculated to keep them out of trouble. Other members bend over backward to explain every vote that any constituent might construe as controversial sometimes well in advance of the vote. It is our hunch that the more issue-oriented a congressman's presentation of self, the more voluminous will be his explanations. Our Congressman B, for example, produces a heavy volume of explanation. "We have explained every difficult vote. Anyone who gives a twit about how I vote has the opportunity to know it. We have explained our votes on all the toughies." Given his presentational style, it is

style. In both cases, the content of what he says is less important than the fact that he says it i.e., than his style.

I shall resist the temptation to spell out all the possible relationships between the presentational and explanatory aspects of home style. But it seems obvious that they exist. We would, at the least, expect to find a "strain toward compatibility" operating between the two. And we would expect to find the presentation of self – as the centerpiece of home style – to be the more controlling aspect in the relationship. We would further expect both aspects of home style to be influenced by the same constituency constraints. For example, we would expect the broadest perception of me-in-the-constituency - the sense of fit the congressman has with his various constituencies - to underlie the choice of explanatory styles, just as it underlies the choice of presentational styles. A good illustration of this latter point lies outside my particular group of House members. It lies, instead, in the explanation of their historic impeachment vote by two members of the House Judiciary Committee.

In the face of extraordinary constituency interest, two Judiciary Committee Republicans took to statewide television and radio to explain their upcoming vote in favor of impeaching their own party's president. Representative Lawrence Hogan of Maryland cast his explanation heavily in terms of voting his individual conscience against the acknowledged wishes of many prospective constituencies primary and re-election - in his announced try for the Maryland governorship. To accent his act of conscience, he acknowledged the grave risks to his career.

I know that many of my friends, in and out of Congress, will be very displeased with me. I know that some of my financial contributors (who have staunchly supported Richard Nixon and me) will no longer support me. I know that some of my long-time campaign workers will no longer campaign for me. But to those who were my campaign workers back in my first campaign, I want to remind you of something. Remember, I was running for Congress as a Republican in an area that was registered 3-1 Democratic, and in an effort to convince Democrats that they should vote for me, a Republican, I quoted John F. Kennedy who said: 'Sometimes party loyalty demands too much.' Remember that?

Well, those words have been coming back to haunt me in recent weeks. Clearly, this is an occasion when 'party loyalty demands too much.' To base this decision on politics would not only violate my own conscience, but would

also be a breach of my own oath of office to uphold the Constitution of the United States. This vote may result not only in defeat in my campaign for governor of Maryland, but may end my future political career. But that pales into insignificance when weighted against my historic duty to vote as my conscience dictates.³²

Representative William Cohen cast his explanation in terms of voting as the people of his state of Maine would vote if they were in his shoes. He did not say they were pressuring him to vote, but that he and they, as members of the same community, thought alike on fundamental matters.

I have tried to put all of these events into the context of a political system that I know well, that of the state of Maine. I have asked myself some questions.

What if the governor of Maine ordered his aides to keep a list of those people who supported his opponents? What if he tried to have the state treasurer's department conduct audits of those who voiced dissent? What if he ordered that state police to investigate those who were critical of his policies or speeches? What if he asked aides to lie before legislative committees and judicial bodies? What if he approved of burglaries in order to smear and destroy a man's credibility? What if he obtained information that was to be presented to a grand jury for the purpose of helping his advisors design a strategy for defense?

What would the people of Maine say? You and I both know that the people of Maine would not stand for such a situation, for it is inconsistent with our principles and our constitutional system of government.³³

Hogan and Cohen offered their listeners very different kinds of explanations for the same vote. It is our hunch that underlying the difference in their explanatory styles are different perceptions of me-in-the-constituency. Cohen sees himself, we would guess, as part of a fairly homogeneous re-election-plus-primary constituency. He identifies strongly with it; and he has a confortable sense of fit with this broad constituency. That being so, he would neither perceive nor explain his vote in terms that set his conscience against theirs. His explanation was, therefore, communitarian. "I am one of you, and the issue is our conscience as a community." Hogan, we would guess, perceives his geographical constituency to be heterogeneous, and has no strong sense of fit with any large or differentiable element of it. Almost surely he feels uncertainty about his prospective statewide constituency; but it seems that he feels uncertainty, even, about his relationships with the primary constituency in his district. It is hard, therefore, for him to find any communitarian bases for explanation. His explanation was individualistic. "I am following my conscience and disregarding the opinions of my constituents."

Neither explanation is inherently "better" than the other. Each had an element of strategic calculation to it. That is, each man chose his words deliberately; and, presumably, neither wanted to lose the next election by virtue of his explanation. That being the case, the acceptability of the two explanations by the Maryland and Maine constituencies becomes a question of some moment. We lack sufficient information to assess the actual impact of the two explanations on each man's political support. But we can try a more general answer. To the degree that Maryland voters already perceived Hogan to be "a man of conscience," and to the degree that Maine voters already perceived Cohen to be "one of us," their explanations probably would have been accepted. On the other hand, to the degree that Maryland voters had already come to view Hogan as a self-serving opportunist, and to the degree that Maine voters had already come to view Cohen as a spineless follower of the herd, their explanations probably would not have been accepted. That is, the credibility of any given explanation probably depends less on the content of the explanation itself than on its compatibility with some previously established perception of the explainer "as a person." "Issues" said one member, "are not as important as the treatment of issues."

Their reasoning goes something like this: There probably are, in every district, one or two issues on which the congressman is constrained in his voting by the views of his re-election constituency. (Whether he feels constrained — which depends on whether or not he agrees with those constituent views - is beside the point.) But on the vast majority of votes, a congressman can do as he wishes - provided only that he can, if and when he needs to, explain his vote to the satisfaction of interested constituents. The ability to get his explanations accepted at home is, then, the essential underpinning of his voting leeway in Washington. Thus the question arises, how can the congressman act so as to increase the likelihood that his explanations will be accepted at home. And the answer the House members give is: he can win and hold constituent trust. The more your

³²Congressional Record, Daily Edition, July 23, 1974, H6962-H6963.

³³Congressional Record, Daily Edition, August 1, 1974, E5209-E5210.

various constituencies trust you, House members reason, the less likely they are to require an explanation of your votes and the more likely they are to accept your explanation when they do require it. The winning of trust, we have said earlier, depends largely on the presentation of self. Presentation of self, then, not only helps win votes at election time; it also makes voting in Washington easier. So Congressmen make a strategic calculation. Presentation of self enhances trust; trust enhances the acceptability of explanations; the acceptability of explanations enhances voting leeway; therefore, presentation of self enhances voting leeway.

When I asked Congressman C if he wasn't more liberal than his district, he said:

Hell, yes, but don't quote me on that. It's the biggest part of my problem — to keep people from thinking I'm a radical liberal. How do you explain to a group of Polish Catholics why you voted to abolish the House Internal Security Committee or why you voted against a bill to keep Jane Fonda from going to North Vietnam? How do you explain that? You can't.

When queried, later, on a TV interview to comment on his opponent's charge that he was "ten times more radical than George McGovern," Congressman C answered in terms of identification and trust. He said simply, "if he means by that that I'm some kind of wild-eyed radical, people around here know me better than that." Later still, he mused out loud about how he managed this problem.

It's a weird thing how you get a district to the point where you can vote the way you want to without getting scalped for doing it. I guess you do it in two ways. You come back here a lot and let people see you, so they get a feel for you. And, secondly, I go out of my way to disagree with people on specific issues. That way, they know you aren't trying to snow them. And when you vote against their views, they'll say, 'Well, he's got his reasons.' They'll trust you. I think that's it. If they trust you, you can vote the way you want to and it won't hurt.

A pair of examples from two different types of districts will illustrate variations on this theme.

Congressman D perceives his urban, predominantly black district to be homogeneous. His re-election constituency is "the whole black community"; and his primary constituency consists of the civic-minded, middle-class activists who have organized black politics in his city. As tends to be the case in homogeneous districts, the line between his re-election and his primary constituencies is not sharp. Congressman D, who was born, raised, and employed in his district, sees himself as a microcosm of both groups. Voting in Congress, therefore, is rarely problematical for him. "I don't have any trouble knowing what the black community thinks or wants.... I don't have any trouble voting. When I vote my conscience as a black man, I vote right for the district." Though he does not feel constrained, he, of course, is. "If I voted against civil rights legislation, my people would probably ask me why I did that. But I never would do it." On one occasion when it might have looked to them as though he had, he explained and they were satisfied.

When I come home, I go to the church groups and tell them what's been going on in Washington and explain to them why I voted as I did. For instance, I explained to them that I voted against the Voting Rights Bill (1970) because it was a fraud. Nixon wanted to get the fifty-seven Registrars working in the South out of there. After they heard me on the Voting Rights Bill, they went home mad. I know my people will agree with me.

Congressman D falls in the "medium frequency" category in trips home, having made thirty such trips in 1973. "I meet with church groups and other groups; and I let people see me just to let them know I haven't lost touch with them." The "other group" with which he meets most frequently (once a month formally and more often informally) is his district's political organization — "my political lifeline." "The more people see me working for them in our organization, the more popular I become, the more they trust me and are proud of me." From this presentation of self comes, then, the essential condition for his voting leeway in Washington.

The fact is that I have the freedom to do almost anything I want to do in Congress and it won't affect me a bit back home. My constituents don't know how I vote, but they know me and they trust me.... They say to themselves, 'Everything we know about him tells us he's up there doing a good job for us.' It's a blind faith type of thing.

Congressman E describes his district as "heterogeneous — one-third urban, one-third suburban, one-third rural." His primary constituency is the rural area, where three generations of his family have lived. At the very moment we turned off a four-lane highway onto a back road on our way to a small town fair, he said,

It must be terrible to be without roots, without a place to call home. I have a profound sense of identification with these rural people. My wife still worries about me a little bit in this respect — that I'm too much of a country boy. But life's too short to play a role or strike a pose.

This. will be fun. I'm really going to enjoy myself.

Not only is he clearly most "at home" in the rural part of the district, but it casts a strategically disproportionate 40 per cent of his party's primary vote. "If these city people (in my party) with their slick city ways think they could go out to Mrs. O'Leary's cow pasture and get the farmers to throw out the local boy, they're crazy." His presentation of self is skewed toward his primary constituency. During his 35 trips and 100 working days at home in 1973, his scheduled appearances were allocated as follows: 41 in the urban area, 38 in the suburban area and 70 (or about 50 per cent) in the rural one-third of the district. His accessibility - especially to his primary constituency - is the essence of his presentational style and, hence, he believes, the basis for constituent trust.

Sometimes I do my talking to the same people over and over again. But they talk to others and they speak favorably about me. They tell others that 'old George' is always available and accessible. And I get a reputation in that way. That's how I succeed in this kind of district. People think of me as a nice guy, one of the boys, and they make presumptions in my favor because I'm a nice guy.

It is sometimes argued that representatives from heterogeneous districts enjoy a special degree of voting leeway because no single constituency interest controls their electoral future. Congressman E makes this argument. When asked if any single vote could defeat him in his district, he replied, "No, not in my district. It's too diverse, not all urban ghetto or Idaho potato farmers. That gives me a chance to balance interests in my votes. There really aren't any dominant interests." The crucial extra ingredient in the argument - one usually left out - is this: The congressman must be able to explain his voting pattern to his constituencies and have that explanation accepted. And this is especially true as regards his primary constituency. Congressman E com-

If I want to vote for an urban program, I can do it, and the people in the rural area will say, 'He does have an urban constituency and he has to help them, too.' And they will still vote for me so long as they think I'm a nice fella. But if I had no urban constituents — if I had all countryside — and I voted for an urban program, people in the rural areas would say, 'He's running for governor, he's forgotten who his friends are.' The same is true in the urban area. They know I'm a country boy and that I

have a lot of rural area. So they say, 'He gives us a vote once in a while; he's probably all right.'

Congressman E is not unconstrained. He would, he admits, lose his primary constituency if he voted consistently for urban programs.³⁴ He will also lose his primary constituency if they stop trusting him as "a nice fella" and no longer make "presumptions in my favor" when he explains his few urban votes. If we are to understand a congressman's voting patterns in Washington, it seems that we must also understand his presentational and explanatory patterns at home.

As a final note, two general patterns of explanation deserve mention. One I had expected to find but have not, and one I had not expected to find but have. Both invite further research. In view of the commonly held notion that elective politicians "talk out of both sides of their mouths" (which Goffman discusses in terms of performances before "segregated audiences"),35 I had expected to find members of Congress explaining their activity somewhat differently to their various constituencies. The likelihood seemed especially strong in heterogeneous districts, where the opportunity and temptation would be greatest. But I have found little trace of such explanatory chameleons in my travels. The House members I observed give the same explanations for their Washington activity before people who disagree with them as before people who agree with them - before nonsupporters as well as supporters, from one end to the other in the most segmented of districts. The lack of this kind of demagoguery, and the patient doggedness with which members explained their activities to unsympathetic audiences surprised me. I do not mean they went out of their way to find disagreement (though such a practice is of central importance to the presentation of self in some cases — such as Congressman C). I only mean that when disagreement was present, members offered the same explanation for their vote that they offered under all other conditions. Their presentation of self may vary from group to group in the sense that the basis for demonstrating

34Kindgon's respondents told him that they could explain one vote that went contrary to constituent expectations, but not "a string of votes." Kingdon, pp. 41-42

³⁵Goffman, Presentation of Self in Everyday Life, pp. 136ff. A possible pattern of explanation requiring further research is that members explain their activities in different policy areas to distinctive groups in the constituency. See Clausen, How Congressment Decide, especially his discussion of a special foreign policy constituency on pp. 225–226.

from group to group. As they reach out to each group in a manner appropriate to that group, they may take on some local coloration; and they may tailor their subject matter to fit the interests of their audience. However, they rarely alter their explanations of their Washington activity in the process.

An explanatory pattern I had not expected to find was the degree to which the congressman at home explains his Washington activity by disassociating it from the activity of his colleagues and of Congress as a whole. I had assumed that home styles would be highly individualized. And I should not have been surprised, therefore, when I heard every one of my seventeen members introduced at home as "the best congressman in the United States." But I was not prepared to find each of them polishing this individual reputation at the expense of the institutional reputation of the Congress.³⁶ In explaining what he was doing in Washington, every one of my House members took the opportunity to portray himself as "different from the others" - the others being "the old chairmen," "the inexperienced newcomers," "the tools of organized labor," "the tools of big business," "the fiscally irresponsible liberals," "the short-sighted conservatives," "the ineffective leadership," "the obstructionist minority," "those who put selfish concerns before country," and so on. The diversity of the House provides every member with plenty of collegial villains to flay before supportive constituents at home. Individual members do not take responsibility for the performance of Congress; rather each portrays himself as a fighter against its manifest shortcomings. Their willingness, at some point, to stand and defend their votes contrasts sharply with their disposition to run and hide when a defense of Congress is called for. Congress is not "we"; it is "they." And members of Congress run for Congress by running against Congress. Thus, individual explanations carry with them a heavy dosage of critical commentary on Congress.

Conclusion: Political Support, Home Style, and Representation

"A congressman has two constituencies," Speaker Sam Rayburn once said. "He has his

³⁶See also, Richard F. Fenno, "If, as Ralph Nader Says, Congress is 'The Broken Branch,' How Come We Love Our Congressmen So Much?", paper presented to Time, Inc. Symposium, Boston, Massachusetts, December, 1972, and reprinted in Congress in Change, ed. Norman Ornstein (New York: Praeger, 1975).

identification or empathy will have to differ constituents at home and he has his colleagues here in the House. To serve his constituents at home, he must serve his colleagues here in the House." For over twenty years, political scientists have been researching the "two constituencies." Following the thrust of Rayburn's comment, we have given lopsided attention to the collegial constituency on Capitol Hill. And we have neglected the constituency at home. Knowing less than we might about one of the two constituencies, we cannot know all that we should about the linkage between them. This paper argues for opening up the home constituency to more political science investigation than it has received. It suggests that students of Congress pay more attention to "home" as a research focus and a research site. No one can say what we might learn if the suggestion were heeded. But a few speculations will serve as a conclusion to this exploratory effort.

For one thing, it appears that a congressman's constituency is more complicated than our normal treatment of it in our literature suggests. We have not, of course, obtained a constituent's-eye view of the constituency, having tried to keep the congressman's perceptions as our sole vantage point. But from that vantage point alone, it seems that we must be more precise about what we mean by "his (or her) constituents." If we are going to continue to talk in the language of role orientations - in which the "trustee's" votes are not determined by his constituents, and in which the "delegate" does follow the wishes of his constituents - we shall have to know just which "constituents" we (and he) are talking about. If we are going to continue to do survey research - in which we match the attitudes of the congressman with the attitudes of his constituents, or with his perceptions of the attitudes of his constituents — we shall, again, have to know just what "constituents" we (and he) are talking about. The abstraction "his (or her) constituents" is only slightly more useful as an analytical variable than the abstraction "the people." It may be that the distinctions attempted here are not the most useful ways of dividing up the home constituency for analytical purposes. But the days when our literature acknowledges the complexities of politics on Capitol Hill while accepting the most simplistic surrogates for political reality at home those days ought to be numbered.

While this article has not dwelled directly on the topic of representation, our exploration has implications for the family of questions, both descriptive and normative, raised by studies of representation. For people studying the conditions of electoral accountability, this study has

something to say about the electoral accountability of members of Congress. They are not among Kenneth Prewitt's "volunteerists."37 House members work hard to get the job and work hard to keep it. In the course of that effort, they expend a great deal of time and effort keeping in touch with their various constituencies at home. Furthermore, they feel much more uncertain, insecure, and vulnerable electorally than our objective measures reveal. When a congressman describes his seat as "safe," he implicitly adds: "because, and so long as, I work actively to keep it so." And many a House retirement decision is made in anticipation of electoral difficulty. Surely, then, some of the necessary conditions for electoral accountability are present in Congress as they are not in certain other representative bodies in this country. But the existence of such conditions does not, by itself, tell us whether House members are as responsive to constituent desires as democratic theory may require. Our study can provide, however, some perspectives with which to pursue this normative inquiry.

For example, it appears that members of Congress feel a good bit more accountable and, hence, I would guess, are more responsible - to some constituents than to others. House members feel more accountable to some constituents than to others because the support of some constituents is more important to them than the support of others. Thus, the process by which people become and remain representatives is closely related to their activity while they are representatives. In a representative democracy, the process is electoral; and the central problem for the representative is that of winning and holding voter support. From the viewpoint of this paper, problems of representation and problems of support are inseparable. It is precisely because a congressman's right to represent depends on his cultivation of electoral support that he develops such a complex and discriminating set of perceptions about his constituents. In a representative democracy, the representative learns who his, or her, various constituencies are by campaigning for support among them. The one deficiency in Hanna Pitkin's splendid book is the undifferentiated, uncomplicated idea of the constituency which she employs in her study of representation.³⁸

And her monolithic view of "the constituency" remains viable only because she separates the process of running for office from the problems of representation. The traditional view of representation is somewhat more static and structural than the process-oriented view one gets by focusing on the cultivation of constituent support. To the congressman, the very word "political" connotes the ongoing problem of support. A "political vote" is one calculated to win (or to avoid losing) support. A "politically unwise move" is one that will lose support, etc. Focusing on the problem of support keeps the dynamism and the politics in the subject of representation. If politics is about "who gets what, when, and how," then the politics of representative democracy is about "who supports whom, when, and how — and how much." And, "what do various constituencies get in return for their support?"

A more inclusive, process-oriented view of representation has the effect of making it less exclusively a policy-centered subject. Traditionally, representation has been treated mostly as a structural relationship in which the congruence between the policy preferences of the represented and the policy decisions of the representative is the measure of good representation. The question we normally ask is: "How well does Representative X represent his or her district?" And we answer the question by matching and calibrating substantive policy agreement. But our view here is that there is an intertwining question: "How does Representative X carry his or her district?" To answer that question, we shall need to consider more than policy preferences and policy agreements. We shall need to consider the more encompassing subject of home style and the constituent trust generated by home style. We shall need to entertain the possibility that constituents may want good access as much as good policy from their representative. They may want "a good man" or "a good woman" whom they judge on the basis of home style and whom they trust to be a good representative in terms of policy. Indeed, the growing political science literature on voter behavior in congressional elections contains both evidence and speculation -- usually under the rubric of the rising pro-incumbent vote - that voters are looking increasingly to just such candidate-centered bases for tendering electoral support.39 The point is not that

³⁷Kenneth Prewitt, "Political Ambitions, Volunteerism and Electoral Accountability," *American Political Science Review*, 64 (March, 1970), 5-17.

³⁸Hanna F. Pitkin, *The Concept of Representation* (Berkeley: University of California Press, 1972). Pitkin recognizes the complexity of constituencies – see p.

²²⁰ of her book - but she does not incorporate the idea into her analysis.

³⁹Alan Abramowitz, "Name Familiarity, Reputation and the Incumbency Effect in a Congressional

policy preferences are not a crucial basis for the representational relationship. They are. The point is that we should not start our studies of representation by assuming they are the *only* basis for a representational relationship. They are not.

This last comment may be as valid normatively as it is empirically. What reason is there to believe that a relationship based on policy is superior to one based on home style - of which policy is, at most, only a part – as a normative standard for representative democracy? It may be objected that a search for support that stresses stylistic compatibilities between the representative and the represented easily degenerates into pure image-selling. And, of course, it may. But the search for support that emphasizes policy compatibilities between the representative and the represented easily degenerates into pure position-taking. It is, perhaps, the signal contribution of David Mayhew's elegant essay to make exactly this point.40 Position taking is just as misleading to constituents and as manipulative of their desires as image selling. Both representational bases, we conclude, may take a corrupt form. Appearing to do something about policy without a serious intention of, or demonstrable capacity for, doing so is no less a corruption of the representative relationship, no less an impediment to accountability and responsiveness than is the feigning of a personal relationship without a serious intention of establishing one. They are equally corrupt, equally demagogic. They are substitutes for any real effort to help make a viable public policy or to establish genuine two-way communication and trust. At the least, normative theory ought to take account of both policy and extra-policy standards of good

Election," The Western Political Science Quarterly, 28 (December, 1975), 668-684; Albert Cover, "Cne Good Term Deserves Another: The Advantage of Incumbency in Congressional Elections," paper delivered at American Political Science Association Convention, Chicago, September 1976; John Ferejohn, "On the Decline of Competition in Congressional Elections," American Political Science Review, 71 (March, 1977), 166-176; Morris Fiorina, "The Case of the Vanishing Marginals: The Bureaucracy Did It." American Political Science Review, 71 (March, 1977), 177-181; Raymond Wolfinger and Robert Arseneau, "Voting Behavior in Congressional Elections." See also Edward N. Muller, "The Representation of Citizens by Political Authorities: Consequences for Regime Support," American Political Science Review, 64 (December, 1970), 1149-1166, esp. p. 1157; Glenn R. Parker and Roger H. Davidson, "Bases of Public Assessments of Government Performance: The Content of Congressional Evaluations," unpublished paper, University of California at Santa Barbara.

representation, and acknowledge their respective corruptions.

Our concentration on and our preference for the policy aspects of representation carries a related implication. For a people who profess an attachment to representative democracy, we have always seemed curiously uncomfortable when our representatives devote themselves to contact with their constituents. We tend to denigrate the home part of the representational process as mere "errand-running" or "fencemending" and to assume that it takes the representative away from that which he, or she, really should be doing, i.e., making public policy in Washington. As one small example, we have always criticized, out of hand, the "Tuesday to Thursday Club" of House members who go home for long weekends - on the assumption, presumably, that going home was ipso facto bad. But we never inquired into what they did at home or what the consequences (other than their obvious dereliction of duty) of their home activity might have been. Predictably, the home activities described in this paper will be regarded, by some, as further evidence that members of Congress spend too little of their time "on the job." But this paper asks that we entertain the alternate view that the Washington and the home activities can be mutually supportive. Time spent at home can be time spent developing leeway for activity undertaken in Washington. And it may be that leeway in Washington should be more valued than the sheer number of contact hours spent there. It may be, then, that the congressman's effectiveness in Washington is vitally influenced by the pattern of support he has developed at home and by the allocational, presentational, and explanatory styles he displays there. To put the point most strongly, perhaps we cannot understand his Washington activity without first understanding his perception of his constituencies and the home style he uses to cultivate their support.

No matter how supportive of one another their Washington and home activities may be, House members still face constant tension between them. Members cannot be in two places at once. They cannot achieve legislative competence and maintain constituency contact, both to an optimal degree. The tension is not likely to abate. The legislative workload and the demand for legislative expertise are growing. And the problems of maintaining meaningful contact with their several constituencies — which may make different demands upon them — are also growing. Years ago, House members returned home for months at a time to live with their supportive constituencies, soak up the

⁴⁰ Mayhew, The Electoral Connection.

first hand. Today, they race home for a day, a weekend, a week at a time. The citizen demand for access, for communication, for the establishment of trust is as great as ever. The political necessity and the representational desirability of going home is as great as ever. So members of Congress go home. But the quality of their contact has deteriorated. It is harder to sustain a genuine two-way relationship - of a policy or an extra-policy sort - than it once was. They worry about it and as they do, the strain and frustration of the job increases, Many cope; others retire. Indeed, retirements from the House appear to be increasing. Political scientists do not know exactly why. 41 But the research reported in this article points to the possibility that an inability or an unwillingness to improve the quality of the home relationship may be a contributing factor. Those who cannot stand the heat of the home relationship may be getting out of the House kitchen. If so, people prepared to be more attentive to home are likely to replace them. Thus, our focus on home activity may help us understand some changing characteristics of House members.

Our professional neglect of the home relationship has probably contributed to a more general neglect of the representational side of Congress's institutional capabilities. At least it does seem to be the case that the more one focuses on the home activities of its members, the more one comes to appreciate the representative strengths and possibilities of Congress. Congress is the most representative of our national political institutions.42 It mirrors much of our national diversity, and its members maintain contact with a variety of constituencies at home. While its representative strengths surely contribute to its deserved reputation as our slow institution, the same representative strengths give it the potential for acquiring a reputation as our fair institution. In

home atmosphere, absorb local problems at a period of our national life when citizen sacrifice will be called for, what we shall be needing from our political institutions are not quick decisions, but fair decisions. Some of the recent internal congressional reforms have increased the potential for both representativeness and fairness. They have increased the equality of representation by distributing influence more broadly inside Congress. They have increased the visibility of representation by opening up congressional proceedings to public view. This is by no means all that is needed. But it is enough to place an added obligation on the members of Congress to educate their constituencies in the strengths as well as the weaknesses of the institution. Members should participate in consensus building in Washington; they should accept some responsibility for the collective performance of Congress; and they should explain to their constituents what an institution that is both collective and fair requires of its individual members.

> The evidence of this study indicates that members have the leeway to educate their constituents if they have the will to do so. The evidence of this study also shows that they may not have that will. Instead, they often seek support and trust for themselves by encouraging a lack of support and trust in the Congress. By refusing to accept responsibility for an institutional performance, they simultaneously abdicate their responsibility to educate their constituents in the work of the institution. Instead of criticizing House members for going home so much, we should criticize them for any failure to put their leeway to a constructive purpose - during the legislative process in Washington and during the explanatory process at home. The trust of his, or her, supportive constituents should be viewed by a House member as his, or her, working capital - not just to be hoarded for individual benefit but to be drawn on, occasionally, for the benefit of the institution. So long as House members explain themselves but not the institution, they help sustain (wittingly or unwittingly) the gap between a 10 per cent approval level for Congress and a 90 per cent re-election record for themselves. If this imbalance seems unhealthy for both Congress and the Republic, we have yet another justification for an increased scholarly attentiveness to our House members at home.

⁴¹See Stephen Frantzich, "Congressional De-Recruitment: A New Look at Turnover in House Membership," unpublished paper, Hamilton College,

⁴²The argument of the last paragraphs is more fully contained in Richard Fenno, "Strengthening a Congressional Strength," paper presented at a conference on "The Role of Congress" sponsored by Time, Inc., Washington, 1975, reprinted in Congress Reconsidered, ed. Lawrence Dodd and Bruce Oppenheimer (New York: Praeger, 1977).

Partisan Patterns of House Leadership Change, 1789—1977*

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Of all these Formes of Government, the matter being mortall, so that not onely Monarchs, but also whole Assemblies dy, it is necessary for the conservation of the peace of men, that as there was order taken for an Artificiall Man [the state], so there be order also taken for an Artificial Eternity of life... This Artificial Eternity is that which men call the Right of Succession.

- Thomas Hobbes, Leviathan

Introduction

Few of the world's legislative institutions have remained as unchanged over as long a period of time as the United States House of Representatives. Unlike the assemblies of France, Germany, and Italy, it has not had to contend with a number of regime changes and functional redefinitions. Unlike the British House of Commons, it has maintained its basic form of representation for almost two centuries. Extending the suffrage to the landless, the blacks, and women has not significantly changed the white, male middle-class character of its membership. The two-party system which exists within it today is very similar to the one which it spawned early in its existence.

The House has enjoyed the luxury of stability and continuity. When the few successful challenges to this continuity have occurred, they have become almost legendary. The "revolt" against Speaker Cannon in 1910–1911 occurred six decades ago, but so little has happened in the House since then, that this incident has taken on grandiose proportions. It has been recounted by congressional analysts in

*I would like to extend my appreciation to Jerome Clubb, Samuel Patterson, James Sundquist, and David Vogler for their thoughtful critiques of earlier incarnations of this manuscript and to W. Ross Brewer, Kathleen Frankovic, Barbara Hinckley, Norman Ornstein, Robert Peabody, and John Wahlke for their helpful comments. An earlier version of this paper was presented to the 70th Annual Meeting of the American Political Science Association, Chicago, August 29—September 2, 1974.

a dozen books and articles. Yet it rates only a paragraph in most of the widely-read American history textbooks.

Perhaps the real story of the House is its continuity and not its changes. This may account for the popularity and widespread acceptance of Nelson Polsby's "institutionalization" thesis.2 The "institutionalized House" which Polsby describes has become much like the "Artificiall Man" of the Hobbes quotation opening this paper. The House now appears to have a life of its own, separated from the parties, committees, and leaders who occupy space within it. The accumulated momentum of almost two centuries has left the House's legislative parties usually more in agreement than disagreement, most of its bipartisan legislative committees with institutional and not partisan loyalties, and a leadership shaped by the House not the shapers of it. The portrait of the House drawn by Polsby, Peabody, Huntington, Hinckley, Jones, Ripley, and others is that of a legislative assembly characterized by internal bureaucratization, electoral insulation, and

Among some of the better-known studies of the revolt are: Charles R. Atkinson, The Committee on Rules and the Overthrow of Speaker Cannon (New York: Columbia University Press, 1911); William Rea Gwinn, Uncle Joe Cannon, Archfoe of Insurgency: A History of the Rise and Fall of Cannonism (New York: Bookman Associates, 1957); Kenneth W. Hechler, Insurgency: Personalities and Politics of the Taft Era (New York: Columbia University Press, 1940), Chapter III; and James Holt, Congressional Insurgents and the Party System, 1909–1916 (Cambridge, Mass.: Harvard University Press, 1967), Chapter 2. An effort to parallel the revolt against Cannon with the 1961 Rules Committee fight may be found in Charles O. Jones, "Joseph G. Cannon and Howard W. Smith: An Essay on the Limits of Leadership in the House of Representatives," Journal of Politics, 30 (August, 1968), 617–646.

²Nelson W. Polsby, "The Institutionalization of the U.S. House of Representatives," *American Political Science Review*, 62 (March, 1968), 144–168; and Nelson W. Polsby, Miriam Gallaher, and Barry Spencer Rundquist, "The Growth of the Seniority System in the U.S. House of Representatives," *American Political Science Review*, 63 (September, 1969), 787–807.

the regular beat of its own well-worn drum.³

The "Artificiall Eternity" which Hobbes bestows upon legislative assemblies manifests itself in the floor leadership succession system of the House. Analysts of congressional leadership have detected signs of institutional routinization in succession patterns which appear to transcend political, partisan, and personality considerations. One attains a post because one occupies the penultimate position which leads to it. Barbara Hinckley's statement addresses this phenomenon directly:

Although formally elective, the actual selection bears all the marks of a much more automatic procedure. Leaders stay in their posts for a number of congresses. New leaders are recruited from apprenticeship posts. Whip leads to floor leader. Floor leader to Speaker.4

Hinckley goes on to suggest that both parties in both houses operate this way in the selection and retention of their leaders. Exceptions have occurred and are duly noted. The thrust of Hinckley's analysis, however, is toward the identification of institutionalized leadership

³Certainly not all of the authors listed here would agree totally with my characterization of the House, but their studies provide sufficient data to enable one to gain this portrait. In addition to the Polsby articles cited above, some of the recent books and articles which have tried to assess the contemporary House which have tried to assess the contemporary House within an institutionalized context are: Michael Abram and Joseph Cooper, "The Rise of Seniority in the House of Representatives," Polity, 1 (Fall, 1968), 52–85; Roger H. Davidson, The Role of the Congressman (New York: Pegasus, 1969), esp. Chap. 2; Samuel P. Huntington, "Congressional Responses to the Twentieth Century," in The Congress and America's Future, ed. David B. Truman (Englewood Cliffs, New Jersey: Prentice-Hall, 1965), 5–31; Charles O. Jones, Every Second Year: Congressional Behavior and the Two-Second Year: Congressional Behavior and the Two-Year Term (Washington, D.C.: The Brookings Institution, 1967); Charles O. Jones, The Minority Party in Congress (Boston: Little Brown & Co., 1970); Randall B. Ripley, Party Leaders in the House of Representatives (Washington, D.C.: The Brookings Institution, 1967); Randall B. Ripley, Majority Party Leadership in Congress (Boston: Little Brown & Co. Institution, 1967); Randall B. Ripley, Majority Party Leadership in Congress (Boston: Little Brown & Co., 1969); Barbara Hinckley, Stability and Change in Congress (New York: Harper & Row, 1971); Robert L. Peabody, "Party Leadership Change in the United States House of Representatives," American Political Science Review, 61 (September, 1967), 675-693; and T. Richard Wittmer, "The Aging of the House," Political Science Quarterly, 79 (December, 1964), 526-541; and the articles by H. Douglas Price and Morris P. Fiorina, David W. Rohde, and Peter Wissel in Congress in Change: Evolution and Reform, ed. Congress in Change: Evolution and Reform, ed. Norman J. Ornstein (New York: Praeger Publishers, 1975), 2-57.

⁴Hinckley, Stability and Change in Congress, p. 121. She also deals with this subject in greater detail in "Congressional Leadership Selection and Support: A Comparative Analysis," *Journal of Politics*, 32 (May, 1970), 268–287, esp. pp. 275–278.

institutional autonomy marching most often to selection norms which seem to triumph over party-related and chamber-related ones.

> Hinckley relies upon Robert Peabody's 1967 analysis of leadership change to account for the "violations" of these congressional norms that occur. Peabody contended that change often represents the internal consequences of the external factor of election results: majority or minority status and the aggregate size of the net gain or loss of seats. As explained by Peabody:

Strong victories promote good will and generally reflect to the benefit of party leaders. Conversely, defeat results in pessimism, hostility and a search for scapegoats. If the net losses are particularly severe, as many as thirty to fifty seats, then the possibilities of minority leadership change through revolt are greatly enhanced.5

Combining the Hinckley and Peabody analyses, two propositions emerge: Since House leadership succession patterns have been institutionalized for both parties, the only violations of these patterns will occur during periods of party frustration. The inference encompasses all of the House changes in the six Democratic Congresses which Peabody observed in his initial study (1955-1966) and all but three of the changes in the ten postwar Congresses which Hinckley examined (1947-1966). While this proposition may illuminate change in the contemporary House, it is less useful in explaining leadership change in previous Congresses for the simple reason that the present era in the House is very atypical. Democratic dominance of the House in twenty-two of the twenty-four Congresses since 1931 has produced an even more persistent era of one-party control than that of the Democratic-Republicans in the first twelve Congresses of the nineteenth century. Assertions about the impact of majority or minority status on party leadership in the contemporary House may mask genuine differences between the Democrats and Republi-

Peabody's latest study, Leadership in Congress, recognizes the fact that the present-day uniqueness, "makes it impossible to prove or disprove any hypotheses relating leadership change to majority-minority status as over and against party differences, Democrat and Republican. For such interpretations longer range historical refinement will be required."6

⁵Peabody, "Party Leadership Change," pp. 687-688.

⁶Robert L. Peabody, Leadership in Congress: Stability, Succession, and Change (Boston: Little Brown & Co., 1976), p. 443. Peabody also indicates that the implication of his earlier research that party differen-

It is in the spirit of providing the historical dimension that this study was undertaken for it is my belief that the contemporary quest for both institutional norms of leadership succession and electoral system predictors of leadership change has led to an undervaluation of the impact of political party identity upon the selection of House leaders.

Party factors persist. For years, legislative analysts have contended that party identity is the single best predictor of congressional voting. Pavid Mayhew, for example, has pointed out that the House Democrats have become an "inclusive" party, attentive to the demands of many interests, while the House Republicans have become an "exclusive" party limited to a smaller range of interests. Regarding the recruitment of House members, Joseph Schlesinger has observed that both parties are "office-based" but that the Democrats appear to be recruited in a more "open" system while the Republican system appears to be more "hierarchic."

Since party members in the House have been recruited differently and seem to vote differently, these differences should also manifest themselves in each party's leadership selection system. In order to test this party-related proposition, the analysis was extended back to the First Congress. In this way, changes over time could be identified and periods could be analyzed in which a number of parties — not just the contemporary Democrats — held majority status.

ces were less important than status differences has been changed. He now sees the party identity factor in leadership change as "considerable" (p. 291).

⁷The existence of party-related voting patterns is generally acknowledged. How much of the variations in the patterns is attributable to party factors is the debatable issue. Compare two entries in the debate: Cleo H. Cherryholmes and Michael J. Shapiro, *Representatives and Roll Calls* (Indianapolis, Ind.: Bobbs-Merrill, 1969), pp. 106–110, on the importance of party influence; and Aage R. Clausen, *How Congressmen Decide: A Policy Focus* (New York: St. Martin's Press, 1973), pp. 91–100, for a caveat.

⁸David R. Mayhew, Party Loyalty among Congressmen: The Difference between Democrats and Republicans, 1947–1962 (Cambridge: Harvard University Press, 1966), pp. 148–160. Two nonacademic tooks make a similar argument: Dean Acheson, A Democrat Looks at His Party (New York: Harper and Brothers, 1955); and Milton Viorst, Fall from Grace: The Republican Party and the Puritan Ethic, rev. ed. [New York: Simon and Schuster, 1971), esp. Chap. 5 on "The Lost Constituencies."

⁹Joseph A. Schlesinger, "Political Careers and Party Leadership," in *Political Leadership in Industrialized* Societies: Studies in Comparative Analysis, ed. Lewis J. Edinger (New York: John Wiley & Sons, 1967), 266–293, esp. pp. 280–284. For this analysis the voting records on the floor during speakership contests, votes in the various party caucuses, retention rates of House leaders in their positions, and the reasons for the leadership departing were examined. These four sets of data make it possible to discern both party-related and time-related impacts upon the leadership selection process in the House.

House Leaders: Positions and Periods

Of all the approaches to leadership identification, the simplest to understand is the "positional" or "formal-leadership" approach which selects as leaders the occupants of key posts. 10 Within the House of Representatives, the most important organizational positions are those of Speaker, floor leader, and whip. Not all holders of these posts have been prominent members of the House, nor have they even enjoyed the full confidence of the legislative parties which selected them, but there is no question that these offices are crucial, regardless of their occupants. For this reason, any member who held the office of Speaker, floor leader, or party whip must be categorized as a leader of the House.

Positions. Apart from the Speakership, no leadership position can be consistently defined over the entire course of House history. Different positions during different eras fulfilled the functions of floor leadership. The floor leadership of the majority party was first associated with the appointed chairmanship of the Ways and Means Committee shortly after its creation as a standing committee in 1795. When the Appropriations Committee came into existence in 1865, the floor leadership was often shared by both chairmen. Movement between these two committees was not infrequent and their chairmen and ranking members often served with the Speaker on the Rules

¹⁰Wendell Bell, Richard J. Hill, and Charles R. Wright, *Public Leadership* (San Francisco: Chandler Publishing Co., 1961), p. 6.

11William Loughton Smith of South Carolina was the initial chairman of Ways and Means as a standing committee. He was replaced in the Third Session of that Congress by Robert Goodloe Harper, also of South Carolina. Harper is the first party floor leader mentioned by DeAlva Stanwood Alexander in his chapter on "Floor Leaders" in History and Procedure of the House of Representatives (Boston: Houghton, Mifflin, 1916), p. 107. For an assessment of how and when the various leadership posts came into being in the House, see Garrison Nelson, "Leadership Position-Holding in the United States House of Representatives," Capitol Studies, 4 (Fall, 1976), 11–36.

Committee during the peak of its influence as a vehicle for the House majority. The most fundamental change in the majority leadership occurred in 1911 when the Democratic caucus asserted its authority and made the post elective. Since then all majority leaders have been elected.

Minority leadership prior to 1863 was blurred by the multiplicity of parties in the House and the frequent manifestation of intraparty conflict on the floor during the balloting for Speaker. After 1863 all but one of the sixty-one speakership contests were settled on the first ballot with the minority nominee receiving virtually all of the votes cast by his party. Deciding to use this designation for the minority leadership represents a variation on the positional approach for the simple reason that the position did not exist until 1883 and did not receive official recognition until 1911. For practical purposes I am willing to regard the minority's nominee for

¹²Ripley indicates the presence of both positional leaders in his description of majority leadership patterns between 1861 and 1911, see his Party Leaders in the House of Representatives, p. 84. Thaddeus Stevens of Pennsylvania, Chairman of Ways and Means (1861–1865) was the first to chair the Appropriations Committee. Henry L. Dawes of Massachusetts chaired both Appropriations (1869–1871) and Ways and Means (1871–1875). In the years from 1880 through 1911 when the House Rules Committee functioned under the chairmanship of the Speaker, twenty-four of the other thirty-eight committee assignments held by the two other majority members were from Ways and Means (17) and Appropriations (7).

13Chang-Wei-Chiu, The Speaker of the House of Representatives Since 1896 (New York: Columbia University Press, 1928), pp. 318-320. See also The New York Times, January 19, 1911, p. 1.

14Prior to 1863, there were thirteen multi-ballot speakership floor contests: the 3rd (1793–1795); 6th (1799–1801); 9th (1805–1807); 11th (1809–1811); Second Session of the 16th (1820–1821); 17th (1821–1823); 19th (1825–1827); Second Session of the 23rd (1834–1835); 26th (1839–1841); 30th (1847–1849); 31st (1849–1851); 34th (1855–1857); and the 36th (1859–1861) Congresses. See the House Journals for the relevant years and various issues of Niles' Weekly Register (Baltimore).

15The only exception involved Frederick Gillett of Massachusetts who was renominated by the Republican caucus in 1923 to be its candidate for Speaker, but it took nine ballots before he was able to gain the votes of twenty Republican members who cast their earlier ballots for Henry A. Cooper of Wisconsin and Martin B. Madden of Illinois. House Journal, 68th Congress, First Session, pp. 5-11.

¹⁶Ripley, Party Leaders in the House of Representatives, p. 28. James R. Mann of Illinois was the first to be designated "Chairman of the Minority Conference," in the Congressional Directory, 62nd Congress, 1st Session (May, 1911), p. 190.

Speaker as the person to whom they would have entrusted the most important office in the House, hence their leader.

Party whips emerged first among the Republicans with Speaker Reed's designation of James A. Tawney in 1897.¹⁷ The Republican leadership held the power of appointment of whips until 1919 when the caucus took control of the post.¹⁸ In 1921 the Republican Committee on Committees elected the whip and held this responsibility until 1965 when the Republican membership reasserted its authority and made the post fully elective again.

The position of Democratic whip had a sporadic history following Oscar Underwood's initial appointment to the post in 1900.¹⁹ Not until 1921 did the post develop clear continuity. It has been an appointive post since its inception. Both the reform-minded 94th and the newly-elected 95th Congresses defeated efforts to make the Democratic whip post elective.²⁰

To summarize, the positions and relevant years used to identity the House leadership population are (1) the Speakers of the House (1789–1977); (2) the appointed chairmen of the Ways and Means Committee from its inception in the Fourth Congress to its separation from Speaker selection in the 62nd Congress (1795–1911); (3) the appointed chairmen of the Appropriations Committee from the 39th Congress through the 55th Congress when only the Ways and Means chairman was designated as majority leader (1865–1899) plus one six-year stint when James A. Tawney was its chairman (1905–1911); (4) the caucus-elected majority leaders from the 62nd Con-

¹⁷Randall B. Ripley, "The Party Whip Organizations in the United States House of Representatives," American Political Science Review, 58 (September, 1964), 561–576. Ripley, using Edward T. Taylor's A History of the Committee on Appropriations, House Document 299, 77th Congress, First Session (1941), p. 51, argues the case for Reed's appointment of Tawney in 1897.

18Ripley, Party Leaders in the House of Representatives, p. 33. See also The New York Times, March 12, 1919, p. 1.

¹⁹*Ibid.*, pp. 36–38.

²⁰The Democratic Caucus for the 94th Congress voted down a proposal to make the whip post an elective one, 138–32, Congressional Quarterly Weekly Report, 33 (December 7, 1974), p. 3247. The organizing caucus for the 95th Congress "shouted down" the proposal without a vote, see the Boston Globe (December 9, 1976), p. 20.

21See "Tawney Republican Leader," in *The New York Times*, December 3, 1905, p. 3. An early comment on Tawney's aggressive role may be found in "House Chiefs in Panic at Tawney's War Howl," *The New York Times*, December 18, 1905, p. 9.

gress to the present (1911-1977); (5) the minority's nominees for the speakership (1863-1911); (6) the caucus-elected and Congressional Directory-designated minority leaders (1911-1977); (7) the Republican party whips (1897-1977); and (8) the Democratic party whips (1899-1909, 1913-1915, and 1921-1977).

The positional approach identifies 128 men as holders of leadership posts in the House from 1789 through 1977.²² These posts have been held by seventy-one members identified with the Democratic party or its predecessor, the Democratic-Republicans, and by fifty-four members associated with the major opponents of the Democrats. This latter figure includes six Federalists, five Whigs, one American and forty-two Republicans. Three leaders held their positions under different party labels.²³

²²A list of the leaders selected for this study may be found in Garrison Nelson, "Appendix: Roster of Leaders of the United States House of Representatives, 1789–1976," *Capitol Studies*, 4 (Fall, 1976), 25–36.

²³Frederick A. C. Muhlenberg served as Speaker for both the Federalist First Congress (1789–1791) and the Democratic-Republican Third Congress (1793–1795), see William Henry Smith, Speakers of

In Table 1, the positions are identified by relevant time frame and the number of selections and occupants for each post are enumerated. The figure of 364 selections constitutes the population of cases examined in this study.

Periods. The House's 189 year history can be divided into three eras based upon the elective-appointive dimension for the leadership posts. The first era, 1789-1863, contains the Congresses in which only two leadership posts can be readily identified: the elected Speaker and the appointed Chairman of the Ways and Means Committee. The second era, 1863-1911, begins with the 38th Congress, where an organized minority floor vote for Speaker can be detected. This era also includes the addition of the Appropriations Committee chairman as majority co-floor leader and the initial appearances of appointed party whips. It ends in

the House of Representatives of the United States (Baltimore: Simon J. Gaeng, 1928), p. 20. The other two leaders to serve under differing party majorities were John W. Taylor and Louis McLane who served as Speaker and Ways and Means chairman respectively as National Republicans in the 19th Congress (1825–1827) after holding the same posts during the Democratic-Republican majorities earlier.

Table 1. House Leadership Positions and Selection Methods: Number of Selections and Occupants by Party, 1789-1977

	Number of								
	Selections								
	Dem.	Non- Dem.	N	Dem.	Non- Dem.	N			
Elective Posts									
Speaker of the House, 1789-1977	66	36	102	28	20	46 ⁸			
Minority floor leader, 1863-1977	25	36	61	17	10	27			
Majority floor leader, 1911-1977	28	8	36	11	4	15			
Republican whip, 1919, 1965-1977	0	8	8	0	3	3			
Semi-Elective Post		•			_				
Republican whip, 1921-1965	-	23	23	-	5	5			
Appointive Post	36	. 27	63	23	16	202			
Chairman, Ways & Means, 1795–1911 Chairman, Appropriations, 1865–1899,	30	21	03	23	16	38 ^a			
1905–1911	9	13	22	4	7	11			
Republican Whip, 1897–1919		11	11	~	5	5			
Democratic Whip, 1900-1908, 1913,					_	_			
1921–1977	38		38	16	-	16			
Totals									
Elective posts	119	88	207	56	37	91ª			
Semi-elective posts	0	23	23	0	5	5			
Appointive posts	83	51	134	43	28	70ª			
	202	162	364	99	70	166a			

⁸Two Speakers (Muhlenberg and Taylor) and one Chairman of Ways and Means (McLane) held their posts during changes in party control.

^bAlso, two Republican Whips (Knutson and Arends) were selected by two differing methods. This further reduces the number of post occupants to 164.

1911 with the first election of a majority floor leader by the caucus. The present era, 1911-1977, is marked by an increase in the number of House party floor leaders directly elected by the membership.

Reducing the House's history to three time periods makes it more manageable but it does reduce the number of temporal observation points. Consequently, time-related changes can only be noted in broad outline and not as a steady progression towards some ultimate destination.

Continuity and Change in Leadership Position-Holding

In examining leadership change and continuity, one must differentiate between changes in leadership post and changes in leadership rank. The distinction is important because a leader may change his post, but not his rank. For example, leaving the speakership to become minority leader represents a change of institutional posts, but a continuity in party rank. Similarly, a leader may maintain his post but change ranks (e.g., a leader who remained in the post of whip when his party gained control of the House would be "moved" from the second-ranking post to the third-ranking one).

This particular distinction has increased in importance because of the high degree of interpositional mobility which has occurred since 1863. During the first period, only three of forty-seven leaders (6 per cent) moved between posts. Following 1863, mobility has increased with twenty-eight of the eighty-two leaders (34 per cent) having held at least two posts. The reason for this increase is that three more leadership posts were added as the minority became more recognizable and the whip system was formalized.

Both parties have moved leaders between positions in the last two eras. Since 1863, seventeen of the forty-four Democratic leaders (39 per cent) and eleven of the thirty-eight Republican leaders (29 per cent) held more than one post. One major difference occurs in recent years. The rate of interpositional mobility among Democratic leaders since 1931 is 50 per cent (9/18) while the Republican figure in the same era is only 22 per cent (2/9). There are two reasons for this party differential. First, the Democrats have become a semipermanent minority since 1931 and they have had continually one more leadership post to disburse at the opening of each Congress, namely, the speakership. Second, the emergence of the "leadership ladder" - the whip to leader to Speaker succession - has made holding the upper-level

offices contingent upon holding the lower ones.

Interpositional mobility as manifested in the "leadership ladder" is deceptive as a guide to House leadership change. The first post in the sequence, that of whip, is a much more integral part of the House Democratic leadership structure than it is of the Republican one. Four Democratic majority leaders began their ascents as party whips: Oscar Underwood, Carl Albert, Hale Boggs, and Thomas P. O'Neill. In the cases of the latter three, their service as whip was a crucial element in their elections for the higher post.24 Also, the Democrats have used the whip position as their second-ranking post in the last two Congresses in which they served in the minority. Sam Rayburn placed ex-Majority Leader John McCormack in that slot when he assumed the minority leadership in both 1947 and 1953.25 This is not true of the Republicans. Only the first Republican whip, Tawney, moved into either of the two top leadership positions and that occurred when the floor leadership was also an appointive post. No Republican whip has been displaced by the former majority leader when the party lost control of the House.

The two parties have treated the position differently. For the House Democrats, the position of whip has become a "penultimate" one, a step away from majority leader; but for the Republicans, it is a "career office," to be surrendered only upon departure from the

24On Albert's election in 1962, see Nelson W. Polsby, "Two Strategies of Influence: Choosing a Majority Leader, 1962," in New Perspectives on the House of Representatives, ed. Robert L. Peabody and Nelson W. Polsby, 2nd ed. (Chicago: Rand McNally, 1969), pp. 325–358. Specific references to Albert's service as whip are made on pp. 335 and 342. Hale Boggs's victory is described in Larry L. King, "The Road to Power in Congress," Harper's Magazine, 252 (June, 1971), 39–63, esp. pp. 48 and 62 on the "leadership ladder." See also John F. Bibby and Roger H. Davidson, On Capitol Hill: Studies in the Legislative Process, 2nd ed. (Hinsdale, Ill.: The Dryden Press, 1972), pp. 124–144. O'Neill's position as whip was mentioned in all of the news accounts covering his contests for leader and in these accounts, the pattern of ordered succession was often described, see The New York Times, November 11, 1972, p. 20, and the editorial, "Potential Speaker," The New York Times, November 24, 1972, p. 36. See also, Peabody, Leadership in Congress, Chapter 8.

²⁵Following the Democratic defeat in the 1946 congressional elections, Sam Rayburn purportedly intended to leave the House leadership to John McCormack. Opposition to McCormack from a number of southern members was strong enough to dissuade Rayburn and he reluctantly accepted the nomination as minority leader. He then appointed McCormack to be whip, *The New York Times*, January 3, 1947, pp. 1 and 4; and January 8, 1947, p. 14.

House.²⁶ Eight of the ten Republican whips had no House career following their last term in that post, and one other served less than six months after relinquishing it. Leslie Arends of Illinois, who held the post from 1943 through 1974, is the latest to follow in that pattern.

This difference between the parties has been overlooked by some House analysts and has led them to conclude that the Republicans are violating a House "norm" when their whip retains his post regardless of the majority or minority status of the party.²⁷ Unlike the Democratic whip, the Republican whip is not part of the rank structure.

One curious feature of this party differential is that it appears to contradict some of the assumptions of Joseph Schlesinger's "ambition theory." Republican whips with constituencies almost congruent (and identical since 1965) with the floor leaders should be expected to move up when the floor leadership falls vacant; and the Democratic whips, the appointees of the incumbent leadership, should be expected to return to the ranks of the ordinary members once their patrons had left their positions of influence. This has not occurred. For this reason it is important to maintain the party variable in discussions of "ambition theory" as well as "institutionalization."

The leadership contests of 1976 reaffirmed in part the continued reliance of the Democrats upon the "leadership ladder" to control the succession process and limit internecine warfare among the members. Majority Leader O'Neill was nominated for Speaker without a dissenting vote and Majority Whip John McFall felt obliged to move up as well.²⁸ McFall's failure to gain the floor leadership is an indication that the Democratic membership's tolerance for automatic elevations had declined. It is important to note, however, that McFall was

²⁶These terms come from Joseph A. Schlesinger, Ambition and Politics: Political Careers in the United States (Chicago: Rand McNally & Co., 1966), p. 90 for "penultimate office," and p. 46 for "career office." There have been only three Republican whips since 1933 — Harry Englebright of California, 1933–1943; Leslie Arends of Illinois, 1943–1975; and Robert Michel of Illinois, 1975–present.

²⁷Barbara Hinckley, "Congressional Leadership Selection and Support: A Comparative Analysis," pp. 275-277.

28O'Neill was nominated without a contest, but McFall finished fourth in a four-man field and was eliminated on the first ballot, see "House Democrats Elect Leaders, Slow Reforms," Congressional Quarterly Weekly Report, 34 (December 11, 1976), pp. •3291-3295. The eventual winner, Jim Wright of Texas, defeated Philip Burton on the third ballot, 148 to 147.

O'Neill's personal choice to succeed him in the post and had McFall not been so badly tarnished by the "Korean payoff" scandal, then the "ladder" may have made him the fourth consecutive Democratic whip to become majority leader. O'Neill seems to recognize that the heterogeneity of the party's membership would spawn a number of bitter leadership contests if the succession were less tightly controlled. While he may have been unable to help McFall, the Speaker was able to defeat the man he least wanted in the position, Representative Philip Burton, and also to retain the power of appointment over the party whip.

The Republicans, with their more homogeneous composition seem to have no need for a routinized succession system. Leadership changes in that party, particularly in more recent years, appear to have more to do with style than substance.²⁹

Leadership Selections

All of the 364 selections since 1789 have been classified. Operationally, leadership continuity in any given Congress occurs when the leader remains in the same post as in the previous Congress. Change, therefore, occurs when a leader vacates his post.

Defined in this manner, change and continuity appear to occur at an identical rate. Of the 364 selections, 177 represented post continuations, while 181 represented post changes. There were six cases which involved the first selections in new posts. Focusing solely upon the 358 selections which involved post continuations or post changes, Table 2 presents the number of instances which occurred in each party by period.

The least surprising aspect of Table 2 is the steady increase in the proportions of post continuations relative to changes over the three periods. One reason for the increase is the stabilization of the leadership posts. A second reason relates to institutionalization; as more leaders choose to remain in the House, they receive more selections, as may be seen in Table 3.

The only period in which the Democratic leadership received more selections and experi-

²⁹In both the 1959 Martin-Halleck and 1965 Halleck-Ford contests, commentators noted the presence of style considerations and the absence of philosophical ones. See Charles O. Jones, Party and Policy-Making: The House Republican Policy Committee (New Brunswick, N.J.: Rutgers University Press, 1964), pp. 37–38; and Robert L. Peabody, The Ford-Halleck Minority Leadership Contest (New York: McGraw-Hill, 1966), p. 32.

enced fewer post changes than their opponents was in the first one. This was due largely to the fact that the Democrats (and Democratic-Republicans) organized twenty-six of the period's thirty-seven Congresses and held this control for lengthy spans of time.

The House Democrats had more leadership changes than the Republicans did in both the second period (in which they organized only eight of the twenty-four Congresses) and the third period (in which they organized twenty-six of thirty-four). Despite the consistency of the interparty differences in Table 2, the reasons for the higher rates of Democratic change vary. In the second period, the major problem was the inability of the Democrats to organize themselves effectively as waves of new southern congressmen rejoined their ranks. As a result, ten of the sixteen caucus nominations

for the Democratic speakership candidate were contested in the years between 1871 and 1899.³⁰ Only four Republican caucuses in the same period showed dissension.³¹ Not until 1899 under the successive leadership stints of James Richardson, John Sharp Williams, and Champ Clark did the Democrats manifest any significant degree of leadership continuity.

Period III's interparty difference indicates more post changes for the Democrats, but with a higher number of selections per leader. The "leadership ladder" accounts for this anomaly

30Caucus nominations in the Democratic party were contested in 1871, 1873, 1875, 1876, 1877, 1879, 1883, 1891, 1897, and 1899, see various editions of *The New York Times* and *The Washington Post*

³¹Caucus nominations in the Republican party were contested in 1881, 1883, 1885, and 1889.

Table 2. Post Continuations and Changes by Party and Period

	Cont	inuation	Ch	iange	Totals		
Era and Party	N	%	N	%	N	%	
1789-1863							
Democratic	20	36.4	35	63.6	55	100.0	
Opponents	4	17.4	19 54	82.6	$\frac{23}{78}$	100.0	
	24	30.8	54	69.2	78	100.0	
$\chi^2 = 2.7398$, Insignificant; g	amma = +.462					
1863-1911			,				
Democratic	16	34.8	30	65.2	46	100.0	
Republican	$\frac{31}{47}$	52.5	$\frac{28}{58}$	47.5	59	100.0	
•	47	44.8	58	55.2	105	100.0	
$\chi^2 = 3.3109$, Insignificant; g	amma =350					
1911-1977							
Democratic	53	54.6	44	45.4	97	100.0	
Republican	53	67.9	2 <u>5</u> 69	32.1	78	100.0	
	106	60.6	69	39.4	175	100.0	
$\chi^2 = 3.2021$; Insignificant; g	amma =275					
1789-1977							
Democratic	89	44.9	109	55.1	198	100.0	
Opponents	88	55.0	72	45.0	160	100.0	
	177	49.4	181	50.6	358	100.0	
$\chi^2 = 3.5809$; Insignificant; g	amma =199					

Table 3. Party Differences in Mean Selection Frequencies

Periods	Democratic		Non-Democratic		DemOpp.	All Leaders	
	Mean	N	Mean	N	Differences	Mean	N
I, 1789–1863	1.87	30	1.15	20	+.72	1.68	47
II, 1863–1911	2.18	22	3.05	20	87	2.59	42
HÍ, 1911–1977	4.08	24	4.11	19	03	4.09	43
Totals	2.73	74 ^a	2.84	57 ^a	11	2.87	128 ^a

^aAdjusted for period overlapping and party switching.

not out of the leadership. Republicans in this era appear to be more conscious of post continuity and leave their leaders in place until either the leader's career ends or the caucus arises and removes him from office.

This variety in reasons for leadership change needs more refinement. In the next section, types of leadership change will be examined for each period.

Types of Leadership Change

In the most thorough study of leadership in the contemporary Congress, Robert Peabody identified three forms of change in the House: interparty turnover, intraparty change, and institutional reform.³² The last appears in the redefinition of leadership posts, such as a change from an appointive to an elective majority leader or the emergence of a new post among the leadership hierarchy. Comparatively speaking, institutional reform has not been an important agent of leadership change. Interparty and intraparty factors have had a far greater impact upon the identity of the House leadership.

Peabody's identification of change agents allows for but does not deal with the disappearance of leaders because of resignation, death, or electoral defeat. So few leaders have resigned from either the leadership or the House itself in recent times that this factor may not seem relevant to analysts of the contemporary Congress. Similarly, recent House leaders, like the vast majority of the members, have become so immune from the vicissitudes of the American electorate that it is difficult to believe that leadership careers have ended because the leader failed to obtain renomination or re-election in his own district. As presently interpreted by students of Congress, the electorate's impact upon the House leaders is indirect. Party failure at the polls presumably angers the House caucus which thereupon garrots its leaders.

In Table 4, the various reasons for changes in House posts are catalogued by period. This table includes Peabody's three types of leadership change and adds another category which I call "career-related changes."

Career-Related Changes. Included in this category are events which affect the leader's own career regardless of what happens to his party at the polls or to his position within it. This category has two subdivisions: personal and

as Democratic leaders move between posts but political. Personal career-related events which affect change include retirement from the House, death in office, or resignation from a leadership post. Retirement refers to a leader's leaving the House and not directly occupying another political office. A number of the retirees in early periods returned to public life after an absence of a year or more. Retirees in this present era are often too old to do much more than contemplate their past achievements. This age factor is reflected in the sizeable number of deaths among House leaders in this century. For example, between 1789 and 1931. only one of thirty-six Speakers died in office, but since then, five of the last nine Speakers to leave the office died holding the chair.

> Comparing the two career-related subdivisions in the present period with the two previous ones indicates that personal factors now outweigh political ones in causing leadership change. Thus, Period III's retirements and deaths easily outnumber leadership departures arising from electoral defeats. Only two of Period III's forty-three leaders were defeated in re-election bids: Champ Clark in 1920 and Carl Bachmann in 1932. Prior to 1911, re-election defeats ended eighteen leadership careers. The party identity of the leader had no bearing on the rates of re-election defeat; all parties suffered equally.

Leaving the House in quest of other political offices was a very common occurrence in the first period, but less so in the later ones. Most often it was the lure of the Senate which took leaders out of the House. Seventeen of the twenty-four leaders who left the House for other elective offices contested for Senate seats. Thirteen were successful. The last one was John Sparkman of Alabama who relinquished the whip post in 1947. Two were elected governor (two were defeated), two were elected Vice President (Colfax in 1868 and Garner in 1932), and one, James A. Garfield, left the House to become president. Two leaders became Secretary of State (Henry Clay and Elihu Washburne). Since 1869, only one leader has left the House directly for an appointive post: Gerald Ford in October, 1973.

Polsby's contention that the House has "boundaries" from the other developed branches and levels of government is supported by the decline in the number and proportion of House leadership changes due to career-related political events.33 The leadership appears insulated from both their constituents and from

³² Peabody, "Party Leadership Change," op. cit., pp. 676-678.

³³Polsby, "The Institutionalization of the House," pp. 145-153.

Table 4. Reasons for House Leadership Changes by Period

	17	89-18	363	18	1863-1911			1911-1977			1789-1977		
Types of Changes	Dem.	Opp.	Tot.	Dem.	Rep.	Tot.	Dem.	Rep.	Tot.	Dem.	Opp.	Tot.	
Career-Related Changes	51%	42%	48%	40%	57%	48%	36%	52%	42%	42%	51%	46%	
Personal	14%	0%	9%	10%	14%	12%	25%	32%	28%	17%	17%	17%	
Retired from House	14	0	9	3	7	5	7	16	10	8	8	8	
Died in office	0	0	0	3	7	5	18	8	15	8	6	7	
Resigned post	0	0	0	3	0	2	0	8	3	1	3	2	
Political	37%	42%	39%	30%	43%	36%	11%	20%	14%	25%	35%	29%	
Not re-elected	11	16	13	20	18	19	2	4	3	10	12	11	
Elected: other office Appointed: other	11	11	11	10	18	14	7	4	6	9	11	10	
office	11	11	11	0	4	2	0	4	1	4	6	4	
Defeated: other office	3	5	4	0	4	2	2	8	4	2	6	3	
Inter-Party Turnover	17%	47%	28%	27%	29%	28%	32%	24%	29%	26%	32%	28%	
Loses elective post	6	32	15	0	0	0	2	8	4	3	11	6	
Loses appointive post	11	16	13	17	11	14	5	0	3	10	8	9	
Keeps rank; new post	0	0	0	10	18	14	25	16	22	13	21	13	
Intra-Party Change	31%	11%	24%	30%	11%	21%	30%	24%	28%	30%	15%	24%	
Changes post and rank	6	0	4	3	11	7	23	4	16	12	6	9	
Defeated on floor	6	0	4	0	0	0	0	0	0	2	0	1	
Defeated in caucus Replaced as elected	0	0	0	3	0	2	0	16	6	1	6	3	
leader	3	5	4	10	0	5	0	4	1	4	3	3	
Replaced as appointed leader	17	5	13	13	0	7	7	0	4	12	1	8	
Institutional Reform	0%	0%	0%	3%	4%	3%	2%	0%	1%	2%	1%	2%	
Post eliminated	0	0	0	3	4	3	2	0	1	2	1	2	
TOTALS							····						
Percentages	99%	100%	100%	100%	101%	100%	100%	100%	100%	100%	99%	100%	
N of cases	35	19	54	30	28	58	44	25	69	109 ′	72	181	

Note: Italicized percentages represent category subtotals.

whatever "progressive ambitions" they may have once had.

Interparty Turnover. This has been a constant factor in House leadership change. The mean deviation from the 29 per cent recorded for all three eras is a meager one per cent. However, the number of leaders who lose their party ranks during changes in party control of the House has dwindled greatly, from 28 per cent in the 1789-1863 era to 7 per cent in the post-1911 one. There are two major reasons for this change. First, the inability of the opponents of the Democrats to maintain control of the House during Period I insured a high turnover of their elective and appointive leaders as a result of election defeats. The second reason involves the development of a coherent minority following 1863. Leaders whose parties had lost control of the House now had a place to relocate their influence. In the present era, only two elected floor leaders (Kitchin in 1919 and Halleck in 1949 and 1955) lost their posts when loss of House control resulted in a constriction of the number of leadership positions available to their parties. The two appointed leaders in this era to lose their posts because of interparty turnover were Sereno Payne in 1911 and J. Percy Priest in 1953. Priest was removed from the whip post by Minority Leader Rayburn to make room for ex-Majority Leader McCormack. This move preserved McCormack's second-place ranking. One advantage of having appointed whips is that they cannot loudly protest such moves.

Institutional Reform. Concentrating the majority leadership in Ways and Means, Chairman Sereno Payne in 1899 eliminated Joe Cannon and the Appropriations Committee from the leadership for a short time. The other two instances involved the Democratic whip post which disappeared as a leadership position in 1909 and 1913. Since both disappearances of

this post were accompanied by changes in the elective leadership of the Democratic party, it would not be unreasonable to consider these to be intraparty changes.

Intraparty Changes. Of the various types of leadership change, none is more fascinating than intraparty change when a leader remains in the House and is moved within or removed from the hierarchy by the members of his own party. Often filled with intrigue and interpersonal machinations, these intraparty changes provide insight into the factions which exist within legislative parties. Changes within this context have shaped much of the writing about the House.

This category has five different manifestations: (1) interpositional mobility when a leader holding one post moves to another within the hierarchy; (2) defeat on the floor for the speakership when disaffected members of the leader's own party join with the opposition to deny a re-election; (3) a defeat in the caucus for any of the party's elective leaders; (4) replacement of an elective leader as a nominee without a formal denial of support; and (5) demotion to the ranks of the ordinary members of an appointed leader by an elected one. To these might be added the elimination by one leader of a post held by another, such as happened twice to Democratic whips.

Of the forty-three interparty changes in these top posts, seventeen were interpositional moves. The six pre-1911 changes included the moving of three Democratic appointed leaders into the speakership and of three Republican appointed leaders into other appointive posts.

The eleven interpositional cases in the present era involved one move by a Republican and ten by Democrats. The lone Republican to move was Majority Leader Nicholas Longworth who became Speaker in 1925. His succession was bitterly contested in the caucus, but he won on the first ballot with 62 per cent of the votes.³⁴ John Rhodes's uncontested elevation to the minority leadership in 1973 from the chairmanship of the Republican Policy Committee may portend a new development in the Republican patterns of leadership succession.³⁵ From the historical standpoint of this study, however, the chairmanships of the Republican

34The New York Times, February 28, 1925, p. 1. Longworth defeated Martin Madden of Illinois, 140 to 85 on the first ballot. Madden had been one of the two Republican candidates challenging Speaker Gillet's re-election on the floor during the previous Congress.

³⁵Congressional Quarterly Weekly Report, 31 (December 5, 1973), p. 3171.

Policy Committee and Conference as well as their counterparts in the Democratic Caucus and Steering Committees have not been clearly identified as leadership posts for a sufficient length of time to warrant their inclusion here. ³⁶ At some future time they may well qualify for this category.

Interpositional mobility in the highest posts generally meets with less opposition in Democratic caucuses. Only four of these moves were greeted with caucus opposition. Henry Rainey captured 60 per cent of the vote in his successful move from the floor leadership to the chair in 1933,37 and in 1971 Carl Albert received 92 per cent of the vote in his identical move.³⁸ The most serious challenge to an interpositional move was that faced by Hale Boggs in his successful contest to move from whip to floor leader in 1971. He won 57 per cent of the votes on the second ballot.³⁹ Although challenges were hurled at Byrns's 1934 succession to the speakership, 40 and Albert's and O'Neill's successions to the floor leadership in 1962 and 1973 respectively, they were withdrawn before the caucus vote. Four Democratic interpositional moves between the floor leadership and the chair were uncontested and unchallenged: Bankhead in 1936, Rayburn in 1940, McCormack in 1962, and O'Neill in 1976. The "leadership ladder" as it pertains to the floor leader-Speaker rung has enormous legitimacy in the Democratic caucus.

³⁶Ripley asserts that "Chairmen of various party committees (for example, Committee on Committees, Steering Committee, Policy Committee) or of the party caucus or conference have occasionally been prominent figures in the majority party in the House," in Majority Party Leadership in Congress, pp. 3–4 (emphasis added). Jones states that "Although they have existed on occasion in both parties, until the 1960s policy or steering committees were not heavily relied on by the minority party," in The Minority Party in Congress, p. 38.

³⁷Rainey defeated John McDuffie of Alabama who had served as whip under Garner. The vote was decided on the first ballot: Rainey's 166 to McDuffie's 112, *The New York Times*, March 3, 1933, p. 1. A fuller account of this contest may be found in Robert A. Waller, "The Selection of Henry T. Rainey as Speaker of the House," *Capitol Studies*, 2 (Spring, 1973), pp. 37–47.

38 Albert defeated John Conyers of Michigan 220-20, Congressional Quarterly Weekly Report, 29 (January 22, 1971), p. 176.

³⁹The final tally on the second ballot was Boggs, 140; Udall, 88; and Sisk, 17; in *Congressional Quarterly Weekly Report*, 29 (January 22, 1971), p. 176.

40The most serious threat to Byrns came from Sam Rayburn, but Rayburn withdrew after Pennsylvania announced for Byrns, C. Dwight Dorough, Mr. Sam (New York: Random House, 1962), p. 253. See also The New York Times, December 13, 1934, p. 17.

John McFall's fourth place showing in the 1976 floor leadership contest indicates that the whip-floor leader rung has less legitimacy. McFall's Korean troubles aside, a counter-trend has emerged among a segment of the Democratic membership which opposes the ladder concept. If caucus opposition to the "ladder" continues to grow, it is quite possible that interpositional mobility may decline as a major form of intraparty change.

Voted Floor Defeats. The most dramatic cases of intraparty leadership change occur when the sitting Speaker of the incumbent party is defeated in an open floor fight. Both cases occurred in the first period. 41 Since 1835 only Speaker Frederick Gillett faced defeat on the floor from fellow Republicans. It took nine ballots in 1923 to overcome this challenge. 42

Intraparty floor contests for the speakership disappeared with the institutionalization of party caucuses for both majorities and minorities following the Civil War.⁴³ Conflict was moved from the floor into the confines of the caucus room. Here conflict could be contained and a party which had arrived in Washington with a majority of the seats would not have to face the prospect of seeing its victory snatched away by renegades on the floor.

Voted Caucus Defeats. Newspaper accounts of party caucuses from 1863 to 1977 revealed that the sixty Republican caucuses held seventy-

⁴¹Both cases involved Philip P. Barbour of Virginia. He defeated Speaker John W. Taylor in 1821 and was later defeated for re-election as Speaker by Henry Clay two years later. See Niles' Weekly Register, 21 (December 8, 1821), p. 234 and 25 (December 6, 1823), p. 22, respectively. One unusual case which does not technically qualify for this category occurred in 1834 when John Bell, a Whig, was elected to preside over the 2nd Session of the Democratically-controlled 23rd Congress when the Democrats could not agree on a candidate. In the next Congress, the united Democrats elected James K. Polk over Bell. See Niles' Weekly Register, 46 (June 7, 1834), pp. 248 and 49 Niles' Weekly Register (December 12, 1835), p. 248 for the two contests.

⁴²This contest has been described earlier, see note 15, supra. See also, Paul D. Hasbrouck, Party Government in the House of Representatives (New York: Macmillan, 1927), p. 19 and The New York Times, December 6, 1923, pp. 1–2.

⁴³This may be seen in the decline of multiballot floor contests and the fact that the mean percentage of votes received by the top two speakership candidates since 1865 in fifty-eight regular and two special speakership elections has been 98.86 per cent. The lowest percentage was 93.4 in the 55th Congress (1899). The first ballot percentage in 1923 was 94.4 for Gillett and Garrett combined.

seven elections for these major posts, of which sixteen (21 per cent) were contested.⁴⁴ The Democrats in the same period held sixty-one caucuses with ninety-one elections. Twenty were contested (22 per cent).

Only one Democratic floor leader was defeated in the caucus: Samuel Randall, the minority nominee in the 47th Congress, was defeated on the first ballot by John Carlisle in his bid to become Speaker of the 48th Congress in 1883.⁴⁵ As a conciliatory gesture, Carlisle appointed Randall to chair the Appropriations Committee. Ironically, it was this Congress that Woodrow Wilson used to illustrate his observation that "all [House chairmen] are subordinate to the Chairman on Appropriations." So even the lone case of direct removal of a Democratic leader by the caucus had little discernible impact upon the party's power relationships.

Since then the only challenges to incumbent Democratic floor leaders occurred in 1969 and 1973. Speaker McCormack beat back Morris Udall's frontal assault 178 to 58 in the 91st Congress⁴⁷ and Speaker Albert thwarted a challenge from John Conyers and the Black Caucus in the 93rd.⁴⁸

Leadership challenges in the Republican caucuses are more successful. In 1919 Minority Leader Mann's bid for the speakership was stopped by Frederick Gillett who received twice as many votes from his fellow partisans (138-69).⁴⁹ Twelve years later following another change in House party control, John Q. Tilson, who had been elected three times to be

44This information is derived from the accounts of two newspapers, The New York Times and The Washington Post. The first contest reported by the Post occurred in 1879. A number of caucus nominations were simply reported with no mention of the number of members present or the votes cast. When this happened, I recorded it as "no reported contest" and assumed that the nomination had met with no opposition.

⁴⁵Randall was defeated by Carlisle, 106-52. Samuel Cox received 30 votes, *The New York Times*, December 3, 1883, p. 1.

46Woodrow Wilson, Congressional Government (New York: Meridian Books, 1956), pp. 82–83. This book was originally published in Boston by Houghton, Mifflin and Co., in 1885.

⁴⁷Congressional Quarterly Weekly Report, 27 (January 3, 1969), p. 2. Wilbur Mills received four votes.

48 Albert defeated Conyers by 202-25 in the 93rd Congress, Congressional Quarterly Weekly Report, 31 (January 6, 1973), p. 5. Conyers's opposition to Albert in 1971 was due to the Speaker's lack of support for an effort to strip three Mississippi members of their seniority. In 1973 he tried for support beyond the Black Congressional Caucus.

⁴⁹The New York Times, February 28, 1919, p. 1.

the party's majority leader, had his floor leadership endorsement revoked by the caucus when the party organized itself as a minority rather than as the majority which it had anticipated earlier. Eight ballots transpired before Bertrand Snell of New York was victorious. 50

The two best-known recent cases of leadership change in the Republican caucus were the defeats of Joe Martin by Charles Halleck in 1959 and of Halleck by Gerald Ford in 1965.⁵¹ Both contests were close, but both were decided early: two ballots in 1959 and one in 1965.

Nonvoted Removals. Similar to defeat in the caucus is removal as the nominee in the subsequent Congress. The leader is back in the House, but his party has nominated someone else to lead it without a formal vote on the floor or in a party caucus. A case in point is the replacement of Speaker Nathaniel Macon by Joseph Varnum, a fellow Democratic-Republican, in 1807. Macon had fallen out with President Jefferson over Macon's continuance of John Randolph as Chairman of the Ways and Means Committee during his three terms as Speaker. Students of the period suggest that Macon stayed away from the balloting on the opening day of the Tenth Congress to avoid an outright rebuff on the floor.52 A less clear example of this bypassing occurred in 1791 when Speaker Frederick Muhlenberg was not in contention to succeed himself.53 However he was elected in the subsequent Congress to preside over the first Democratic-Republican majority. The other three cases involve Democratic minority nominees who were not renominated by the party caucus in the following Congress (Marshall in 1869, Kerr in 1871, and Wood in 1875). Wood wanted to be renominated but he withdrew when he saw that his strength in the caucus was slim. ⁵⁴ Kerr handled credentials contests in the 1871 caucus, but his name was never offered for a formal vote. ⁵⁵ Marshall's leadership career disappeared without a trace.

The lone post-1911 case of an elected leader being replaced without a vote involved a Republican leader, Harold Knutson. He had been elected whip in 1919 by the caucus and in 1921 by the Committee on Committees, but he had broken with the leadership and voted for the insurgent speakership candidate in the 1923 floor fight. Needless to say, he was replaced as an elected leader. Including this nonvoted defeat with the voted ones of Mann, Tilson, Martin, and Halleck brings to five the number of "removals from below" of leaders by members of the Republican caucus in the 1911–1977 era. No such removals occurred in the Democratic caucus.

Demotions

Demotion of appointed leaders represents "removal from above." In these cases, the top-ranking party leader simply replaces an

⁵⁰Snell's nomination was made unanimous on the eighth ballot. His tally over Tilson on the seventh ballot was 96 to 64, *The New York Times*, December 1, 1931, pp. 1 and 4.

51 Accounts of the Halleck-Martin contest may be found in Martin's own book, My First Fifty Years in Politics (New York: McGraw-Hill, 1960), pp. 12–19; and in Charles O. Jones, Party and Policy-Making, pp. 33–42. The Ford-Halleck struggle is covered by Robert L. Peabody in The Ford-Halleck Minority Leadership Contest, and in "Political Parties: House Republican Leadership," in American Political Institutions and Public Policy: Five Contemporary Studies, ed. Allan P. Sindler (Boston: Little Brown & Co., 1969), pp. 181–229.

52Noble E. Cunningham, Jr., The Jeffersonian Republicans in Power: Party Operations, 1801–1809 (Chapel Hill: University of North Carolina Press, 1963), p. 88. Macon was a candidate for Speaker in the 11th Congress and lost to Varnum on the second ballot, 45 to 65, Annals of Congress, 11th Congress, First Session, p. 56. Macon's relationship with Randolph is described by Cunningham on pp. 77, 86–88, and 230.

53Muhlenberg was apparently not a contestant in the Second Congress. According to a letter from Elbridge Gerry to his wife, the two leading candidates were Jonathan Trumbull of Connecticut who had lost to Muhlenberg in the previous Congress and John Laurence of New York. The letter was dated October 24, 1791 and may be found in the Russell W. Knight Collection of Elbridge Gerry Papers in the Massachusetts Historical Society. Special thanks are extended to Professor Patrick J. Furlong of the Department of History of Indiana University at South Bend for this reference.

54Wood withdrew a week before the balloting amidst speculation that he was trying to arrange an appointment to the chairmanship of Ways and Means, which was not forthcoming, *The New York Times*, December 1, 1875, p. 1. A fuller account of the 1875 election may be found in Albert V. House, "The Speakership Contest of 1875: Democratic Response to Power," *Journal of American History*, 52 (September, 1965), pp. 252–274.

55The New York Times, March 3, 1871, p. 1.

⁵⁶Knutson was described as a "Cannon pupil" at the time of the 1919 whip contest, *The New York Times*, March 12, 1919, p. 1. Four years later he voted for the leading insurgent candidate in the caucus, Henry A. Cooper of Wisconsin, *The New York Times*, December 2, 1923, p. 1. The Republican Committee on Committees gave the post to Albert Vestal of Indiana, see *The Washington Post*, December 13, 1923, p. 5.

appointive leader. There generally is little fanfare surrounding these post departures because the power to hire (i.e., appoint) carries with it the inherent power to fire. The first demotion occurred in 1800 when Federalist Speaker Theodore Sedgwick removed Robert Goodlow Harper from the Ways and Means chairmanship. Harper had been initially appointed by Speaker Dayton and had been continued by Sedgwick, but before the end of the session, Sedgwick concluded that he was a man "whom no one can control," and removed him. ⁵⁷ This is the only case of a non-Democratic leader being demoted.

In the 1789-1863 era, Democratic Speakers demoted six chairmen of Ways and Means.⁵⁸ In the next era, 1863-1911, four Democratic appointed leaders were removed by elected ones.⁵⁹

The pattern of Democratic demotion has continued into this present period. Speaker Rainey demoted John McDuffie from the whip post after the latter had failed in his bid to defeat Rainey for the speakership nomination in 1933.⁶⁰ Two years later, Speaker Byrns removed Rainey's whip, Arthur Greenwood, and replaced him with Patrick Boland of Pennsylvania. Boland held his office during two speakership changes, Bankhead's succession in 1936 and Rayburn's in 1940. He was the last Democratic whip to remain in his post when the elected leaders above him changed.

If one includes the cases of the two Democratic whips whose posts disappeared in 1909 and 1913, and of J. Percy Priest who was not reappointed whip by Speaker Rayburn when the Democrats regained control of the House in 1955,61 as well as O'Neill's non-reappointment

⁵⁷Richard E. Welch, Jr., *Theodore Sedgwick, Federalist: A Political Portrait* (Middletown, Connecticut: Wesleyan University Press, 1965), p. 211n.

58John Randolph in 1807, Ezekiel Bacon in 1812, William Lowndes in 1819, George McDuffie in 1831, Thomas Bayly in 1851, and J. Glancy Jones in 1858. Both Bacon and Jones were removed between sessions of a Congress. Serious questions existed about their fitness for floor leadership, Alexander, History and Procedure of the House of Representatives, pp. 124-125.

⁵⁹Two chairmen of Ways and Means, William R. Morrison in 1877 and William Springer in 1893; one chairman of Appropriations, William S. Holman in 1893; and one whip, Oscar Underwood in 1901 were removed from their appointive posts.

⁶⁰Waller, "The Selection of Henry T. Rainey," p. 45.

61Ripley reports that Priest "decided not to continue as whip in 1955 because he had become Chairman of the Committee on Interstate and Foreign Commerce," in "Party Whip Organizations" in Peabody and Polsby, 2nd ed., p. 208n.

of John McFall, then there is a total of sixteen demotions of appointed Democratic leaders by the party's elected leaders.

Clearly, this is a power which Republican leaders do not possess. The independently elected Republican whips may lack opportunities to gain the top slot, but they have been protected from those "above" them. A ready example of this appeared in 1965 when Gerald Ford, then a newly elected minority leader, backed Peter Frelinghuysen as his candidate for whip against Leslie Arends, who had held the post for twenty-two years. Frelinghuysen was soundly defeated in the caucus, and Minority Leader Ford quickly became aware of the limits of power in the Republican leadership. 62

Even when the elected Republican leaders had appointive posts under their control (1859–1917) they were reluctant to exercise their authority. None of the eighteen appointed Republican floor leaders was removed. This reluctance extended to other appointed officials as well. In the years from 1881 through 1911, the Republican speakers "violated" seniority in chairmanship selections much less often than the Democratic speakers did (43 per cent to 60 per cent). Greater unity and organization combined to minimize the number of rolling heads following each change in the elected officers of the Republican party.

Summary. The patterns of leadership change and continuity reveal little variation over time in the proportions of House leaders leaving their posts under the three broad categories of: career-related departures, interparty turnovers, and intraparty changes. Within these categories, however, there are important trends toward institutionalization as personal career-related departures have increased and politically related departures have decreased. The House leadership career is now an entity unto itself and has declined as a springboard to other offices. In addition, House leaders have more to fear from illness and old age than they do from their constituents. The ratio of electoral defeats to deaths in the pre-1911 periods was 6:1, but the ratio has been reversed since then with a 1:5 relationship between defeats and deaths.

Post continuations in the years since 1911 outnumber post changes for both parties,

62See Arthur Krock's article, "Rep. Ford's Defeat," in *The New York Times*, January 17, 1965, IV, 13. Arends defeated Frelinghuysen 70-59. *The New York Times*, January 15, 1965, p. 1.

63These figures are based upon a recomputation of Table 10, "Violations of Seniority by Speaker, 1881-1910," in Polsby, Gallaher, and Rundquist, p. 799

another indication of institutionalization. Also the development of the minority leadership and whip positions has made it possible for leaders to retain their party ranks regardless of their party's success (or lack of it) at the polls. This has led to greater professionalization within these posts.

As important as the time-related institutionalizing trends may be, there appear to be more important interparty differences in the patterns of leadership change and continuity. Three of these relate directly to intraparty change. Democratic leaders change posts more frequently and do so with little opposition from the caucus. Republican leaders change posts infrequently and the only interpositional move by a Republican in this century was met with caucus opposition. Democratic leaders face infrequent and impotent opposition in their caucuses, while Republicans face similarly infrequent, but highly potent opposition in theirs. And finally, because Democratic leaders retain the power to appoint leaders under them, they can also remove them with relative impunity. Elected Republican leaders did not remove appointive leaders when they possessed the authority to do so and now they possess neither the power of appointment nor the power of removal. They must content themselves with whomever the caucus chooses to assist them in the leadership.

The House parties do differ in their patterns of intraparty leadership change. Combining their separate patterns to generate a picture of House "norms" in the furtherance of the institutionalization thesis may lead to valuable conclusions but it may also lead one to overlook party differences.

Establishing these important party differences indicates that institutionalization theories should maintain the party variable in future explorations of leadership change, but it does not bear on the important contemporary statement regarding the impact of majority or minority statuses upon leadership change. In the next section of this paper, these variables will be directly examined.

The Impact of Majority and Minority Statuses upon Leadership Contests

In focusing wholly upon majority vs. minority statuses, it becomes necessary to confine the analysis to the later two periods. No discernible minority exists before 1863, and since thirteen of the forty reported speakership contests went beyond a single ballot, it is also hard to discern a majority. In many of the speakership contests in that era the major opponent to the eventual victor was a fellow member of his own party. Consequently, the terms "majority" and "minority" do not have as much meaning as they do in the two most recent ones.

The literature on leadership change suggests that there should be greater turnover among minority leaders because the frustration of losing often takes the form of "scapegoating" the leadership. 64 Regardless of the leaders' inability to affect the outcome of congressional elections throughout the nation, it is presumed in the literature that the leaders must face the consequences of an electorate dissatisfied with the party's House candidates. This contention is examined in Table 5 for Periods II and III.

Note that the mean frequencies in Table 5 are generally lower by period than those in Table 3, which dealt with frequencies by party. The reason is that majority or minority leadership is not the mutually exclusive phenemonon which party leadership has been since 1827. Eighteen of the eighty-two post-1863 leaders (22 per cent) served their parties under both conditions.

The general expectation that leadership selection frequencies would be lower for minorities than for majorities is borne out, but the sizes of the differences are similar to those presented in the interparty comparisons. The lack of mutual exclusivity is one reason, but not the sole reason for this similarity.

64Peabody, "Party Leadership Change," p. 688. Joe Martin certainly felt that the size of the 1958 defeat had cost him the minority leadership, see his book, My First Fifty Years in Politics, pp. 4-5.

Table 5. Majority-Minority Differences in Mean Selection Frequencies

Periods	Majority		Minority		MaiMin.	All Leaders	
	Mean	N	Mean	N	Difference	Mean	N
II, 1863–1911	2.44	32	1.63	19	+.81	2.59	42
III, 1911–1977	3.45	31	3.29	21	+.16	4.09	43
Totals	2.98	62 ⁸	2.56	39a	+.42	3.48	828

⁸Adjusted for overlapping periods.

The greatest difference in this table appears among the leaders during Period II. A closer examination of this difference reveals an important party factor. None of the ten Democratic minority nominees from 1863 through 1901 was renominated. This contrasts directly with the nine minority nominations made by the Republican party in this same span. The Republican minority caucuses nominated Reed four times and Garfield three times. The other two Republican nominees, Blaine and Keifer, left the House after their single selections in the minority. This strengthens the assertion that the Republican party had an effective House organization before the post-Civil War Democrats did. Even when they were in the minority, the Republicans had a clearer sense of which of their members they wished to nominate as Speaker. Only two of their nine minority nominations for Speaker in Period II were contested in the caucus.⁶⁵ Once again a party contrast emerges. The Democrats had caucus contests for four of their seven minority nominations from 1871 through 1901, one of which took six ballots to decide.66

Focusing only upon the elective leaders reveals a sizeable difference in Period II's majorities and minorities (2.50 - 1.53 = +.97), but an inconsequential one between Period III's elective leaders (3.68 - 3.86 = -.18). The lack of any meaningful difference in this category represents a serious challenge to much of the contemporary wisdom concerning the impact of party status upon leadership change. It is possible, however, that the use of averages to examine this proposition may be insufficient.

A more direct test of the minority frustration hypothesis would be to examine only the leadership selections voted on by the membership and to see if caucus dissension appears more often among minorities than among majorities. The first step in this test involves eliminating ninety-five appointive and twenty-three semi-elective selections from the total of 285 recorded in the two later periods. This leaves 167 leadership selections made by the party membership. To this may be added the second majority leadership election in the 1919 Republican caucus. 67 Ninety-one of these elec-

tions involved Democrats, and seventy-seven involved Republicans; 100 occurred when a party was in the majority and sixty-eight when the party was not. This set of data seems sufficiently large to test the impact of party status upon the House leadership.

The dependent variable to be used in this analysis will be the frequency of caucus contests as reported in contemporaneous newspaper accounts. The newspapers report contests in thirty-six of the 168 elections (21 per cent). Often the news account the following day makes no mention of either a contest or a nomination by acclamation. Since every contest referred to in the House literature may be found in the news accounts plus a number which have eluded previous analysis, I am willing to assume that the absence of a reported contest means that none occurred. The precaucus barrage of news stories surrounding genuine contests makes it seem unlikely that an account would overlook any manifestation of conflict.

In three cases, selections were made without caucuses. Two occurred at the opening of the 40th Congress, which convened a day after the 39th adjourned. Each party rallied around a candidate, and the two men received every vote cast in the House for speaker between them.68 The other case occurred in 1863 when the Democrats held a caucus, but made no nominations because of their internal divisions over the Civil War. Samuel Cox, the leading Democrat in the speakership balloting on the floor, received forty-two votes from the seventy-five Democrats who had been elected to that Congress.69 This degree of obvious dissension within the party justifies adding this event to the caucus contests, bringing the total to thirty-seven.

The 168 intraparty elections are compared in Table 6 by party and era for the relative frequency of contests within differing party statuses. Correlations are also presented to determine if the generally accepted proposition concerning the greater frequency of leadership contests in the minority is borne out.

In every case but one in Table 6, the data disconfirm the proposition that minority status engenders leadership contests. In three of the four period and party comparisons, contests are more prevalent in the majority party. The only case which supports the proposition - i.e., the

⁶⁵The two Republican minority contests occurred in 1883 and 1885.

⁶⁶The four Democratic minority contests occurred in 1871, 1873, 1897, and 1899. The last contest took six ballots before James D. Richardson of Tennessee defeated David DeArmond of Missouri, 90 to 47, The New York Times, December 3, 1899, p. 2.

⁶⁷ James R. Mann was elected majority leader by the caucus after his defeat for the speakership nomination. He refused the post and Frank Mondell of

Wyoming was elected. The New York Times, March 12, 1919, p. 1.

⁶⁸The New York Times, March 5, 1867, p. 1.

⁶⁹House Journal, 38th Congress, First Session, p.

Table 6. Frequency of Intraparty Contests for House Elective Posts: By Period and Party Status, 1863-1977

	1863-19	11	1911-19	77	1863-19	77	
Party and Status	Contests/ Caucuses	%	Contests/ Caucuses	%	Contests/ Caucuses	%	
Democratic Majority	6/9	67	10/57	18	16/66	24	
Democratic Minority	5/17	29	0/8	0	5/25	20	
Democratic Totals	11/26	42	10/65	15	21/91	23	
Gamma	655 ^a		-1.00	00	123		
Republican Majority	3/16	19	6/18	33	9/34	26	
Republican Minority	2/9	22	5/34	15	_7/43	16	
Republican Totals	5/25	$\overline{20}$	11/52	$\overline{21}$	16/77	21	
Gamma	+.106		.106 –.487		29	9	
All Majorities	9/25	36	16/75	21	25/100	25	
All Minorities	7/26	27	5/42	12	12/68	18	
Totals	16/51	31	$\frac{21/11}{7}$	18	37/168	22	
Gamma	208		33	5	217		

Note: In none of the comparisons does statistical significance emerge at the .05 level.

case of the Republicans from 1863 to 1911 — represents the smallest percentage differential in the table. And when all majorities are compared to all minorities, regardless of party, the incidence of majority contests is higher in each period.

One explanation for this finding is that the stakes are higher in the majority party than in the minority. As Peabody describes the difference:

There are more committee assignments and appointments to prestige boards and commissions to be distributed. It is the majority which receives most of the credit when legislation is passed. Their projects receive higher priority. Majority members chair the committees and subcommittees. With position comes staff, superior access to executive officialdom and greater influence on legislative outcomes. 70

Also in Table 6, an intriguing interparty difference appears with regard to the incidence of these contests over time. Republican caucuses had a slight increase in dissension in Period III, while the Democratic caucuses have shown a marked decrease between the two eras. This decline in contested Democratic leadership elections is most likely due to the conscious efforts of the leadership itself to minimize conflict and to institutionalize the party's succession system. It appears as if the initial selections for the Democratic leadership ladder

are designed to appeal to the widest possible group of members.⁷¹

There is support for this inference in the congressional voting literature. David Truman's study of the 81st Congress (1949–1951) indicated that the floor leaders in both House parties tended to be ideological "middlemen." But Barbara Hinckley's analysis of more recent Congresses reveals that only the Democratic leaders continue to occupy this location. In these Congresses, the Democratic leadership stood astride the gulf between a liberal-moderate membership and a brace of conservative committee chairmen, while the Republican leadership was more conservative than its membership and ranking committeemen. Whether this was true in the pre-World

71A case in point is that of "Tip" O'Neill whose widespread popularity made his move from appointed whip to elected floor leader without opposition. Peabody reports that Congressman Sam Gibbons of Florida, who had hoped to challenge O'Neill, told him, "Tip, I can tell you something that nobody else in this room can. You haven't got an enemy in the place.'" Leadership in Congress, p. 258.

72 David B. Truman, The Congressional Party: A Case Study (New York: John Wiley & Sons, 1959), pp. 205–208. A recent study suggests that the "middleman" role is adopted following the selection as leader rather than being a reason for the selection itself. See William E. Sullivan, "Criteria for Selecting Party Leadership in Congress: An Empirical Test," American Politics Quarterly, 3 (January, 1975), 25–44.

⁷³Hinckley, "Congressional Leadership Selection and Support," pp. 281–284.

^aThe direction of the signs tests the proposition that contests will be less frequent in majority status situations.

⁷⁰ Peabody, "Party Leadership Change," p. 687.

War II Congresses is an important question beyond the scope of this study.

Leadership Change Typology. In order to add the dimension of incumbency to the study, the data have been placed into Robert Peabody's typology of "intraparty leadership change."⁷⁴ Peabody's typology identifies six possible change situations: (1) status quo; (2) routine advancement; (3) appointment or emergence of a consensus choice; (4) open competition; (5) challenge to an heir apparent; and (6) revolt or its aftermath. Two of Peabody's situations —

74Peabody, "Party Leadership Change," pp. 681-686. The fullest presentation of this typology may be found in *Leadership in Congress*, pp. 266-294.

(2) and (5) — depend upon the existence of established succession patterns for their meaning. Since the Democrats appear to have an orderly succession system and the Republicans do not, these two situations lack interparty comparability and will not be included in this analysis.

Using Peabody's other two criteria — vacancy or no vacancy in the leadership post, and a contest or no contest in the caucus — a simple four-fold typology can be created from Peabody's original one. This typology is used in Table 7 to classify the 168 floor leadership elections in the House since 1863.

Operationally, status quo refers to the unopposed continuance of a leader in his party leadership rank, regardless of party control

Table 7. Frequencies of Intraparty Leadership Situations by Period, Party Status, and Post Occupancy, 1863-1977

	Occupied	Post	Vaca	int Post	To	Totals		
	Status Quo	Revolt	Consensus	Competition	N	%		
1863-1911								
Democratic majority Democratic minority Democratic totals	$\frac{33\%}{41}$	$\frac{44\%}{0}$	$\frac{0\%}{\frac{20}{19}}$	22% 29 27	$\frac{9}{\frac{17}{26}}$	99 100 99		
Republican majority Republican minority Republican totals	56 67 60	$\frac{12}{11}$	$\frac{25}{11}$	6 11 8	$\frac{16}{\frac{9}{25}}$	99 100 100		
Both majority Both minority Both totals	48 50 49	24 4 14	$\frac{16}{23}$	12 23 18	25 26 51	$\frac{100}{101}$		
1911–1977						, <u>.</u>		
Democratic majority Democratic minority Democratic totals	$\frac{61\%}{62}$	4% -0 -3	$\frac{21\%}{\frac{38}{23}}$	$\frac{14\%}{\frac{0}{12}}$	57 8 65	$\frac{100}{100}$		
Republican majority Republican minority Republican totals	44 <u>76</u> 65	$\frac{17}{\frac{9}{12}}$	$\frac{22}{\frac{9}{13}}$	$\frac{17}{\frac{6}{10}}$	18 34 52	$\frac{100}{100}$		
Both majority Both minority Both totals	57 74 63	7 7 7	21 14 19	$\frac{15}{5}$	$\frac{75}{42}$ $\overline{117}$	$\frac{100}{100}$		
1863-1977					· · · · · · · · · · · · · · · · · · ·			
Democratic majority Democratic minority Democratic totals	58% 48 55	9% 0 7	$\frac{18\%}{32}$	15% 20 16	66 25 91	$\frac{100}{100}$		
Republican majority Republican minority Republican totals	50 74 64	$\frac{15}{9}$	24 9 16	$\frac{12}{\frac{7}{9}}$	34 43 77	$\frac{101}{99}$		
All majority All minority All totals	55 65 59	$\frac{11}{6}$	20 18 19	14 12 13	$\frac{68}{168}$	$\frac{100}{101}$		

changes. A Speaker becoming minority leader after his party has lost the House represents a continuation of his party's leadership status quo. Consensus choices represent new moves into ranks which are unopposed by the caucus. Also included in this category are unopposed moves by appointed Democratic whips into the elected majority leadership post following changes in party control (e.g., McCormack in 1949 and 1955). The rank remains the same, but dependence upon caucus approval has increased.

Contests occur in two cases: competition when there is a vacancy in the leadership rankings, and revolt when there is no vacancy and when opposition to the incumbent leader manifests itself in actual caucus votes against him.

The three instances in Period II in which Democratic minority speakership candidates remained in the House but were replaced as nominees without voted defeats have been classified on the basis of each leader's caucus activity. Since neither Marshall in the 41st Congress nor Kerr in the 42nd Congress figured in the leadership contests, these cases were treated as vacancies. In the 44th Congress (1875–1877), however, Fernando Wood, the previous speakership nominee, contended until his support evaporated in the final week before the vote, that election is considered a revolt.

Focusing first upon the 1863-1911 data in Table 7, we see that contests occurred at a higher rate in Democratic caucuses regardless of the status of the party or the incumbency of its leaders. Democratic disarray was very pronounced throughout most of this era, particularly during the party's majorities from 1875 through 1885. All four majorities in that period opened with leadership revolts. Two were successful - the replacement of Wood in 1875 and the voted defeat of Randall in 1883; two were unsuccessful - Randall's two first-ballot victories in 1877 and 1879.75 This contrasts with the failures of the three Republican revolts against Keifer in 1883, Reed in 1889, and Cannon in 1909.76

75 Randall was renominated in 1877 with 75 per cent of the votes on the first ballot, *The New York Tribune*, October 15, 1877, p. 1; and in 1879 with 53 per cent on the first ballot, *Washington Post*, March 18, 1879, p. 1.

⁷⁶Keifer defeated George D. Robinson of Massachusetts, 44 to 15, *The New York Times*, December 2, 1883, p. 1. Six years later Reed received 85 votes of 166 cast in a second ballot victory, *Washington Post*, December 1, 1889, p. 1. Cannon received 162 votes of 187 cast on the first ballot in 1909, *The New York Times*, March 14, 1909, p. 1.

In both parties, contests for occupied offices were more frequent in majority situations. Six of the seven revolts occurred during majorities. This lends further support to the contention that when the stakes are higher competition will be intensified. Such a finding is not unexpected in an era when the Speaker of the House had sole control over the naming of sixty-plus committee chairmen.⁷⁷ Many House careers prior to 1911 were affected by members' decisions about which speakership candidate to back.

Regarding minorities, Republican ones were slightly less competitive than Democratic ones (22 per cent to 29 per cent). As mentioned earlier, however, stability in the Democratic minority was an outgrowth of the consolidation of the post-1899 leadership of Richardson, Williams, and Clark. These three leaders accounted for six of the twelve uncontested minority nominations in this era. Prior to that time, Democratic minority contests were almost as frequent as majority ones.

The post-1911 data indicate a marked decline in the frequency of Democratic contests regardless once again of the party's status or its leaders' incumbency. Only two revolts occurred during forty-two incumbency situations (5 per cent): Udall's in 1969 and Conyers's in 1973. No contests occurred during Democratic minorities in this era. Even the three successions of Claude Kitchin in 1921, Finis Garrett in 1923, and John Garner in 1929 were uncontested.

Of the eight instances of competition in the Democratic caucus for open posts, all occurred during majorities, and five were decided on the first ballot. The 1935 Bankhead-O'Connor and 1971 Boggs-Udall contests went to a second ballot, while the Wright-Burton election of 1976 had three ballots. Competition within Democratic caucuses in this era has not been protracted.

Among the post-1911 Republicans, revolts occurred more frequently and were more potent. Three of the six resulted in leadership overthrows — Mann in 1919, Martin in 1959,

77During the second period the number of committees increased from 38 in 1863 to 61 by 1905, the highest figure ever recorded for the House. See Lauros G. McConachie, Congressional Committees (New York: Thomas Y. Crowell and Co., 1898), pp. 349-358 and various issues of the Congressional Directory. As a measure of the care with which committees were selected, the Speakers in the twenty-five congresses from 1857-1907 averaged 42.2 days in making their final committee appointments, Hinds Precedents of the House of Representatives of the United States (Washington, D.C.: U.S. Government Printing Office, 1907), IV, p. 891.

and Halleck in 1965; while two of the others shortened leadership careers. Mann refused the majority leadership after another contest in 1919 and Gillett left the House upon completing his third (and vigorously contested) speakership in 1925. Only Leslie Arends withstood the revolt against him in the caucus and remained in office during subsequent Congresses. Republican leaders apparently heed the messages of their caucuses.

In cases of a vacancy, Republicans are more willing to have a contest than are Democrats (42 per cent to 35 per cent). As in the case of the Democrats, however, most open Republican contests were settled on the first ballot with only the eight-ballot Snell-Tilson fight an exception.

Contests have declined in the most recent era regardless of the occupancy of the leadership post. Contests for occupied posts decreased from 22 per cent in Period II to 10 per cent in Period III, while those for vacant posts decreased from 47 per cent to 37 per cent. The decrease, however, has been confined to the leadership of the Democratic party. Contests for occupied Democratic posts have declined by 24 percentage points (from 29 to 5 per cent) and contests for vacant ones have dropped by 23 points (from 58 to 35 per cent). Republican leaders have not been as fortunate; the rate of contests for occupied Republican posts has declined slightly (from 17 to 15 per cent) but the number of contests for vacant ones has increased sharply (from 29 to 42 per cent) in the post-1911 era.

Two party-minimizing explanations account ' for the general decline in contests. The first would be the institutionalization of the House leadership role. Greater acceptance of the authority of the leaders would presumably explain an increased reluctance of members to challenge either the sitting leadership or its anointed successors. The other explanation would stress the shift in power from the floor leaders to the committee chairmen in the years following the revolt against Speaker Cannon. The post-1911 diffusion of power has lowered the stakes involved in floor leadership contests and has thus led to a minimization of conflict. As plausible as each of these explanations may be, the fact that the decline in contests has been limited to Democratic caucuses limits their utility.

Types of Majorities and Minorities. One very important interperiod difference emerges from the data in both tables. The Democrats had a higher proportion of contests in Period II

regardless of their party status or office occupancy. In the subsequent period, the Republicans had a greater frequency of contests in these categories. One factor which characterizes both the Period II Democrats and the Period III Republicans is that both had difficulty holding control of the House. The Period II Democrats held the House for three spans totaling sixteen years (an average of 5.3 years per span). The Period II Republicans were more successful. They held control of the House during four spans totaling thirty-two years (an average of 8.0 years). In Period III, the Republicans averaged 5.3 years per span (16/3) while the Democrats averaged 13.0 years (52/4). Thus, it is possible that the certainty (or lack of it) of gaining and keeping control of the House may have an impact upon the frequency of leadership contests.

Some support for this proposition appears when the Democratic party leaders are analyzed. From 1863 through 1899, the Democrats had eleven contests for the leadership. During this time, they emerged from near-extinction as a party to become a very competitive force in the House. Despite their frequent majorities in the House, however, only once did they control the Senate and the presidency at the same time. Certainly, this was a continual reminder of their tentative status even when they held the House. They were an uncertain minority at this time, and leadership contests were one way of gaining access to the perquisites of power. Following their sizeable defeats in the congressional elections of the late 1890s they became a secure minority. Even their consecutive victories in the House elections of 1910-1916 were marred by the fact that their presidential standard-bearer had gained the White House twice with less than half of the votes. Their majority was insecure. In keeping with the expectation that contests will be infrequent for such insecure majorities, as for secure minorities, there were no contests for the twenty-two elected Democratic leaders from 1901 through 1931.

Following Roosevelt's landslide victory in 1932 and the ever-mounting number of House Democrats in subsequent elections, contests for the leadership reappeared. Five of the eleven elected Democratic leaders were challenged in the caucus between 1933 and 1940. The next quarter-century, 1942–1967, was free of contests, largely because of the Rayburn-McCormack power combine. The Democratic party's majority throughout segments of this period, however, was not as secure as it had been in the 'thirties nor as it has been in the past decade. Five of the ten leadership selections

since 1969 have been contested and the caucus defeats for committee chairmanships⁷⁸ lend substance to the "secure majority" thesis as an explanation.

The Republican pattern is not as clear. Four of their contests occurred in one eight-year span between 1881 and 1889 during which they alternated with the Democrats in controlling the House. Whether or not they were an insecure minority at the time is difficult to determine. Their next spate of contests appeared in the 1919-1925 period when they contested six leadership elections as a majority. One can speculate that having two presidential landslides behind them, they had become a secure majority which could afford to allow intraparty contests. But the proposition fails when the 1863-1875 and the 1895-1911 spans of Republican control are examined. No contests occurred in the first period, and only one (a minor challenge in 1909) occurred in the second. Secure Republican majorities do not always unleash a multitude of leadership contenders.

In one further effort to test the impact of party status upon House leadership change, the "declining minority" hypothesis will be explored. This proposition, advanced by Peabody, holds that severe losses in congressional elections will enhance the likelihood of minority party challenges.⁷⁹ Peabody's initial assessment was based upon his analysis of Republican revolts in the 84th-89th Congresses (1955-1966). When pushed back in time this proposition has some validity for the Republican party. In addition to the Martin and Halleck overthrows, there is the replacement of ex-Majority Leader John Q. Tilson by Bertrand Snell in 1931 after the Republicans lost control of the House and the challenge to ex-Speaker J. Warren Keifer in 1883 after a similar loss. Keifer was the only Speaker to be challenged for renomination in the caucus following hisparty's defeat at the polls.80

78The Democratic caucus in 1975 ended the chairmanship career of Representatives Wright Patman of Banking and Currency, W. R. Poage of Agriculture, and F. Edward Hebert of Armed Services. Two subcommittee chairmanships were also voided. Congressional Quarterly Weekly Report, 33 (January 18, 1975), pp. 114-118; (January 25, 1975), pp. 210-212; and (February 1, 1975), p. 275. The most recent victim was Robert L. Sikes, who was stripped of his chairmanship of the Military Construction Subcommittee on January 26, 1977, Congressional Quarterly Weekly Report, 35 (January 29, 1977), p. 159.

79Peabody, "Party Leadership Change," pp. •692-693.

⁸⁰Keifer had become a serious embarrassment to many of his fellow House Republicans because of his

Looking at the Democrats, one sees a different picture. There have been ten instances since 1863 when the Democrats have convened as a minority after having lost either twenty-plus seats or 10 per cent of their membership from the previous Congress. No Democratic leader who remained in the House and stood for renomination following these defeats was either overthrown or challenged in the caucus. Electoral success may have an impact upon the amount of discomfort which Democratic leaders face but electoral defeat apparently has none.

From this interpretation of the data, it appears that party status — be it majority or minority, secure, insecure, or declining — does not consistently explain the frequencies of House leadership contests and changes. The failure of party status as an explanatory variable forces a return to the concept of party distinctiveness: Democrats select their leaders differently than Republicans do.

Summary and Implications

The most direct finding in this study is that the two parties differ significantly in their patterns of House leadership change. Consequently, theories of House development which minimize the party variable in quest of institutional norms and electoral status predictors may slow our understanding of the dynamics of leadership change.

It is important to note however that institutionalized norms have appeared. Over time the number of House organizational posts has steadily increased as the leadership has become more bureaucratized. The increased importance of minor elective posts in the Republican organization (e.g., the chairmanships of the Republican Conference and Policy Committee) and the emergence of new appointive posts in the Democratic party (e.g., the deputy whips) indicates that this trend will continue.

intemperate attacks on the press and reports of nepotism in the staffing of his office. (The New York Times, March 7, 1883, p. 1). On the eve of the vote for Speaker of the 48th Congress he rebuffed suggestions that he not run for the Republican nomination. He won the caucus nomination, but only 59 of the Congress's 118 Republicans attended, and 15 of them voted against him. The New York Times, December 2, 1883, p. 1.

⁸¹Congresses in this category include: the 39th (1865-1867), 43rd (1873-1875), 54th (1895-1897), 59th (1905-1907), 66th (1919-1921, 67th (1921-1933), 69th (1925-1927), 71st (1929-1931), 80th (1947-1949), and the 83rd (1953-1955).

Other signs of institutionalization may be found in the increased selection frequencies for House leaders and the greater incidence of continuations either in specific posts or party ranks during this latest House era. These developments contribute to the greater professionalization of the House leadership career, as does the decline in the number of departures arising from the leaders' own extra-House ambitions and the vagaries of their constituents.

It is the interparty differences, however, which hold the most meaning, for these differences appear to be related to fundamental questions of each party's social composition and governing philosophy. In this analysis of leadership change over time, the following observations were made for the Democrats: they have a higher proportion of appointed leaders than Republicans; their leaders moved between posts within the party hierarchy in an ordered succession; their appointed leaders are often "removed from above" by their elected ones; and their elected leaders are subjected to infrequent and unsuccessful contests in their caucuses. Republicans, on the other hand, were found to rely more upon election to select their leaders; their rate of interpositional mobility among leaders is very low; their appointed leaders were never removed by the elected ones: and while their leaders face contests at the same rate that the Democrats do, the incidence of successful challenges is much greater in their caucuses. Republican leaders, in short, are often "removed from below."

Regarding the impact of majority vs. minority status upon leadership challenges, contests appeared to occur more frequently in the majority party than in the minority one. Since the stakes are higher and the rewards are greater in the majority, the high incidence of contests for posts which contain real power should not be surprising. The last three intraparty contests which toppled leaders occurred during minority situations, but all three involved Republicans. The Democratic leadership's general invulnerability to caucus challenges, regardless of party circumstances, reduces the power of this variable in explaining change.

These interparty differences reveal two distinct leadership succession systems. The Democratic House organization is hierarchically arranged in a tightly controlled system which is designed to minimize internal conflict. The end result is a leadership with wide ranging "inclusive" attitudes toward issues that face the House. In contrast, the House Republicans appear to be relatively egalitarian in their

leadership succession system, with open competition and a dominant role for the membership. The Republican leaders who emerge from this system appear to have "exclusive" attitudes toward House issues.

At this point, a possible reason for the difference in the functioning of the two parties may lie in the difference in their composition: the House Democrats are a large and heterogeneous mix of contending social groups, regions, and ideologies, while the House Republicans are a smaller and more homogeneous band of predominantly conservative small-town and small-city white males. Conflict within a homogeneous setting will not result in one social or ideological group's triumphing over another. Only the names in the offices will change, and in such a context, change of style will be more common than change of substance.

When a party is divided as the Democrats are, leadership change can result in a complete reshuffling of power. Formerly excluded groups can gain access to key posts and deny others this privilege. The legislative agenda may undergo an enormous transformation following an upheaval in the leadership. Any observer of the last few Democratic nominating conventions can readily see the consequences of such leadership change upon the party's presidential wing. It certainly would appear as if the controlled succession system which the House Democrats have carefully constructed is designed to avoid a similar outbreak of internecine warfare in the House.

Projections are always tentative, but it seems as if the Republican leadership succession system will remain relatively open and competitive. The party has undergone no fundamental reorientation in recent years. Hence, it is likely that its operating procedures will remain intact. The Democratic succession system has entered a transitional period as younger and more assertive members have joined the party's congressional ranks. Congressman Jim Wright is the first newly elected Democratic floor leader in thirty-six years who did not previously hold the position of whip. His election against a backdrop of deposed committee and subcommittee chairpersons indicates change for the floor leadership of the party. As the Democratic party becomes more philosophically united, and as the ancient regional conflicts recede, it is likely that the Democratic leadership will no longer need their controlled succession system and that the membership will no longer tolerate

Party Realignment and the Transformation of the Political Agenda: The House of Representatives, 1925—1938*

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Perhaps because of the political upheavals of the 1960s, party realignment has recently become a major area of research in political science. The scholars who have dealt with the phenomenon have concentrated on its electoral aspects — on what Burnham calls the "sharp reorganizations of the mass coalitional bases of the major parties." Another important characteristic ascribed to realignments has, to this point, received much less attention. According to Burnham, party realignments "result in significant transformations in the general shape of policy" or, in Ladd's terms, in a new political agenda. 4

From the realignment literature, the following theoretical sketch of the link between realignment in the electorate and policy transformation emerges. A party realignment is the result of the mass electorate's response to a new and highly salient issue which cuts across old party lines. Such massive changes in voting

*I would like to thank the Academic Senate of the University of California, Riverside, for an intramural grant which facilitated this research and the Inter-University Consortium for Political Research for the roll-call data.

¹See Walter Dean Burnham, Critical Elections and the Mainsprings of American Politics (New York: W. W. Norton and Co., Inc., 1970); James Sundquist, Dynamics of the Party System (Washington: The Brookings Institution, 1973); Gerald M. Pomper, Elections in America (New York: Dodd, Mead and Co., 1970); Everett Carl Ladd, Jr., American Political Parties: Social Change and Political Response (New York: W. W. Norton and Co., Inc., 1970); Kevin P. Phillips, The Emerging Republican Majority (Garden City: Doubleday and Co., Inc., 1970); Robert H. Blank, "Party Realignment and Congressional Elections," paper presented at the 1975 Meeting of the Western Political Science Association.

²Burnham, p. 10.

³Ibid., p. 10. Sundquist and Ladd do discuss the transformation of policy, but neither undertakes a detailed analysis.

⁴Ladd, p. 2.

behavior will only occur if a considerable proportion of the electorate feels that the government should respond to or in some sense handle the new issue. That is, the catalytic event must be perceived as being amenable to government action. The new alignment will be lasting only if the party which benefits from the new issue does respond in a way that the new majority considers satisfactory. A realignment thus has policy implications: a new element is added to the political agenda.

Because the issue which precipitated the realignment is highly salient, the majority party must quickly attempt to satisfy its new coalition by responding to the issue. Because the issue is new, a satisfactory response is likely to require fairly radical departures from past policies. If the problem had been amenable to incremental, politics-as-usual solutions, realignment would have been unlikely. Through its responses to the issue now at the center of controversy, the majority party may create a new political agenda. Not only are certain pieces of innovative legislation passed, but the centers around which controversy revolves are permanently changed. Thus, in the United States, realignments are an important mechanism through which policy changes come about. They may be the major mechanism through which rapid and nonincremental change occurs.

It, therefore, seems important to scrutinize the policy process during periods of realignment. This study will examine the transformation of policy at the national level during the New Deal realignment. The above theoretical sketch suggests that a relatively abrupt and nonincremental transformation in policy should have occurred in direct response to the Depression and that the coalition supporting the new agenda should be a partisan one. Thus, the questions to be asked are: Was there a major transformation, in what areas of policy, when

and how abruptly did it take place, and what were the coalitional bases for the new agenda?

Data and Framework of Analysis

The data used are roll-call votes in the House of Representatives, 1925 through 1938 (69th-75th Congresses). The House is used since biennial elections should make it particularly sensitive to the winds of change. The study begins with 1925 so as to provide a baseline against which change can be measured.

In order to examine transformation in policy outputs, some sort of categorization of policy types is needed. Aage Clausen has developed a useful categorization of policy in his work on the Congresses of the 1950s and early 1960s.5 From an examination of the content of roll calls taken during that period, Clausen delineated five major policy domains - government management of the economy, social welfare, agricultural assistance, civil liberties, and foreign affairs. In each of the Congresses and in each issue domain, Clausen isolated a preponderant issue cluster which met the criteria for unidimensionality. Clausen further showed that, from the 83rd through the 88th Congresses, voting alignments on each issue dimension were highly stable.

Since no critical realignment occurred between the New Deal era and the period Clausen studied, one would expect that at least some of his policy dimensions took their present shape during the New Deal realignment. This should be the case if realignments are the major source of basic agenda change and if a new agenda did come out of the New Deal realignment. Clausen's categorization will thus be used. The questions to be asked are: Do any of the dimensions exist in the Congresses before realignment? Which dimensions, if any, develop in response to the realignment? Are there other stable dimensions during this period which have since disappeared, and if so, was their disappearance due to the realignment? Within the broad definition of each policy area, what is the policy content of the dimensions found, and does it change during the period of realignment? Did voting alignments change and, if so, how?

Procedure

The procedure used to establish the existence of unidimensional clusters of roll calls is similar to that used by Clausen. Roll calls on which the majority was greater than 90 per cent were excluded from the analysis. The remaining roll calls were categorized by issue content. The Johnson hierarchical clustering technique was applied to matrices of Yule's Q-coefficients within each congress. A dimension was defined as consisting of a group of roll calls with common content for which the minimum Yule's Q-intercorrelation was .60. This criterion produces clusters with mean intercorrelations of approximately .80.

Since this procedure establishes unidimensionality, a simple scoring procedure can be used. In each cluster, one roll call was arbitrarily chosen to establish direction; the direction of the other roll calls was then determined by the sign of their correlation with the chosen roll call. For each congressman who had voted on one half or more of the roll calls in a given cluster, the percentage of the roll calls on which he voted in the chosen direction was computed, and this became his score.

Realignment and the Changing Regional Bases of the Major Parties

The data on the landslide Democratic victories during the 1930s are well known and will not be belabored here. The presentation of data on the regional composition of the major parties in the House shows how much the Democratic party changed not only in size but in composition during the period under study. Table 1 shows the percentage of the seats in each of eight regions held by Democrats over the seven Congresses and the percentage of all House seats held by Democrats.7 During the 1920s, the Democrats were a regional party. In the 69th, 70th and 71st Congresses (1925-1930), almost two-thirds of Democratic House members were from the South, which then held about 30 per cent of all House seats. In no other region did the Democrats consistently win as much as one-third of the seats.

The magnitude of the change can be seen by comparing these three Congresses with the first three New Deal Congresses. In the 73rd (1933-1934), Democrats held over half the

⁵Aage Clausen, How Congressmen Decide: A Policy Focus (New York: St. Martin's Press, 1973). See also Clausen, "Measurement Identity in the Longitudinal Analysis of Legislative Voting," American Political Science Review, 60 (December, 1967), 1020–1035; and Clausen and Richard B. Cheney, "A Comparative Analysis of Senate-House Voting on Economic and Welfare Policy, 1953–1964," American Political Science Review, 64 (March, 1970), 138–152.

⁶For a description of Clausen's method see Clausen and Cheney. For the Johnson technique see Steve Johnson, "Hierarchical Clustering Schemes," *Psychometrika*, 32 (September, 1967), 241–254.

 $^{^{7}}$ The regional categorization is that used by the Survey R-search Center.

Table 1. The Changing Regional Composition of the Democratic Party in the House 1925-1938 (Democratic % of Seats by Region)

				Congress			
Region	69	70	71	72	73	74	75
New England	12.5	10.3	12.5	25.0	41.4	55.2	44.8
Middle Atlantic	26.1	32.6	28.3	33.7	47.9	59.6	68.1
East North Central	16.3	18.6	14.0	37.2	71.1	65.6	72.2
West North Central	24.6	29.8	15.8	31.6	63.8	59.6	53.2
Solid South	98.9	98. 9	94.7	97.9	100.0	100.0	100.0
Border South	65.9	70.7	51.2	78.0	94.9	92.3	92.3
Mountain	28.6	35.7	21.4	28.6	92.9	100.0	100.0
Pacific	15.8	10.5	10.5	15.8	65.5	69.0	79.3
Democrats as % of							
House members	42.1	44.8	38.2	50.6	72.0	74.0	76.6

seats in all regions except New England and the middle Atlantic. In the other nonsouthern regions, the Democratic percentage varied from 63.8 to 92.9. More than half the seats in every region were held by Democrats in the 74th Congress (1935–1936); and in the 75th (1937–1938), Democrats held a majority in all but New England and more than two-thirds in all but one of the other regions.

The New Deal realignment changed the Democratic party in the House from a southern and thus heavily rural party to a national party.⁸ The southerners' proportion of the Democratic House membership dropped from almost two-thirds to about 40 per cent.

The Government Management Dimension

Clausen's government management issue domain centers on legislation dealing with the

⁸For a more detailed analysis of the change in the Democrats' constituency base, see David W. Brady, "Critical Elections, Congressional Parties and Clusters of Policy Changes: A Comparison of the 1896 and 1932 Realignment Eras," paper delivered at the 1975 annual meeting of the American Political Science Association.

economy and the nation's resources. Since tax policy and questions about the overall level of government spending are included, roll calls which fall into this issue category have existed since the nation began. By the 1920s, government regulation of business and involvement in public works had also had a long history. The fact that roll calls fall into the domain does not, however, guarantee the existence of a government management dimension. The requisite unidimensionality will result only if congressmen see the roll calls as similar and thus use similar voting cues.

In each of the Congresses under study a predominant government management dimension does emerge. Table 2 shows the stability of the dimension over time. The mean intercorrelation of scores is .93, and throughout the period, the relationship between government management scores and party is high.

This seeming stability hides considerable change in the content of the roll calls over time. In the pre-New Deal Congresses, federal spending was not seen as a tool for government

⁹Clausen, How Congressmen Decide, pp. 49-50.

Table 2. The Government Management Dimension - Intercorrelations between Congresses

				Congress			,
Congress	69	70	71	72	73	74	75
69		.93	.92	.96	.95	.92	.94
70			.95	.93	.94	.92	.91
71				.94	.93	.92	.90
72					<i>.</i> 95	.92	.92
73						.91	.95
74							.87
Correlation with party (Dem=1)	.95	.94	.90	.94	.96	.90	.94
(Rep=0)							

management of the economy. The Democrats favored additional government spending when it would directly help their constituents. Money for additional rural letter carriers 10 and for flood control projects on the lower Mississippi,11 for example, were strongly supported by the heavily rural Democratic membership. When the proposed spending did not benefit their constituents, Democrats tended to interpret it as milking the taxpayer for the benefit of selfish private interests or for the benefit of the Republican party and voted in opposition. Thus the Democrats opposed spending several hundred thousand dollars to improve a road in northern Alaska, claiming it would benefit only a few gold prospectors. 12 They attempted to reduce the appropriation for ocean mail service, saying it was simply an unwarranted subsidy for certain steamship companies, 13

On regulatory legislation, varying tinges of populist-progressive ideology are evident. Democrats consistently opposed any legislation which would directly or indirectly weaken the anti-trust laws. Thus a proposal to allow pools or combinations for the purchase of raw rubber abroad¹⁴ and another to lift the prohibition on interlocking directorates in banks not in substantial competition with each other brought forth classic populist statements on the House floor. 15 The Democrats opposed some regulatory legislation, arguing that the legislation did not contain sufficient safeguards against monopoly. 16 State's rights and Jeffersonian minimal government arguments, however, also appear. Democrats, for example, opposed additional employees for the Federal Power Commission claiming it was just the first step towards the creation of a giant bureaucracy.17

On tax policy, elements of populist-progressive ideology are also evident. While no longer united in opposition to Secretary of the Trea-

 10 Amendment to H.R. 5959. 69th Congress, Jan. 4, 1926, CR-67-2-1439.

 11 S.3740. 70th Congress, April 24, 1928, CR-69-7-7124.

¹²H. J. Res. 73. 69th Congress, February 2, 1927, CR-68-3-2845.

¹³H.R. 853. 71st Congress, January 18, 1930, CR-72-2-1920.

¹⁴H.R. 8927. 70th Congress, April, 1928, CR-69-6-6047.

¹⁵H.R. 6491. 70th Congress, February 1, 1928, CR-69-3-2339.

¹⁶See, for example, debate on H.R. 10288, a bill regulating buses in interstate transportation, 71st Congress, February 24, 1930, CR-72-6-6028.

¹⁷H.R. 8141. 70th Congress, March 20-21, 1928, CR-69-5-5066, 5112.

sury Mellon's proposals to lower taxes on high incomes as they had been in the early 1920s, ¹⁸ Democrats did oppose Mellon's proposals much more consistently than Republicans did. Democrats also favored a graduated corporate tax and attempts to close loopholes in corporate tax law. ¹⁹ Tariff legislation, which was important in the 71st Congress (1929–1930), saw Democrats tending to take the free trade position on manufactured goods but favoring some protection for farm products and raw materials.

Several of the minor government management dimensions show a stronger populist-progressive thrust. In the 69th (1925–1926), a number of roll calls on allowing branch banking and on radio regulation form a unidimensional cluster. In the 72nd (1931–1932), roll calls on limiting the size of loans under the Reconstruction Finance Corporation, for imposing a surtax on high incomes, for congressional review of any tax refund over \$20,000, and for government operation of Muscle Shoals cluster. These dimensions bring forth moderately similar voting patterns (r = .75). Neither is as highly related to party as are the major government management dimensions (69th, r = .61; 72nd, r = .71).

During this period, the Republicans are a pro-business party — as shown by their support for the Mellon tax bills, their attempt to turn over Muscle Shoals to private business and the sort of regulatory legislation they sponsored. Thus, in the pre-New Deal Congress, the extent of pro- or anti-big business sentiment does to some extent distinguish the parties, but neither party advocated an activist federal role in regulating or managing the economy.

With the 73rd Congress (1933–1934), the major government management dimension becomes a New Deal dimension. In the 73rd, a high score indicated the congressman voted for the creation of the TVA, for the invalidation of the gold standards clause, ²⁰ for Roosevelt's tax bill, ²¹ for the Reciprocal Trade Agreements Act, and for the Securities Exchange Act. In the 74th and the 75th Congresses (1935–1938), the major government management dimension included roll calls on Roosevelt's "soak the rich" tax bill, ²² on a controversial corporate tax bill, ²³ on the regulation of public utility

¹⁸David Burner, *The Politics of Provincialism: The Democratic Party in Transition 1918-1932* (New York: Alfred A. Knopf, 1968), pp. 162-164.

 $^{^{19}\}mbox{See}$ roll calls on H.R. 1, 70th Congress, December 15, 1927, CR-69-1-715, 715.

²⁰H.J. Res. 192.

²¹H.R. 7835.

²²H.R. 8974, 74th Congress.

²³H.R. 12395, 74th Congress.

sage), on extending the time in which the President could further devalue the dollar,24 and on the Banking Act of 1935. By and large, the bills included in the dimension were proposed or at least strongly supported by the was highly partisan. Thus, while the gross voting response remained stable, the content of the measures included in the government manthe New Deal. The result was a much greater ideological distance between the parties, with figures clearly indicate that Roosevelt by no the Democrats now clearly supporting an activist position. The average Democrat's support for the party position on the major government management dimension is actually higher after 1930 than in the less ideologically charged preceding years. (See first column of Table 3.) thrust too far.

Not all major New Deal measures cluster in

holding companies (recommital and final pas- each is influenced by party, but on neither is the level of party voting as high as on the major government management dimensions. (The correlation with party is .68 for the NIRA; .72 for the death sentence dimension.) These lower correlations are due primarily to the Demoadministration and represent an activist federal crats' lower support for these measures as government philosophy. Congressmen's voting compared with their support for those included behavior on these measures, as shown before, in the main government management dimension. The mean Democratic support on the major dimensions for the 73rd through the 75th Congresses is 90.1 per cent; on the minor agement dimension changed with the coming of dimensions, 74.2 per cent. While Democrats did give substantial support on these measures, the means had a blank check even from these overwhelmingly Democratic Congresses. The party position had shifted toward favoring activist government, but many Congressional Democrats were leery of carrying the activist

Throughout the period, party affiliation is the main dimension. In the 73rd (1933-1934), highly related to voting on the major governa number of roll calls on the National Industrial ment management dimensions and substantially Recovery Act form a dimension of their own, related on the minor ones. But, since the The same is true of roll calls on the death relationship is not perfect, other factors which sentence provision of the Public Utility Holding might affect the vote must be examined. In the Company bill in the 74th (1935-1936). This contemporary Congresses region is an imbill would, in effect, have outlawed holding portant predictor of vote; and, as was shown companies in the public utilities area.²⁵ Both earlier, the realignment significantly changed were administration proposals, and voting on the regional composition of the Democratic party. It thus seems appropriate to ask: Did regional subgroupings display distinctive voting patterns in the 1920s, and what effect did the realignment have?

The procedure used to answer this question

Table 3. Government Management Voting Scores by Party and Region

	•	Dem	ocrats		Republicans			
Congress	Ail	NE&MA ^a	SS&BS ^a	WNCa	All	NE&MA ^a	WNCa	
69	86.4	93.4	85.8	86.7	5.7	3.8	2.6	
70	83.4	62.4	90.6	81.5	14.2	6.8	18.6	
71	84.7	67.9	88.9	93.2	9.9	1.5	23.2	
72	94.8	95.9	94.6	93.3	11.6	4.8	21.0	
73	91.6	88.8	96.4	91.4	10.9	6.0	24.7	
74	89.3	86.5	90.0	91.5	11.8	4.5	35.4	
75	89.4	89.8	94.7	80.7	6.2	3.2	12.1	
69 Populist-								
Progressive	49.9	3.8	55.0	73.4	6.8	1.6	11.8	
72 Populist-								
Progressive	71.4	37.4	79.6	90.9	20.3	4.3	38.9	
73 NIRA 74 Public Utility	78.9	94.5	77.5	62.2	16.2	10.2	18.2	
Holding Co.'s	69.4	60.0	72.4	83.0	8.4	2.5	26.2	

aNE&MA = New England and Middle Atlantic; SS&BS = Solid South and Border South; WNC = West North Central.

²⁴S416, 75th Congress.

²⁵For a good description of the controversy, see Arthur M. Schlesinger, Jr., *The Politics of Upheaval* (Boston: Houghton Mifflin Co., 1960), pp. 302-325.

is stepwise multiple regression analysis.²⁶ The dependent variables are the scores on the various government management dimensions, the independent variables, party and the eight regions defined by the SRC. For each dimension, an equation is estimated for all House members using party and region as the independent variables; then an equation for each party is estimated with the region dummy variables only as independent variables. Because regression equations with this many variables are difficult to interpret at a glance, tables showing mean scores for the most distinctive regional groupings are given.

Despite the strong relationship between party and vote on the major government management dimensions, some regional differences within party do appear. Within the Republican party, regional differences account for substantially more than 10 per cent of the variance in scores in all but one Congress (69th, 1925-1926). In the pre-New Deal Congresses, the greatest differentiation in voting behavior along regional lines occurs in the 70th Congress. Of the major government management dimensions, this one has the strongest populist-progressive content.²⁷ The progressive heritage of many congressmen from the West North Central region shows up in their being consistently the most deviant regional grouping. (See Table 3). Prior to the 73rd Congress (1933-1934), east north central congressmen also show a greater tendency than other Republicans to vote with the Democrats but, in the New Deal Congresses, this deviance ceases. Throughout the whole period, northeastern Republicans are most loyal to party.

From the 69th through the 71st Congress (1925–1930), Democrats show somewhat greater splits along regional lines than do Republicans. After the Democrats become the majority party in the 72nd (1931–1932), the positions of the two parties are reversed. The strong populist-progressive thrust of the government management dimension in the 70th Congress resulted in distinct regional voting patterns with the South and the West North Central region strongly supportive and the northeast most opposed. Through the 73rd Congress (1933–1934), when regional splits

occur, the same pattern emerges. Only in the 75th Congress (1937–1938), do Congressmen from the middle Atlantic states become somewhat more loyal than the average Democrat and, even then, they are less supportive of the party position than are southern Democrats.

The two minor government management dimensions with a strong populist-progressive thrust which appear in the pre-New Deal Congresses elicited similar regional voting patterns. The West North Central and to a lesser extent the southern Congressmen were especially supportive of the populist-progressive pole, the northeastern congressmen especially opposed. Not surprisingly, given its antimonopoly thrust, the public utilities holding companies dimension also produced similar voting patterns. Scores on this dimension show a correlation of .72 with those on the minor dimension in the 69th and of .80 with that in the 72nd. On the National Industrial Recovery Act dimension, in contrast, regional voting patterns are quite different. The governmental philosophy underlying this legislation significantly conflicts with a major tenet in populist-progressive thought, and many members were concerned that the NIRA, in effect, sanctioned monopoly. On this dimension, Democratic congressmen from the West North Central region and from the solid South, who on most government management legislation were most supportive of the Democratic party position, were most likely to vote in opposition.

This discussion of region should not obscure the predominance of party as a predictor of vote on the government management dimensions. The findings do show that the realignment did not immediately change the regional voting patterns of the 1920s into those that prevail today. In the 1920s, southerners were the most loyal segment of the Democratic party while the relatively few northeastern Democrats were the most deviant.²⁸ Within the Republican party, northeasterners were the most loyal; Midwesterners, especially those from the West North Central states, were the most likely to defect from the party position. The immediate effect of the realignment was to alter massively the party balance and, through the infusion of large numbers of new Democrats, to change policy outputs drastically. These new Democrats voted very differently from the Republicans they had replaced. The realignment did not, however, result in any drastic shift in

²⁶The SPSS stepwise regression program was used. See Norman Nie, Dale Bent, and C. Hadlai Hull, Statistical Package for the Social Sciences (New York: McGraw-Hill, 1970), Chapter 15. Tables presenting the regression results in full are available from the author.

²⁷Included are a number of roll calls on tax measures and several on bills which would indirectly relax antimonopoly laws in certain cases. See notes 14, 15 and 19.

²⁸These findings confirm those of Turner. See Julius Turner, *Party and Constituency: Pressures on Congress* (Baltimore: Johns Hopkins Press, 1951), p. 136.

relative party loyalty within either party. On the government management dimensions, which include much of the significant New Deal legislation, the regional patterns of the 1920s persisted through the 1930s.

Agricultural Policy

In the government management area, one major dimension persists throughout the period under study, even though, with the coming of the New Deal, the content of the roll calls included changes significantly. In the area of agricultural policy, on the other hand, voting alignments change drastically within the period under study. As Table 4 shows, the dimension in existence at the beginning of the period is completely transformed. In the 69th and 70th Congresses (1925–1928), voting on agricultural policy is unrelated to party; from the 71st (1929–1930) on, it is highly influenced by party.

This shift is the result of a change in the content of the farm bills being considered and of a change in the environment. Farmers did not participate in the prosperity of the 1920s. With the end of World War I, farm prices dropped drastically, while the prices of farm supplies and other manufactured goods remained high. Whether and how farmers should be helped was one of the major sources of controversy during the 1920s.²⁹ In Congress, the controversy centered on the McNary-Haugen plan, the brainchild of George Peek of the American Farm Bureau Federation. The McNary-Haugen bill proposed the setting up of a two-price system for agricultural products. Domestic prices were to be kept high by restricting the domestic supply. This would be done, not by cutting back production, but by dumping abroad whatever could not be sold at

²⁹For a good brief discussion of the farm situation and the various legislative remedies proposed, see John D. Hicks, *Republican Ascendancy 1921-1933* (New York: Harper Torchbooks, 1960), pp. 193-200.

the high domestic price. The plan appealed to farmers who sold most of their output in the domestic market; it had little to offer those, such as cotton growers, who sold a large proportion of their crop abroad. All the roll calls included in the agricultural dimension in the 69th and 70th (1925-1928) were taken on various versions of the McNary-Haugen plan. A version was defeated in the House in the first session of the 69th, as had been the case in the previous Congress. Both houses passed the bill in the second session of the 69th and in the 70th; but in each case, it was vetoed by President Hoover. Its supporters' inability to muster the votes to override the vetoes killed the McNary-Haugen plan as a viable answer to the farmer's plight.

The Depression, which brought a further drastic drop in cotton and wheat prices, and the great drought of 1930 made it clear even to Hoover that something had to be done and aided in uniting the Democrats. The 71st Congress's agricultural dimension consists of roll calls on Democratic attempts to strengthen and liberalize Hoover's proposals. To the rather weak Agricultural Marketing Act of 1929 the Democrats attempted to add the debenture plan. An exceedingly complex piece of legislation, this plan would essentially have used tariff receipts to aid farmers wherever they sold their crops. The Democrats also attempted to increase the amount of money to be spent on drought relief over the amount desired by Hoover. Since, at this time drought was especially severe in the lower Mississippi Valley, this legislation was of great interest to southerners.

In the 72nd Congress (1931-1932), no agricultural dimension appears.³⁰ With the House narrowly controlled by the Democrats and the presidency still Republican, stalemate on agricultural policy had been reached. From

³⁰H.R. 13991, a fairly comprehensive agriculture bill, was passed in the House on January 12, 1933, but died in the Senate. There was only one roll call vote on the bill.

Table 4. The Agricultural Policy Dimension - Intercorrelations between Congresses

Congress	69	70	71	73	74	75
69		.85	.14	.48	.41	.33
70		L	.21	.54	.47	.43
71				.94	.92	.89
73					.88	.82
74						.80
Correlation						
with party	.09	.13	.93	.90	.89	.76

the 73rd Congress on, the agricultural dimension centers on the New Deal farm program. Roll calls on the Agricultural Assistance Act, on the Emergency Farm Mortgage Act, on the Cotton Control Act, the Soil Conservation Act, and other similar bills are included.³¹ The severity of the farm situation and the wide coverage of the legislation which provided benefits to producers of all farm products assured generally high Democratic party support for this legislation.

This brief overview of the changing farm situation and the resulting changes in policy provide a plausible explanation for the change in voting patterns described above. As one would expect, during the McNary-Haugen period, voting was largely sectional (see Table 5). Congressmen from the northeast and from the solid South opposed the legislation; West North Central congressmen were its most fervent supporters. The split was most intense within the Republican party; in both the 69th and the 70th Congresses, the regional dummy variables account for almost half of the variance in Republican scores. During the New Deal Congresses, the Republican party continued to split on farm legislation, with the West North Central congressmen consistently most supportive of the Democratic position. Among Democrats, regional splits were much less deep. The solid South had the highest levels of support, but all sections of the party supported the administration program.

In the agricultural policy area, regional voting alignments gave way to party voting under the pressure of political and economic events. The change took place before the 1932 election and the precipitant was the worsening of the farmers' position in 1929 and 30. The great Democratic victories of 1932, 1934, and 1936 were, however, required to solidify the

311'or a discussion of this legislation, see Lawrence Henry Chamberlain, *The President, Congress and Legislation* (New York: Columbia University Press, 1946), pp. 252–268.

new alignment behind a comprehensive and, at the time, radical program.

Social Welfare

The origins of the welfare state are popularly associated with the New Deal. Prior to the Depression, legislation designed to give relatively direct help to the individual³² was not completely without precedent in American history. Some labor legislation was on the books;³³ veterans' pensions and other social services for certain classes of veterans had a relatively long history; and, in the early 1920s, legislation concerning maternal and infant health needs was passed.³⁴ Nevertheless, direct help to individuals was generally not considered a legitimate federal function.

During the period under study, the social welfare dimension first appears in the 71st Congress (1929–1930). Roll calls on a veterans' pension bill and on providing additional hospital facilities for veterans are included. More clearly related to the Depression are the roll calls on taking a census of unemployment and on the use of federal funds for relief. President Hoover favored relying upon private charity to alleviate the misery caused by widespread unemployment and was firmly opposed to a federal "dole." The Democrats read their gains in the 1930 midterm election as a mandate, and in the 72nd Congress, Speaker John Nance Garner proposed a several billion-

Table 5. Agricultural Policy Voting Scores by Party and Region

		Demo	crats	Republicans			
Congress	All	NE&MA	SS	WNC	Ali	NE&MA	WNC
69	55.6	41.1	47.6	92.9	50.1	11.3	90.0
70	64.2	38.7	59.6	93.8	52.3	9.8	91.0
71	93.9	81.7	97.2	95.6	8.5	1.6	12.6
73	88.3	82.9	94.8	76.1	11.6	3.2	37.2
74	90.0	79.1	99.0	90.0	7.5	.7	27.0
75	76.8	76.6	88.6	65.8	9.1	.9	28.9

³²For Clausen's definition of the social welfare domain see *How Congressmen Decide*, p. 46.

³³For an overview of pre-New Deal labor legislation, see Chamberlain, *President*, *Congress and Legislation*, pp. 138–164.

³⁴William Chafe, The American Woman: Her Changing Social, Economic, and Political Roles, 1920–1970 (New York: Oxford University Press, 1972), p. 27.

³⁵William E. Leuchtenburg, *The Perils of Prosperity 1914-1932* (Chicago: University of Chicago Press, 1958), p. 252.

Table 6. The Social Welfare Dimension – Intercorrelations between Congresses

Congress	71	72	73	74	75
71 72 73 74		.91	.89 .87	.93 .88 .89	.89 .80 .82
Correlation with party	.89	.89	.90	.92	.83

dollar relief bill.³⁶ Democratic control of the House and Hoover's statement that the bill was "the most gigantic pork barrel ever proposed... an unexampled raid on the public treasury"³⁷ helped ensure that the measure would be perceived in partisan terms. The struggle over relief spending dominates the major social welfare dimension during the New Deal Congresses also. Debate in the 71st and 72nd Congresses (1929–1932) centered around whether the federal government should provide direct relief; in the later Congresses, the controversy concerned the dollar level.

As Table 6 shows, voting on the major social welfare dimension was partisan at its inception and remains so during this period. In neither party are major regional variations in voting response evident. (See Table 7.) Midwestern and Pacific Republicans do show some tendency to support the Democratic position more than their party colleagues. Among Democrats, no consistent regional splits occur; there is certainly no tendency for the South to be less loyal than other segments of the party.

Two minor social welfare dimensions which appear in the 75th Congress (1937–1938) produced quite different alignments. One in-

³⁶E. Pendleton Herring, "First Session of the Seventy-second Congress," American Political Science Review, 26 (October, 1932), 869–872 for a discussion of the bill and the political controversy surrounding relief spending in that Congress.

³⁷Quoted in *ibid.*, p. 869.

cludes roll calls on the National Housing Act; the other consists entirely of roll calls on the Fair Labor Standards Act. On neither is voting as partisan as on the major social welfare dimensions. The correlations with party for the first is .67; for the second, .38. Both of these dimensions produced a North-South split within the Democratic party, with southern Democrats much less supportive of the party position than their northern colleagues. On the fair labor standards dimension, which produced the deeper split, the two southern regional dummy variables account for almost half the variance in Democratic scores.

The regional split on the Fair Labor Standards Act had an economic foundation. Southerners feared that a nationwide minimum wage would nullify their region's advantage in attracting industry. For the same reason, some usually very conservative northeastern Republicans supported the bill. Still, since this split within the Democratic party was to become routine rather than exceptional and since the split also appears, though less strongly, on the other minor dimension, the voting alignment which appeared on the Fair Labor Standards Act cannot be interpreted simply as an anomaly. Rather, the alignments on both of the minor social welfare dimensions seem to mark the beginning of the pattern Clausen finds in the 1950s and early 1960s.

Why, then, did the southern Democrats go along on the major social welfare dimensions, and why did the regional split begin at this

Table 7. Social Welfare Voting Scores by Party and Region

	Democrats			Republicans			
Congress	All	NE&MA	SS	Ail	NE&MA	WNC	
71	97.3	96.9	97.9	20.9	11.9	26.6	
72	96.3	97.4	99.7	11.5	5.0	13.5	
73	86.8	93.8	87.4	2.6	.7	7.6	
74	91.1	93.3	95.0	2.0	.6	4.8	
75	83.6	85.0	84.3	7.7	3.4	15.8	
75—Housing 75—Fair Labor	82.6	96.8	62.2	25.3	16.3	38.9	
Standards Act	77. 8	99.0	39.2	43.5	46.1	39.8	

time? Certainly the severity of the national emergency played an important role in solidifying the support of all Democrats behind Roosevelt's programs. The South as the poorest section of the country was hit especially hard by the Depression and southern congressmen were quick to demand federal relief. The emergency itself and most of the social welfare programs designed to alleviate it were seen as temporary. Almost all of the roll calls included in the major social welfare dimensions concern such temporary relief programs. A congressman's vote for work relief appropriations was simply a pragmatic response to a temporary emergency. It did not necessarily imply a commitment to a new philosophy of government predicated on ongoing government responsibility for the welfare of the individual.

In contrast to the major social welfare dimensions, the two minor ones which appear in the 75th Congress (1937–1938) are predominantly composed of roll calls on permanent programs and took place at a time when many believed the emergency to be over. ³⁸ A positive vote on them does imply a commitment to continued government activism in the social welfare area. It is on such programs that the North-South split in the Democratic party begins to appear.

Thus, under the stimulus of economic emergency, a new dimension appears. Neither in content nor in the voting alignments it produced is it identical with the modern social welfare dimension. Only at the end of the period under study does one begin to see outlines of the modern dimension taking shape.

Civil Liberties

In addition to the three issue dimensions already discussed, Clausen identifies two others in the modern Congress. During the period under study, no stable civil liberties or international involvement dimension exists. Roll calls on issues with civil liberties implications do appear and, in a number of Congresses, one or more clusters of such roll calls meet the criterion of unidimensionality. These clusters do not, however, correlate at a high level across Congresses.

If the civil liberties domain is defined broadly, seven such clusters appear. One, in the 69th Congress (1925–1926), consists of roll calls on immigration legislation; another, in the 74th (1935–1936), concerns the deportation of ali-

³⁸James T. Patterson, Congressional Conservatism and the New Deal (Lexington, Ky.: University of Kentucky Press, 1967), especially pp. 169, 175.

ens; two, in the 71st and 72nd (1929-1932). concern prohibition; another in the 71st, congressional reapportionment. The two which appear in the 75th (1937-1938) most clearly fall into the civil liberties domain as defined by Clausen. One consists of roll calls on antilynching legislation; the other includes votes on withholding funds for the salaries of District of Columbia teachers who advocate communism and on instituting an investigation of sit-down strikes. In general thrust if not in specific content, these two clusters are similar to the modern civil liberties domain. Yet their correlation is -.22. The only really high correlation (.91) between any pair of these clusters is between the immigration cluster in the 69th Congress and the anti-lynching cluster in the 75th.

The scoring procedure on these clusters is such that a high score always indicates pro-civil liberties voting. A number of the correlations are negative, indicating that a congressman who took a pro-civil liberties position on one was likely to take an "anti" position on the other. The signs of the correlations with party also vary. Voting alignments on these clusters are thus unstable.

As Table 8 shows, Republicans sometimes took the pro- and sometimes the anti-civil liberties position. Regional differences are seldom great. The variation in the Republicans' voting behavior on these clusters indicates that they did not perceive the issues involved as philosophically similar. Among Democrats, the situation is somewhat different. All the clusters produce splits along regional lines and, while the depth of the split varies from cluster to cluster, its form is remarkably stable. In every case, the South - especially the solid South takes the anti-civil liberties position and the northeast takes the "pro" position. West North Central and mountain congressmen tend to take an intermediate position. For Democrats, the mean intercorrelation of scores on these clusters is .55, compared with a mean of .01 for all members (.37 when the signs of the correlations are ignored). The correlations for Democrats only are not high enough to label these clusters a dimension; still they indicate that, among Democrats, voting alignments vary much less from cluster to cluster than they do among Republicans.

The heterogeneity of the Democratic party made party unity impossible on the sort of issues included in these clusters. During the 1920s the Democratic party was primarily composed of two quite distinct and often conflicting elements. On the one side was the rural, mostly southern element which tended to

Table 8. Civil Liberties Voting Scores by Party and Region

	Democrats			Republicans		
Congress-Legislation	All	NE&MA	SS	All	NE&MA	WNC
69-Immigration	31.1	100.0	8.1	95.9	99.6	96.4
71-Prohibition	39.1	97.€	18.0	11.6	16.7	7.1
71-Reapportionment	43.5	100.0	25.7	76.0	93.3	34.8
72-Prohibition	65.6	96.7	46.7	50.2	62.1	31.6
74-Alien Deportation	68.9	98.5	40.6	39.5	49.2	47.2
75-Anti-Lynching	59.4	96.0	1.1	94.7	93.5	96.1
75-Civil Liberties	53.0	69.1	27.8	10.9	3.2	26.0

be Protestant, prohibitionist, and usually nativist; on the other was the big city element which was usually wet, frequently Catholic, and often newly immigrant.³⁹ The 1928 nomination and election campaign was the most dramatic but not the only manifestation of this split. The influx of Democrats from previously Republican districts during the 1930s shifted the balance away from the South but did not alter the underlying division. In the House, this split had little effect on most issues during the period under study, but, on the moral, style-oflife issues included in the civil liberties clusters, the differing outlooks of the elements became predominant. Basically, then, the split within the Democratic party on such issues predates the existence of a stable civil liberties dimension

No other stable issue dimension appears during the Congresses under study. While a number of other scales were found in various Congresses, the voting alignments they call forth are ephemeral. There is no stability across Congresses.

Mechanisms of Change

In response to economic crisis, a new agenda was developed. Even before the 1932 election, congressional Democrats had responded to the crisis in a more innovative way than the ruling Republicans had. The election of a president and of a heavy congressional majority in 1932 gave the Democratic party the chance to build a majority coalition in the country by offering solutions to the economic problems, and Roosevelt was quick to seize this opportunity. The support the congressional Democrats gave Roosevelt's programs made the Democrats the party of the new agenda.

Quite clearly the legislative success of the new agenda was due to the large number of new Democrats elected in 1932 and re-elected, with reinforcements, in 1934 and 1936. As was shown above, voting behavior during the New

Deal was heavily partisan. Several scholars have pointed out that membership replacement is a major mechanism of change in Congress.40 Certainly, in the case under study, partisan turnover had massive policy effects. The hypothesis on the importance of turnover as a mechanism of change is not, however, restricted to partisan change. It is argued that new members, elected under different circumstances, will differ in their voting responses from their senior party colleagues. In a period of realignment, new members of the majority party will be more supportive of the new agenda than will senior members whose political behavior was shaped in an earlier period and who have less reason to interpret their election as a mandate for the new agenda.

To test the hypothesis, support scores on the major government management, social welfare and agricultural policy dimensions and on several of the important minor dimensions have been computed for veterans and newcomers within each party. Veterans are defined as congressmen first elected in 1928 or before; newcomers are members first elected in 1932 or later. Because the 1930 election is transitional, taking place after the Depression had begun and, under that stimulus, resulting in significant Democratic gains, members first elected to the 72nd Congress are excluded.

For Democrats, the hypothesis predicts that newcomers will have higher support scores for New Deal legislation than veterans do. The prediction for Republicans is less clear. On the one hand, because new Republicans were first

40See David W. Brady and Naomi B. Lynn, "Switched-Seat Congressional Districts: Their Effect on Party Voting and Public Policy," American Journal of Political Science, 17 (August, 1973), 528-543; Clausen, How Congressmen Decide, pp. 231-232. Marvin G. Weinbaum and Dennis R. Judd, "In Search of a Mandated Congress," Midwestern Journal of Political Science, 14 (May, 1970), 302; Barbara Hinckley, "Interpreting House Midterm Elections: Toward a Measurement of the In-Party's 'Expected' Loss of Seats," American Political Science Review, 61 (September, 1967), 697-699.

³⁹ Burner, Politics of Provincialism.

elected during the New Deal, one might hypothesize that they would be less set in their ways, less committed to the older party positions, and thus more sympathetic to the new agenda. Conversely, one might argue that since they won despite a strong adverse electoral tide, they would see themselves as mandated to oppose the new agenda and thus should have even lower support scores than their senior party colleagues.

With respect to Republicans, Table 9 offers little support for either form of the hypothesis. There are few consistent differences between Republican veterans and newcomers. Only on agricultural policy do newcomers consistently support the New Deal position more than veterans do, but even here the differences are not impressive in magnitude.

More important for the status of the hypothesis is the result of the test on the Democrats. Here again little support is found. On the major dimensions, the differences which do occur run counter to the prediction. Veterans are more supportive of the new agenda than newcomers are. Only on the Fair Labor Standards Act dimension do newcomers give significantly more support than veterans. The difference is largely due to the overrepresentation of members from the solid South among veterans. This is not, however, sufficient reason for dismissing the difference. Newcomers from the solid South were more supportive on this dimension than were veterans from the same region. Thus, while the hypothesis in general is not supported, it does hold on this one particularly innovative piece of legislation.41

⁴¹But see Brady, "Critical Elections," especially Table 23. On a general measure of party support,

Why generally were veterans somewhat more supportive than newcomers? A part of the explanation may lie in the newcomers' lack of the hindsight we possess. They easily could have interpreted their election as a mandate for the new agenda; nevertheless, many were elected from previously safe Republican districts and this may have resulted in a modicum of caution. After all they could not have known that many of these districts would become safely Democratic. The scores in Table 9 indicate that such responses, if they did occur, played only a minor role in determining newcomers' voting behavior. Newcomers may have been slightly less supportive than veterans, but their absolute level of support was still high.

The problem then resolves itself into explaining why the veterans were highly supportive. Why did congressmen whose ideological positions and political behavior patterns were formed in a previous era accept the new agenda so readily? Certainly the extent of the economic disaster is one answer. Our notions about the resistance of human beings to change are based on observations during periods of stability or slow change in the environment. Furthermore, the veterans participated in the development of the new agenda both before the 1932 election and afterward. 42 With some notable exceptions, this agenda was not simply thrust upon them by the administration. The data certainly support the notion that periods

Brady finds that, in the 73rd Congress, switched-seat Democrats are more loyal than nonswitched-seat Democrats.

Table 9. Veterans' and Newcomers' Support for the New Deal

	Den	nocrats	Republicans		
Congress	Veterans	Newcomers	Veterans	Newcomers	
Government Management					
73	95.1	90.3	10.5	10.7	
74.	90.8	90.1	7.5	18.0	
75	95.9	87.1	8.1	4.7	
Agricultural Policy					
73	90.6	84.8	8.6	14.9	
74	92.5	89.3	5.5	11.2	
75	87.2	72.4	7.3	10.3	
Social Welfare					
73	88.1	85.5	2.4	2.9	
74	94.7	89.8	1.3	2.6	
75	85.2	83.8	7.5	8.9	
Minor Dimensions					
NIRA	86.7	72.3	15.0	15.3	
Utility Holding Companies	69.1	70.9	5.4	12.0	
Fair Labor Standards Act	60.2	82.0	45.8	42.8	

⁴²Chamberlain, *President, Congress and Legislation*. See especially his summary, pp. 448–464.

of radical change in the political-economic environment may bring about rapid ideological change. Depending upon whether circumstances are reinforcing or not, the ideological change may be thorough and permanent or superficial and temporary. But under the felt need to do something to alleviate a new and frightening situation, it does inform action and, in the case of policy makers, leads to major, nonincremental changes in policy outputs.

Many southern Democrats were increasingly to defect from major elements of the new agenda in later years. Northern Democrats, who in the era before realignment had shown even less evidence that they would be receptive to such radical departures in policy, were to remain firm supporters. But, for everyone, the basepoint around which controversy about the role of the government in American society would revolve had shifted in the activist direction.

Conclusion

The 1930s realignment and the new agenda were clearly the products of the great Depression. Key argues that 1928 was a realigning election in New England.43 Nationally, however, the 1928 election saw the Democratic presidential candidate badly defeated and Democratic House strength decline in almost all areas. Even in New England, the Democrats picked up only one House seat, increasing their seats in that region from three to four, the same number they had held in the 69th Congress. One could argue that the potential for a realignment along class lines had existed since the 1890s. The rapid and massive industrialization following the Civil War produced some big winners and many victims. In 1896, the dominant populist element in the Democratic party did attempt to build a coalition of small farmers and urban workers. The attempt did not succeed and, since party identification hardens with the passage of time, some cataclysmic event was necessary to set off the realignment process.

The Great Depression was such an event. Electorally it produced immense national party turnover. From 1933 through 1938, the new majority party had very large majorities in the Congress and it passed a large amount of significant and innovative legislation. In the process, the centers around which political controversy would revolve were permanently changed. That the federal government should

⁴³V. O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3-18.

play a positive role in managing the economy and in protecting the individual from economic forces beyond his control became, during this period, a major tenet of the majority party's ideology.

The beginnings of and the Democratic party's identification with the new agenda predates the election of Roosevelt. In the years between the beginning of the Depression and the election of a Democratic president, congressional Democrats did attempt to represent the interests of the Depression's victims and, in so doing, were much more willing than the Republicans under Hoover to try new approaches and expand the federal government's responsibilities. One may speculate that during the Hoover years, the Democrats' status as the minority party and the greater prevalence among Democrats of remnants of populist-progress ideology made the Democratic party more likely to respond to the emergency in an innovative way.

These activities by congressional Democrats probably did not have a major effect on the elections of 1930 and 1932. In 1930, the Democratic congressional gains primarily reflected an anti-Hoover vote. Seemingly, in our system, only a president with his "bully pulpit" can produce in the voters' minds an identification between certain policies and his party. In the case of the New Deal much of the major legislation was developed in the executive branch. The congressional Democrats' legislative response to the emergency had taken the form primarily of short-run measures which would alleviate some of the suffering without making any structural changes. The Roosevelt administration developed a number of permanent programs intended to produce structural change. Thus, it was Roosevelt who made the Democratic party the party of the new agenda in the eyes of the electorate. In that process, the congressional party was a willing partner.

The agenda, because it was truly new and proposed nonincremental remedies for the nation's problems, increased the ideological content of American politics. Since the response in the House to the new agenda was partisan, the result was a much clearer ideological distinction between the congressional parties.

In each of the three major issue areas, the process of change took a distinct form. Voting on government management issues was heavily partisan before as well as after the Depression and realignment. With the advent of the New Deal, however, the issues involved became much more ideologically charged. During the New Deal, voting on the dimension constituted

governmental role in managing the economy. On agricultural policy, a regional alignment was transformed into a partisan one. The social welfare dimension first appeared in the 71st Congress (1929-1930) and was quite clearly a response to the Depression. From the beginning, it evoked a partisan voting response. As the newest of the issue areas, it was, during the late 1920s and through most of the 1930s, least similar to its modern counterpart. In terms of content and voting alignments, the government management and agricultural assistance dimensions took their modern form during the early New Deal. This was not true of the social welfare dimension. Under the pressure of the economic emergency, the need for temporary federal relief was fairly quickly accepted. Permanent programs which implied a continuing government role in protecting - or, depending upon one's perspective, interfering with - the individual were less readily accepted. Some permanent social welfare programs did pass easily without provoking a split in the Democratic party. But by the 75th Congress (1937-1938), many people had concluded that the worst of the Depression was over, and some Democrats began to have second thoughts. It was then that the regional split within the Democratic party in the House began to take its modern shape.

The very real changes which took place should not, however, lead us to ignore the continuities between the pre- and post-realignment periods. Within each party, voting patterns were not immediately transformed. West North Central Republicans were the most pro-

an endotsement or repudiation of an activist governmental role in managing the economy. On agricultural policy, a regional alignment was transformed into a partisan one. The social welfare dimension first appeared in the 71st Congress (1929–1930) and was quite clearly a response to the Depression. From the beginning, it evoked a partisan voting response. As the newest of the issue areas, it was, during the 1920s and through most of the 1930s, least gressive element in their party during the 1920s and remained so in the 1930s. Northeastern Republicans maintained their position as the most conservative section. The North/South split within the Democratic party was just beginning to take shape at the end of the period under study. During most of the period, and on most issues, southern Democrats were at least as loyal to the party position as their northern colleagues were.

Sundquist talks of the aftershocks of the New Deal realignment. Such a massive change reverberates through the polity; it takes time before all its effects become manifest. In the Midwest, the rebirth of the Democratic party and the sorting out of the electorate into the two parties along ideological lines eventually led to the extinction of the progressive element in the Republican party. The logic implicit in the New Deal's attempts to provide some security for the individual through governmental action led, in the late 1930s, to a regional split within the Democratic party that was to deepen with the years.

While all the effects of the realignment on the electorate and on the House took many years to become fully manifest, the major outlines of the political agenda which dominated politics at least till the mid-1960s took their shape in the period under study. Thus it is clear that a sufficiently intense environmental stimulus can produce rapid and radical change even in a political system seemingly designed to further only incremental change.

⁴⁴Sundquist, Dynamics, Chapters 11 and 12.

Corrupt Politicians and Their Electoral Support: Some Experimental Observations*

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A venerable tradition of scholarship in American politics is concerned with the nature of "political corruption." The consensus in this work seems to be that governmental

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¹For a useful collection of some of this work, see Arnold J. Heidenheimer, ed., *Political Corruption: Readings in Comparative Analysis* (New York: Holt, Rinehart and Winston, 1970), especially Chapters 6 and 9, and John A. Gardiner and David J. Olson, eds., *Theft of the City* (Bloomington, Indiana: Indiana University Press, 1974).

There is no consensus in this literature on the standards appropriate to determining which political acts are "corrupt." For some, political acts which violate the public interest for private interest or gain are corrupt. See, for instance, Arnold A. Rogow and Harold D. Lasswell, Power, Corruption, and Rectitude (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1963), pp. 132–134. In a similar manner, Berg, Hahn, and Schmidhauser define political corruption as a process which "violates and undermines the norms of the system of public order which is deemed indispensable for the maintenance of political democracy." See Larry L. Berg, Harlan Hahn, and John R. Schmidhauser, Corruption in the American Political System (Morristown, New Jersey: General Learning Press, 1976), p. 3. Other authors hold that a political act is corrupt if the public deems it so. See, for example, Arnold J. Heidenheimer's typology of corrupt acts based on this "public opinion" criterion, Political Corruption, pp. 26–28. A third standard for assessing corrupt acts relies predominantly on legal norms: those acts are corrupt which violate the rules or norms of public-office holding for personal or private gain. See J. S. Nye, "Corruption and Political

officials can be expected to engage in corrupt or illegal activities for a variety of reasons. James Q. Wilson summarizes three: (1) the needs and values of particular ethnic groups lead officials identifying with those groups to use their offices illegally to benefit group members with lucrative contracts, jobs, and the like;² (2) "Because each branch can and sometimes does paralyze the other, American government is so constituted that it cannot be carried on without corruption. The boss, the machine, the political party, the bagmen — all these operate ... to concert the action of legally independent branches of government through the exchange of favors";³ and (3)

Development: A Cost-Benefit Analysis," American Political Science Review, 61 (June, 1967), 417-427, and James C. Scott, Comparative Political Corruption (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1972), pp. 3-5 for an excellent discussion of the definitions of political corruption. In this paper we have chosen this latter criterion and define political corruption as engaging in illegal activity for personal or special interest gain.

²James Q. Wilson, "Corruption: The Shame of the States," *The Public Interest*, 2 (Winter, 1966), 28–38, at p. 30, also reprinted in Heidenheimer, pp. 298–306.

³Wilson, p. 31. Here Wilson is referring to an argument made in 1904 by Henry Jones Ford in his "Municipal Corruption," *Political Science Quarterly*, 19 (December, 1904), pp. 673–686. This same theme is developed by Edward M. Sait in "Machine, Political," *Encyclopedia of the Social Sciences*, IX (New York: The Macmillan Company, 1933), 657–661; by Arthur F. Bentley, *The Process of Government*, ed. Peter H. Odegard (Cambridge, Massachusetts: The Belknap Press of Harvard University Press, 1967), Chapter II; and by Robert K. Merton, *Social Structure and Social Theory*, 2nd ed. (New York: The Free Press, 1968), p. 73.

around loose and no one is watching."4

Scholars also seem to agree that officials can often engage in illegal activities without being reprimanded at the polls. Two basic explanations are offered for this. The first claims that public officials can usually conceal their involvement in illegal activities.⁵ Moreover, even when they cannot, the contentiousness of American elections is such that voters discount reports that one or more of the candidates are corrupt, assuming instead that allegations of corruption are merely partisan tricks.6 The other explanation is that officials can trade material benefits (e.g., patronage jobs, the opportunity to gamble) or even appeals to group identification in return for votes.7

What these explanations ignore, and in fact direct attention away from, is whether people would support corrupt politicians if they knew they were corrupt or if they were not themselves benefited materially. How, in other words, would voters weight a candidate's corruption along with other things they know about the candidates in an election? In this paper we identify several conditions under which people will knowingly vote for a corrupt candidate, report some experimental results regarding their operation, and suggest some avenues for additional research. This research does not address the frequency of official corruption, nor does it contest the conclusion that politicians are under no electoral constraint to avoid illegal activities except in the sense that a felony conviction deprives them of

⁴Wilson, "Corruption," p. 31.

5 James Bryce, in analyzing the incidence of corruption in the nineteenth-century Congress, claimed that the "opportunities for private gain are large, the chances of detection small," and because of the relatively short tenure of most members, "the temptation to make hay while the sun shines is all the stronger." See James Bryce, The American Commonwealth, Louis M. Hacker, ed. (New York: G. P. Putnam's Sons, 1959), Vol. I, p. 221.

⁶*Ibid.*, p. 197.

⁷For the typology of incentives alluded to here, see Peter B. Clark and James Q. Wilson, "Incentive Systems: A Theory of Organizations," Administrative Science Quarterly, 6 (September, 1961), 129–166. Solidary inducements are intangible rewards which accrue from socializing or group identification. Banfield and Wilson, for instance, refer to a precinct captain who offers "personal friendship" in return for a vote. See Edward C. Banfield and James Q. Wilson, City Politics (Cambridge, Massachusetts: Harvard University Press, 1963), p. 117. On the use of material incentives to induce electoral support, see Harold F. Gosnell, Machine Politics: Chicago Model, 2nd ed. (Chicago: The University of Chicago Press, 1967), chapter IV.

"men steal when there is a lot of money lying office.8 Rather, our focus is on electoral support for corrupt politicians.

nomenon of electoral support for corrupt poli-

The Ignorant Voter Explanation

There are, of course, reasons why voters would be ignorant or at least uncertain about a candidate's illegal activities. The corrupt candidate usually seeks to conceal this datum from his constituents. Moreover, to the extent that citizens are repelled by political corruption, both candidates in a two-candidate race have similar incentives to accuse each other of being corrupt - even if the charge is false. 9 Because this situation is understood by voters, the credibility of corruption messages during an election - even if true - will normally be quite low. Only when the corruption message comes from a source outside the electoral setting is it likely to be given credibility. For example, Gardiner found that reports by federal lawenforcement agencies about local corruption in one eastern city had a clear impact on three city elections, but similar reporting by local newspapers was discounted in other elections. 10

This explanation implies that people would oppose corrupt candidates if they knew they were corrupt. Without this assumption, any case in which a voter supports a corrupt politician could be explained in one of two ways. Either the voter is in fact ignorant, or he knows the candidate is corrupt and votes for him anyway. Given no a priori reason for assuming that voters always oppose corrupt candidates, and in the absence of independent evidence to that effect, the voter ignorance explanation cannot be accepted as the only possible explanation for voting for a corrupt candidate.

⁸Of course, in principle, politicians, like other citizens, are subject to prosecution for breaking laws, and this threat would seem to constrain their participation in illegal activities. This is not an issue here, since our focus is on the electoral rather than the legal constraint. For a journalistic treatment of the former point, see Jethro K. Liberman, How the Government Breaks the Law (New York: Stein and Day, 1974).

⁹This argument follows from the familiar logic of rational party behavior. See Anthony Downs, An Economic Theory of Democracy (New York: Harper & Row, 1957), chapter 3.

¹⁰John A. Gardiner, The Politics of Corruption: Organized Crime in an American City (New York: Russell Sage, 1970).

The Material Inducement Explanation

The material inducement explanation suggests an incentive that seems sufficient to produce corruption voting. The problem is that in numerous real-world situations, people seem to support corrupt politicians even when material inducements are absent. A voter may be willing to trade his vote to improve his material well being, but whether or not he does so is contingent upon an explicit exchange between the candidate (or his associates) and the voter. For example, a precinct worker would have to arrange the quid pro quo with the voter, or at least the voter would have to understand that he would be rewarded if he voted for the corrupt candidate. Moreover, the inducement would have to be sufficient to change the voter's behavior. The numerous explicit exchanges that would be necessary to control elections would require the presence of large and complex organizations with quite centralized authority structures centered on the corrupt candidates. In this century, such organizations are increasingly rare, not only because people are less willing to respond to material inducements, but because it is expensive for an organization to provide material inducements that are sufficient to change their voting behavior.11 Inducements based on appeals to common interests are also less effective in providing votes for corrupt politicians than they once were. Banfield and Wilson maintain that television and the changing ethnic character of the inner city have made "friendship" harder to give to prospective voters.12 For example, precinct captains have to compete with the evening television viewing habits of citizens for their attention, while white representatives of the political machine find it difficult to become "friends" with the increasing proportion of nonwhites in their precincts.13

One should not construe from what has been said that voters do not support corrupt politicians — rather, in the absence of sufficient organization the material inducement explanation is insufficient to account for why they do so.

Implicit Trading

If the above explanations do not account for corruption voting, what does? The literature on

rational voting in two-candidate elections suggests that whether someone votes for one or the other of the candidates is a function of what he cares about, the intensity of his concern, and whether the candidates take different stands on the things he cares about. Voters may be thought of as evaluating the candidates' positions on items of concern to them, weighting their evaluations according to the strength of their preferences, adding up these weighted evaluations, and then voting for the candidate who receives the highest score. 14

If candidate corruption is treated like any other component in the voter's choice between two candidates, it follows that there are conditions under which a rational voter would knowingly support corrupt candidates. He would do so if he perceives both candidates as corrupt, or if he decides that a corrupt candidate who is closer to his own preferences on other issues is preferable to a "clean" candidate who is not. In either case, he would vote for the corrupt candidate because the candidate shares or is closer to his own political preferences. This explanation differs from the material inducement theory in that the inducement (the candidate's positions) need not be material, and the exchange between the candidate and the voter may be implicit in the sense that no interpersonal negotiations are necessary. The candidate simply takes positions on things and the voter decides those positions weigh more heavily in the candidate's favor than the

14This formulation of rational choice is based on similar models developed by economists and psychologists. An early version of the economic model can be found in Otto A. Davis and Melvin J. Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," in Mathematical Applications in Political Science II, ed. J. L. Bernd (Dallas: Arnold Foundation, Southern Methodist University Press, 1966), 175–205, see also, Otto A. Davis, Melvin J. Hinich and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," American Political Science Review, 64 (June, 1970), 426–448. For the psychological formulation, see Martin Fishbein, "A Behavior Theory Approach to the Relations between Beliefs About an Object and the Attitude toward the Object," in Readings in Attitude Theory and Measurement, ed. Martin Fishbein (New York: John Wiley and Sons, Inc., 1967), pp. 389–400. This approach is applied to voting behavior in Martin Fishbein and Fred S. Coombs, "Basis for Decision: An Attitudinal Approach toward an Understanding of Voting Behavior," paper presented at the sixty-seventh Annual Meeting of the American Political Science Association, Chicago, Illinois, 1971. A useful synthesis of the economic and psychological choice models is presented in Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," American Political Science Review, 63 (December, 1969), 1106–1119.

¹¹Fred I. Greenstein, The American Party System and the American People (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), pp. 47-49.

¹² Banfield and Wilson, p. 122.

^{13&}lt;sub>Ibid</sub>.

knowledge that the candidate is corrupt weighs against him.¹⁵ A corrupt candidate's strategy, therefore, would be to take distinct positions on things many voters care about and thereby arrange as many implicit trades as possible. Hence we refer to this theory as the trading theory of corruption voting.¹⁶

Some Experimental Findings

In order to assess the validity of the above reasoning, we constructed a computer-based experiment to determine whether people always oppose corrupt politicians when they know they are corrupt, and if they do not, whether this reflects implicit trading between candidates and voters. Accomplishing these ends required controlling for the credibility of the corruption message and insuring that only one candidate was viewed as corrupt. We also had to insure the absence of material inducements and the presence of differences between the candidates on items of most concern to the subjects. Our sample was composed of undergraduate students enrolled in introductory political science courses at the University of Illinois during 1972-1973.

In our experiment, subjects received different kinds of information about two candidates in a hypothetical congressional elec-

15At a more general level, James C. Scott's theory of the relationship between changing loyalty patterns and the type of inducements political parties must offer for support fit as well with our own notions of individual candidate and voter choice. For instance, Scott suggests that, as the process of economic growth forges new occupational and class loyalties among former supporters, the political party must change the nature of its inducements "to stress policy concerns or ideology." See James C. Scott, "Corruption, Machine Politics, and Political Change," American Political Science Review, 63 (December, 1969), 1142–1158, at p. 1146.

¹⁶Our distinction between implicit and explicit trading is analogous to that between implicit and explicit logrolling suggested by Buchanan and Tullock. Regarding implicit logrolling, they say:

Here there is no formal trading of votes, but an analogous process takes place. The political "entrepreneurs" who offer candidates or programs to the voters make up a complex mixture of policies designed to attract support. In so doing, they keep firmly in mind the fact that the single voter may be so interested in the outcome of a particular issue that he will vote for the one party that supports their issue, although he may be opposed to the party stand on all other issues.

James M. Buchanan and Gordon Tullock, *The Calculus of Consent: Logical Foundations of Constitutional Democracy* (Ann Arbor: The University of Michigan Press, 1962), pp. 134-135.

tion.¹⁷ On each piece of information, the candidates held opposite positions. The information concerned the candidate's party (Democrat vs. Republican), position on local domestic issues (pro vs. antibusiness), position on President Nixon's Vietnamization policy (hawkish vs. dovish), and relative standing in the polls (ahead, behind). Our subjects were asked to choose between the candidates on the basis of this information in a simulated "opinion poll" (this was not the final vote). Then they were informed that a prominent member of their candidate's own party had withdrawn his endorsement from the subject's most preferred candidate because of alleged illegal activities by the candidate while holding a previous elected position. The message read: "While in the state assembly [their preferred candidate] is alleged to have used his position as chairman of the Public Roads Committee to authorize construction on the new State Route 54 through property his family owned downstate. The sale of the land to the state reportedly brought \$1.3 million." Finally, all subjects were asked to "vote" for one or the other of the two candidates. The purpose of the experiment, conducted during the fall and winter of 1972-1973, was to determine whether or not our subjects would switch from the corrupt candidate. (A detailed description of the experiment is included in the Appendix.)

To determine the extent to which our subjects remained with the corrupt candidate, we utilized generalized least squares regression analysis to estimate the following equation:

(1)
$$Y = A + b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_5(X_1X_2) + b_6(X_1X_3) + b_7(X_1X_2X_4) + b_8(X_1X_3X_4) + U,$$

where:

Y = 1 if subject remained with the corrupt candidate,

0 otherwise;

X₁ = 1 if subject received the party affiliation of the candidates,

0 otherwise;

X₂ = 1 if subject received the Vietnam positions of the candidates,

0 otherwise;

17The experiment was conducted on the PLATO III computer-based education system at the University of Illinois at Urbana-Champaign. A description of this system and the experiment is contained in the Appendix. For a more detailed description of the PLATO system, see D. Alpert and D. Bitzer, "Advances in Computer-Based Education," Science, 167 (March, 1970), 1582–1590.

X₃ = 1 if subject received the domestic policy positions of the candidates,

0 otherwise;

 $X_4 = 1$ if subject received the public opinion polls,

0 otherwise;

and where $X_2X_3=0$ and where U is an error term with E(U)=0 by assumption.¹⁸ Estimation of this equation yields a conditional probability model in which, for example, the probability of remaining with the corrupt candidate for subjects who received only the party affiliations of the candidates is A+b (i.e., $X_1=1$; $X_2=X_3=X_4=0$).

The estimated coefficients are presented in Table 1 with the conditional probabilities of remaining with the corrupt candidate derived from these coefficients presented in Table 2.¹⁹ In examining Table 2, two results are apparent:

18 For descriptions of generalized least squares regression, especially with dummy variables and limited dependent variables, see J. Johnston, Econometric Methods, 2nd ed. (New York: McGraw-Hill, 1972), pp. 208–242 and 176–186; and Arthur S. Goldberger, Econometric Theory (New York: John Wiley & Sons, Inc., 1964), pp. 251–255.

¹⁹Initially there were 217 subjects in our experiment but nine were dropped because of missing data on one or more of the questionnaire items. Also, note that the F-levels reported in Table 1 as well as those in

(1) information about the candidates does make a difference in the probability of remaining with the corrupt candidate, and (2) all types of information are not equal; certain kinds of information induce more voting for the corrupt candidate than do other kinds of information. In particular, we note that the single most powerful piece of information was that on the candidates' Vietnam positions; subjects who received this information had a probability of .44 of voting for the corrupt candidate whereas subjects who received no information had a zero probability of voting for the corrupt candidate. Also note the minimal effect of party information. For subjects who received only the party affiliations of the candidates, the probability of voting for the corrupt candidate was .18, even lower than the probability for those who only received poll information. Moreover, neither party nor domestic policy information seems to make much difference when the two are combined; subjects receiving

Tables 3 and 5 indicate that each variable makes a significant contribution to the explanatory power of the estimated equations. However, as is evident, from an examination of the standard errors reported in these tables, several of the estimated coefficients are not significantly different from zero. Hence, care must be taken not to attribute too much to the exact values of the coefficients.

Table 1. Estimated Coefficients for Initial Conditional Probability Model

Tern	1 .	Coefficient	Standard Error	F Level
A .	(Constant)	.00	.09	
b ₁	(Party)	.18	.13	4.51
$\hat{b_2}$	(Vietnam)	.44	.14	16.00
b3	(Domestic)	.37	.13	11.08
b4	(Poll)	.20	.14	8.02
b ₅	(Party-Vietnam)	12	.19	3.43
b ₆	(Party-Domestic)	37	.18	6.35
b ₇	(Party-Vietnam-Poll)	17	.18	3.92
bg.	(Party-Domestic-Poll)	.03	.19	5.28
(N =	208)			

Note: Multiple R = .37

Table 2. Conditional Probabilities of Remaining with the Corrupt Candidate Estimated from Table 1

Item	
P (Remain/No Information)	= .00 = A
P (Remain/Party Information)	$= .18 = A + b_1$
P (Remain/Party, Domestic Information)	$= .18 = A + b_1^2 + b_3 + b_6$
P (Remain/Poll Information)	$= .20 = A + b_4$
P (Remain/Party, Domestic, Poll Information)	$= .35 = A + b_1 + b_3 + b_4 + b_6 + b_8$
P (Remain/Domestic Information)	= .37 = A + b3
P (Remain/Vietnam Information)	$= .44 = A + b_2$
p (Remain/Party, Vietnam Information)	$= .50 = A + b_1^{2} + b_2 + b_5$
P (Remain/Party, Vietnam, Poll Information)	$= .53 = A = b_1^2 + b_2^2 + b_4 + b_5 + b_7$
	1 2 . 0 ,

the combination package voted similarly to subjects who received only party information. Interestingly, however, when poll information is added to this combination, our subjects, for some reason, acted much like those who received only domestic policy information.

In constructing Tables 1 and 2 and in estimating the previous regression equation, we have implicitly assumed that all our subjects would attach identical importance to each of the pieces of information they encountered in the experiment. This assumption is inappropriate, however, since the implicit trading theory assumes that people are likely to feel more strongly about some things than about others. We thus ask: "Does the intensity with which our subjects hold their positions affect their tendency to vote for corrupt politicians?" To answer this question, we asked our subjects to indicate, on a paper-and-pencil questionnaire administered about two weeks before they participated in the experiment, how strongly they felt about President Nixon's Vietnamization policy, about their own party identification, and about local zoning policies. In each case their preferences were registered on a seven-point scale ranging from "strongly favor" to "indifferent" to "strongly oppose." (The party identification item asked whether they were Democrats, Republicans or Independents; and if they were partisan identifiers, how strongly they identified with their party. Independents were asked if they were closer to the Republican or Democratic party.)20 Consistent with the implicit trading theory, we hypothesized that subjects who felt strongly about party, Vietnam, and local zoning policies (i.e., those in scale positions "1," "2," "6," and "7" on the seven-point scales) would be more likely to stick with the candidate who was closer to their position even if he was corrupt.21

To test this hypothesis, we estimated the following equation:

²⁰This is the standard party identification item used by the Survey Research Center, University of Michigan, for their Presidential Election Studies.

²¹For the purposes of our experiment, we have called respondents "intense" when they recorded their feelings in position 1, 2, 6, or 7 on our seven-point scales:

This measurement technique is very similar to the one employed by Martin Fishbein to measure the "evaluative aspects of belief statements." See, Fishbein and Coombs, "Basis for Decision," p. 12.

(2) $Y = A + b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_5(X_1X_2) + b_6(X_1X_3) + b_7(X_1X_2X_4) + b_8(X_1X_3X_4) + b_9(X_1X_5) + b_{10}(X_2X_6) + b_{11}(X_3X_7) + b_{12}(X_1X_2X_5X_6) + b_{13}(X_1X_3X_5X_7) + U,$

where Y, X_1, X_2, X_3, X_4 , and U are identical to those in equation (1), and where:

X₅ = 1 if subject had an intense party preference,

0 otherwise;

 $X_6 = 1$ if subject had an intense position on Vietnam,

0 otherwise;

X₇ = 1 if subject had an intense position on domestic policy (local zoning),

0 otherwise.

The estimated parameters for this equation, again using the generalized least squares procedure, are presented in Table 3, and the conditional probabilities derived from these parameters are presented in Table 4. Unfortunately, none of our subjects was intensely concerned about local zoning (our domestic policy information), and hence no b₁₁ or b₁₃ appear in Table 3 and no conditional probabilities are given in Table 4.

As hypothesized, the conditional probabilities of voting for the corrupt candidate were much larger for those who felt intensely about Vietnam than for those who were indifferent on this issue dimension. In particular, those who were intense about Vietnam had a conditional probability of voting for the corrupt candidate that was .34 larger than that for those indifferent on Vietnam. Interestingly, however, intense party identifiers had a lower probability of voting for the corrupt candidate than did those without a strong party identification. Party identifiers were more likely to switch and vote for the "clean" candidate when they were informed that their initial choice was corrupt, while independents tended to remain with the corrupt candidate.

Finally, it follows from the implicit trading theory that, if voters have an intense preference for honesty in government, they will be more likely to switch from the corrupt candidate than if this is not the case. Accordingly, we asked our subjects to indicate how intensely they valued the concept of "honesty in government" on a seven-point scale from "strongly for" to "strongly opposed."²² We then con-

²²For the "Honesty in Government" question, a respondent was called "intense" if a 6 or 7 was marked on the item.

Table 3. Estimated Coefficients for the Preference Intensity Model

Term	l	Coefficient	Standard Error	F Level
A	(Constant)	.00	.09	
b ₁	(Party)	.21	.14	5.83
b_2	(Vietnam)	.29	.15	11.43
b_3	(Domestic)	.37	.12	13.94
b4	(Poll)	.20	.13	9.12
b ₅	(Praty-Vietnam)	14	.19	4.30
b ₆	(Party-Domestic)	37	.18	7.60
b ₇	(Party-Vietnam-Poll)	14	.18	5.24
b ₈	(Party-Domestic-Poll)	.04	.18	3.89
bg	(Party Identifier)	08	.09	4.71
b10	(Vietnam Intense)	.34	.12	24.34
b11	(Party Identifier and Vietnam Intense)	10	.18	6.59

Note: Multiple R = .43

Table 4. Conditional Probability of Voting for a Corrupt Candidate^a

	Nonintense on Either Party Intense on or Vietnam Party		Intense on Vietnam	Intense on Both Party and Vietnam	
P (Remain/No Information)	.00 (.00) ^b				
P (Remain/Party, Domestic, Poll)	.17 (.18)	.09			
P (Remain/Poll)	.20 (.20)				
P (Remain/Party)	.21 (.18)	.13			
P (Remain/Party, Domestic)	.21 (.18)	.13			
P (Remain/Vietnam)	.29 (.44)		.63		
P (Remain/Party, Vietnam)	.36 (.50)	.38	.70	.63	
P (Remain/Domestic)	.37 (.37)				
P (Remain/Party, Vietnam, Poll)	.42 (.52)	.34	.76	.68	

^aSubjects who did not receive a particular piece of information are considered nonintense on that information on the assumption that what they did not know could not affect their behavior.

^bFor comparative purposes, the conditional probabilities of Table 2 are repeated here in parentheses.

structed the following variable:

X₈ = 1 if subject is strongly for honesty in government, 0 otherwise;

and re-estimated equation 2 (with b_{14} being the coefficient for X_8). The estimated coefficients are presented in Table 5.

Table 5 shows that, under all of the various information conditions, subjects who intensely preferred honesty in government were less likely to vote for the corrupt candidate. Those with intense preferences for honesty in government had a .08 smaller probability of voting for the corrupt candidate than did subjects who expressed indifference about this concept.

The relatively small number of subjects in this exploratory experiment renders these findings tentative and precludes more detailed analysis. The present findings do seem to lend support, however, to the implicit trading explanation of voting for corrupt politicians. Especially noteworthy is the effect of knowledge of the candidates' issue positions. Information on both the Vietnam and the domestic policy positions of the candidates affected voting for corrupt politicians, while information on the candidates' party seems to have had little effect.

Finally, we should note what happens when the two major assumptions built into our experiment are relaxed. Our theory suggests that an incentive exists for voters to support corrupt politicians even when the message that they are corrupt is credible. If it is not credible, voters should be even more likely to support corrupt politicians, as long as there remains a difference in the candidates' policy positions. But if these latter differences disappear, then even an uncertain corruption message should produce disaffection from the culprit, because the costs to the voter of switching from a "maybe corrupt" candidate to one without the taint of corruption would be minimal. The

Table 5. Estimated Coefficients for Honesty in Government Model

Term		Coefficient	Standard Error	F Level
A	(Constant)	.11	.11	
bi	(Party)	.14	.14	4.89
b_2	(Vietnam)	.24	.15	9.81
b3	(Domestic)	.31	.13	8.22
b4	(Poll)	.14	.14	4.42
b ₅	(Party-Vietnam)	07	.20	3.36
b ₆	(Party-Domestic)	30	.18	7.06
b7	(Party-Vietnam-Poll)	08	.18	3.64
bg.	(Party-Domestic-Poll)	.11	.19	13.25
bg	(Party ID)	08	.09	5.46
b10	(Vietnam Intense)	.32	.12	23.34
b12	(Party ID and Vietnam Intense)	10	.18	4.01
b ₁₄	(Intense on Honesty)	08	.07	6.22
(N =	208)			

Note: Multiple R = .44

extent of defection in this case should be a material inducements among voters. Moreover, function of how strongly the voter values to the extent that machine politicians rely on honesty in government.

Discussion

One implication of the theory that voters trade implicitly with corrupt politicians is that the pool of people with whom corrupt politicians can at least potentially trade is much larger than the number who would vote for them because of material inducement(s). Using only explicit trading and material inducements, corrupt politicians would be restricted to the number of people with whom they could barter, either directly or through lieutenants. Since such trading is costly in organizational resources, this number would be relatively small. Through implicit trading of nonmaterial inducements, however, corrupt politicians can trade with everyone who is aware of the election, has one or more intense preferences and/or is not especially enthusiastic about honesty in government. In the United States, with its practically universal literacy and welldeveloped media, this would effectively mean most voters. While the number of attempted trades would probably be restricted for strategic reasons (e.g., to form minimum winning coalitions), it would seem that there are so many potential voters with whom corrupt politicians could trade that the latter need not fear electoral reprisal for their indiscretions.

A second implication concerns the conventional wisdom that political machines flourish or perish as a function of the political utility of material inducements. The present analysis suggests that machine politicians who are corrupt may owe their re-election to implicit trading as much as or more than to the distribution of

material inducements among voters. Moreover, to the extent that machine politicians rely on implicit trading, they are no different from nonmachine corrupt politicians — an assertion which runs counter to the Progressives' assumption that abolishing machines would decrease the likelihood of corrupt officials being reelected.²³

A third inference of the implicit trading theory is that corrupt candidates have even more incentive to take distinct issue positions than do noncorrupt candidates. Contrary to the theory that candidates converge on the issue position occupied by the largest number of voters, the exigencies of American elections (such as the need to amass financial contributions and to obtain primary as well as general election victories) apparently lead candidates to move somewhat away from the modal position. Corruption would seem to accentuate this divergence.²⁴

Finally, at least on the surface, the implicit trading theory seems to account for the tendency of voters and others to focus on official corruption during periods of economic problems. When voters' expectations of policy satisfaction are low, office holders are more likely to be held accountable for illegal activity (e.g.,

²³For related speculation, see Edward Banfield, *Political Influence* (New York: The Free Press, 1961), p. 259.

²⁴The best statement of the convergence model is given by Downs, *Economic Theory of Democracy*. Multidimensional versions of this model are presented in Otto Davis et al., "Development of a Mathematical Model." For a demonstration of divergence in the 1968 presidential election, see Benjamin I. Page, "Presidential Campaigning, Party Cleavage, and Responsible Parties," unpublished paper, University of Chicago, 1974.

the recent ousters of national leaders in Japan, England, West Germany, Italy, and the United States) — albeit not always at the polls.

Conclusions

Our purpose has been to examine the notion of electoral support for corrupt politicians. We have isolated three different, though not mutually exclusive, explanations of why people would vote for corrupt politicians and have suggested that the implicit trading explanation accounts for large-scale corruption voting under modern conditions, whereas the other two could not.

In conclusion, this paper is only a first step. The implicit trading theory simply rescues for research the role of political corruption as a component in individual voting decisions, a subject that the ignorant voter theory and the material inducement theory tend to conceal. We leave for future research a host of additional questions about this subject. For example, would some kinds of candidate corruption be weighted more or less heavily than our illegal land sale case? Are people with relatively sophisticated political attitudes (e.g., those with much political information and/or ideologies which link policy, institutional, and candidate information in causal patterns) more likely than others to devalue official corruption? Or do the politically sophisticated value honesty in government more highly or weight this value more heavily in making voting decisions than other

In addition, we suggest that these and other questions about the role of candidate corruption in the voters' choice can fruitfully be studied in experimental as well as natural research settings. The exploratory experiment reported on here only begins to tap the full power of this research procedure.

²⁵James Q. Wilson and Edward Banfield, "Political Ethos Revisited," American Political Science Review, 65 (December, 1971), 1048–1062. See also, Herbert McClosky, "Consensus and Ideology in American Politics," American Political Science Review, 58 (June, 1964), 361–382, showing that more political leaders than followers advocate clean government.

On the relationship between economic conditions and voting, see Gerald K. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964," *American Political Science Review*, 65 (March, 1971), 131–143.

²⁶For attempts to explain recent trends concerning trust in government, see Arthur H. Miller, "Political Issues and Trust in Government: 1964–1970," American Political Science Review, 68 (September, 1974), 951–972, and Jack Citrin, "Comment: The Political Relevance of Trust in Government," American Political Science Review, 68 (September, 1974), 073–088

Appendix

To better explain the procedure employed, let us take the fourth treatment (Vietnam Policy/Party Identification/Opinion Poll-Preference/Corruption-Vote) and follow it through the election, displaying the experiment information as it appeared on the subject's TV monitor. After the introductory material and random respondent assignment to the fourth experimental group, the student was introduced to the election and the candidates.

Screen 1:

THE TWO CANDIDATES IN OUR VOTING SIMULATION ARE RUNNING FOR THIS DISTRICT'S SEAT IN THE U.S. HOUSE OF REPRESENTATIVES, NEITHER CANDIDATE IS THE INCUMBENT, i.e., NEITHER HOLDS THIS OFFICE TODAY.

Screen 2:

CANDIDATE JONES IS NOW A LAWYER IN UNIVERSITY TOWN AND IS ON THE GOVERNOR'S COMMITTEE ON THE STATE'S ECONOMY.

CANDIDATE BROWN IS ALSO A LAWYER IN UNIVERSITY TOWN AND SERVES AS SPECIAL LEGAL COUNSEL FOR THE STATE BOARD OF EDUCATION.

Care was taken in designing the information contained in Screens 1 and 2 to control any independent effects the information might have on the results. Neutral candidate names, "Jones" and "Brown," were used to control any identification with a "real world" politician. The automatic advantage an incumbent enjoys was neutralized, while differences in candidate career and background information were minimized.

The Vietnam policy message was the next manipulation our respondent received:

Screen 3:

CANDIDATE JONES WAS A VOCAL OPPONENT OF THE VIETNAMESE WAR SINCE 1966. CANDIDATE BROWN SUPPORTED PRESIDENT NIXON'S VIETNAMIZATION POLICY SINCE 1968.

Policy positions on Vietnamization were chosen for the foreign policy manipulation because of its obvious saliency among the intended audience, college undergraduates, and because of its potential impact on attitude change.

Following the Vietnam policy statements, the respondent received the political party affiliations of the candidates.

Screen 4:

CANDIDATE JONES IS A DEMOCRAT. CANDIDATE BROWN IS A REPUBLICAN.

Next the respondent received information in the form of an opinion poll.

Screen 5:

THE INDIVIDUALS WHO PRECEDED YOU IN THIS EXPERIMENT GAVE THE FOLLOWING INDICATION OF THEIR CANDIDATE PREFERENCE AT THIS POINT:

CANDIDATE BROWN	45%
CANDIDATE JONES	55%

Although the students were told that the poll results were based on the responses of previous subjects, in fact the percentages reported for the two candidates were randomly generated with the only contraints being that each respondent viewed different percentages and that they add to 100%.

The presentation of the opinion poll results concluded the "information" component of the experiment. On the basis of this information, subjects assigned to experimental route 4 were then asked which of the two candidates they preferred so far in the election.

Screen 6:

TO UPDATE OUR OPINION POLL, PLEASE INDICATE YOUR PREFERENCE OF THE TWO CANDIDATES AT THIS POINT IN THE ELECTION.

CANDIDATE JONES CANDIDATE BROWN PLEASE TYPE IN THE LAST NAME OF THE CANDIDATE OF YOUR CHOICE.

Whichever candidate was preferred by the subject, he was informed of that choice's corrupt behavior. Let us assume that in Screen 6 above, our student preferred Candidate Jones.

Screen 7:

THE DEMOCRATIC SENATOR FROM THE STATE HAS JUST ANNOUNCED THAT HE IS WITHDRAWING HIS ENDORSEMENT FOR CANDIDATE JONES. HE EXPLAINED HIS ACTIONS AS FOLLOWS: "ALTHOUGH CANDIDATE JONES SHOULD BE PRESUMED TO BE INNOCENT UNTIL THE CRIMINAL SUIT BROUGHT AGAINST HIM HAS BEEN TRIED IN A COURT OF LAW, I FEEL IT IS IN THE BEST INTERESTS OF THIS STATE THAT CANDIDATE JONES TERMINATE HIS STATE RELATED ACTIVITIES."

Screen 8:

WHILE IN THE STATE ASSEMBLY, CANDIDATE JONES IS ALLEGED TO HAVE USED HIS POSITION AS CHAIRMAN OF THE PUBLIC ROADS COMMITTEE TO AUTHORIZE CONSTRUCTION OF THE NEW STATE ROUTE 54 THROUGH PROPERTY HIS FAMILY OWNED DOWNSTATE. THE SALE OF THE LAND TO THE STATE REPORTEDLY BROUGHT 1.5 MILLION DOLLARS.

If Candidate Brown was chosen, the source was a Republican governor; otherwise the message was the same. After receipt of the information that his preferred candidate was corrupt, the respondent was asked to cast his final ballot. This concluded the experiment.

Screen 9:

OFFICIAL BALLOT

PLEASE VOTE FOR THE CANDIDATE OF YOUR CHOICE BY TYPING IN HIS LAST NAME.

CANDIDATE JONES (D)CANDIDATE BROWN (R)

The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory*

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Introduction

In the intellectual quest for a systematic understanding of the etiology of political and collective violence, relative deprivation theory has emerged as a prevalent explanatory device. One form of relative deprivation theory has received particularly strong scholarly attention through theoretical elaboration and empirical application to a variety of significant episodes of political and collective violence. This form is the progressive variety of relative deprivation and is most commonly referred to as the "J-curve."

*This research was funded by the University of Concinnati Research Council through an award to Professor Miller. The data were obtained from the Inter-University Consortium for Political Research through the facilities of the University of Cincinnati Behavioral Sciences Laboratory. The authors alone are responsible for the use and interpretation of the data. At the time of this study, Louis H. Bolce was a Charles Phelps Taft Graduate Fellow in Political Science and Mark Halligan was an Undergraduate Fellow in Political Science. William Klecka and Alfred Tuchfarber, University of Cincinnati, Ted Robert Gurr, Northwestern University, and four anonymous reviewers contributed useful and insightful suggestions which are gratefully acknowledged.

¹James C. Davies, "Toward a Theory of Revolution," in *Studies in Social Movements*, ed. Barry McLaughlin (New York: The Free Press, 1969), pp. 85–109. Hardly an anthology on revolution or social movements exists without including this work. James A. Geschwender, "Explorations in the Theory of Social Movements and Revolutions," *Social Forces* 47, 2 (December, 1968), 127–135, has attempted to elaborate the theory in terms of cognitive dissonance.

The J-curve resulted from an attempt by James C. Davies to reconcile what he perceived as two antithetical explanations of the phenomenon of revolution. According to Davies, two of the most famous students of revolution, Karl Marx and Alexis de Tocqueville, had arrived at completely opposite conclusions about the causes of revolution. Drawing on Marx's work in The Communist Manifesto, Davies argued that Marx perceived revolutions as most likely to occur when things get worse, when the misery of the proletariate increased relative to the economic standard of living of the bourgeoise. In contrast, Tocqueville had advanced the theory that revolutions occurred when a previously oppressive regime released the yoke and created expectations it could not fulfill.2

Davies attempted to reconcile these divergent explanations by arguing that each of these perspicacious students of revolution had accurately rendered part of the picture of revolu-

²Alexis de Tocqueville, *The Old Regime and the French Revolution*, translated by Stuart Gilbert (Garden City, N.J.: Doubleday Anchor, 1955), p. 177. Tocqueville's observation that those parts of France which experienced the greatest degree of positive change also showed the greatest popular discontent is sometimes referred to as Tocqueville's paradox. Two different explanations of the paradox exist. The most common, and the one used by Davies and in this research, is the rising expectations thesis. A less well-known interpretation is referred to as "the present value of the past." This argues that present achievement is devaluated by the perception of past costs. See, in this regard, Charles Wolf, Jr., "The Present Value of the Past," mimeo (Santa Monica: The Rand Corporation, 1969).

tion, but not the total picture. The complete depiction of the causes of revolution awaited the fashioning of both components into a single entity, which Davies engagingly accomplished. Davies argued that revolutions occur when a period of progress is followed by a period of sharp reversal and decline.

This rise-and-decline phenomenon can be read as: first Tocqueville then Marx. As Davies notes: "Revolutions are most likely to occur when a prolonged period of objective economic and social development is followed by a short period of sharp reversal. The all-important effect on the minds of people in a particular society is to produce, during the former period, an expectation of continued ability to satisfy needs — which continue to rise — and, during the latter, a mental state of anxiety and frustration when manifest reality breaks away from anticipated reality." The theory is depicted in Figure 1 below.

³Davies, "Toward a Theory of Revolution," p. 86.

At a conceptual level the theory has a great deal of appeal. It is the type of theory that is very attractive to social scientists. It reconciles apparent contradictions in the work of two leading theorists and creates a parsimonious explanatory structure. In the search for conceptual simplicity, clarity and phenomenological regularity, it yields, at a minimum, heuristic success and beyond that has had ostensible empirical corroboration.

From the perspective of empirical theory, however, there are several primary empirical, methodological, and conceptual problems inherent in the structure and application of the J-curve. These problems have consequences for the J-curve theory specifically and relative deprivation theory generally.

It is the purpose of this research to examine some of these problems in the context of the application of the J-curve to the black urban riots. Although the primary objective of the research is to test the J-curve theory, consideration will also be given to the substantive

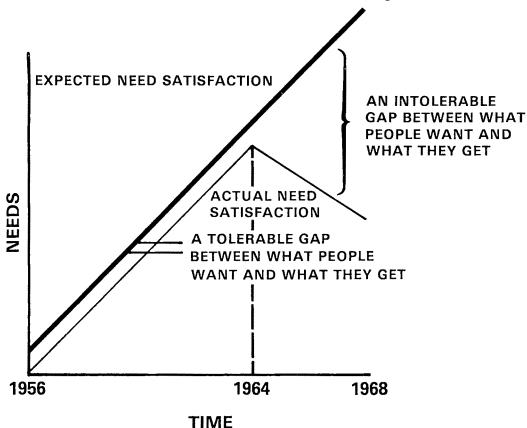


Figure 1.
Pictorial Representation of Davies's J-Curve (Adapted from James C. Davies, "The J-Curve...," p. 691)

implications of the analysis as it affects our understanding of the riots.

Seemingly, a number of different events could have been chosen to examine the theoretical and empirical application of the J-curve. The black urban riots were chosen, in part, because Davies, himself, has applied his theory to this set of events.4 Consequently, it is possible to follow his operationalizations, avoiding questions of operational comparability, and to assess the theory in an appropriate empirical setting. Moreover, one of the major criticisms which we level against the application of the J-curve, and most relative deprivation theories, is the problem of reductionism, sometimes referred to in the philosophy of science as the level-of-analysis issue: the validity of using data and concepts developed at one level of analysis to test theories formulated at a different level.⁵ (This problem is comparable to the one raised by W. S. Robinson's criticism of the use of aggregate correlations to draw inferences concerning the behavior of individuals.) A variety of data exists for the study of the black urban riots, making it possible to conduct analytical comparisons and reconstruct operationalizations at various analytical levels. As a result, the level-of-analysis problem can be empirically assessed rather than simply noted as a caveat affecting the research.

The Black Urban Riots and Relative Deprivation Theory

In his review of the sociological literature on the black urban riots, Clark McPhail⁷ notes that

⁴James C. Davies, "The J-Curve of Rising and Declining Satisfactions as a Cause of Some Great Revolutions and a Contained Rebellion," in *The History of Violence in America*, ed. Hugh Davis Graham and Ted Robert Gurr (New York: Bantam Books, 1970), pp. 690-731.

⁵In the philosophy of science this problem is sometimes alluded to as a problem in reductionism. See, for example, May Brodbeck, "Methodological Individualism: Definition and Reduction," in *Readings in the Philosophy of the Social Sciences*, ed. May Brodbeck (New York: The MacMillian Co., 1968).

6W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," American Sociological Review, 15 (June, 1950), 351-357. See also: L. A. Goodman, "Some Alternatives to Ecological Correlation," American Journal of Sociology, 64 (May, 1959), 610-625; O. D. Duncan and B. Davis, "An Alternative to Ecological Correlation," American Sociological Review, 18 (December, 1953), 665-666. O. D. Duncan, R. P. Cuzzort, and B. Duncan, Statistical Geography (Glencoe: The Free Press, 1961), note, as we do, that the problem arises wherever the units of analysis are changed. The change need not be from one distinct type of unit (e.g., individual) to a different type (e.g., group).

⁷Clark McPhail, "Civil Disorder Participation: A Critical Examination of Recent Research," American

research designs based on relative deprivation theory have played a prominent role in the attempts to explain these events. The results of these studies, in making a case for relative deprivation as a primary explanation of the riots, have been largely disappointing, despite some rather sophisticated and well-conceived methodological procedures.⁸ As McPhail notes "... there is considerable reason for rejecting the sociological and popular cliche that absolute or relative deprivation and the ensuing frustration or discontent or despair is the root cause of rebellion."9 On the basis of his observations, McPhail suggests reconsideration of Gurr's assertion that relative deprivation is "...as fundamental to understanding civil strife as the law of gravity is to atmospheric physics. . . . "10

Davies, in his assessment of the urban riots, strikes a more confident note: "The Negro rebellion appears to have been preceded by the same J-curve of expectations that are first gratified and then frustrated." In contrast to Davies's work, the studies cited by McPhail are generally based on analyses of socioeconomic and political characteristics of riot versus non-riot cities. Relative deprivation is usually measured in these studies by comparing various socioeconomic, census data characteristics of the black community with those of the white community. The underlying logic is that the riot cities should manifest greater disparities along these dimensions than the nonriot cities.

Davies's work is based on population characteristics; nonetheless, these are also aggregates.

Sociological Review, 36 (December, 1971), 1058-1073.

⁸See, in this regard, Milton Bloombaum, "The Conditions Underlying Race Riots as Portrayed by Multidimensional Scalogram Analysis: A Reanalysis of Lieberson and Silverman's Data," American Sociological Review, 33 (February, 1968), 76–91; Bryan T. Downes, "Social Characteristics of Riot Cities: A Comparative Study," Social Science Quarterly, 49 (December, 1968), 504–520; Stanley Lieberson and Arnold Silverman, "The Precipitants and Underlying Conditions of Race Riots," American Sociological Review, 30 (December, 1965), 887–898; Seymour Spilerman, "The Causes of Racial Disturbances: A Comparison of Alternative Explanations," American Sociological Review, 35 (August, 1970), 627–649; and Jules Wanderer, "An Index of Riot Severity and Some Correlates," American Journal of Sociology, 74 (March, 1969), 500–505.

McPhail, "Civil Disorder Participation," p. 1064.

10 Ted Robert Gurr, "Urban Disorder: Perspectives
From the Comparative Study of Civil Strife," in Riots
and Rebellion: Civil Violence in the Urban Community, ed. Louis H. Mascotti and Don R. Bowen
(Beverly Hills: Sage Publications, 1968), p. 52.

11 Davies, "The J-curve," p. 717.

The index for measuring relative deprivation is constructed by dividing family income by average years of schooling for the total and nonwhite populations. The ratio of the resulting nonwhite quotient to the white quotient is described as a measure of nonwhite satisfaction. The nonwhite satisfaction ratio is then plotted from 1940 to 1967 to determine whether a J-curve can be traced for the period of the riots. The numeric data and plot are presented in Table 1 and Figure 2 below.

Although in some remote fashion Figure 2 might be construed to illustrate a J-curve, the reversal in need fulfillment takes place during the wrong years — predicting a peak of unrest in the mid-1950s. Despite this, the data from the black urban riots are not seen as disconfirming the theory. Davies simply argues that the riots needed the exacerbation of southern violence a decade later to produce the aggression that began with the frustration demonstrated by the ratio of education to income. ¹²

12 Ibid., p. 724.

This eclectic interpretation of the data raises issues concerning the validity of the J-curve as an explanation of the urban riots. Moreover, Davies's logic, as that of his compeers in

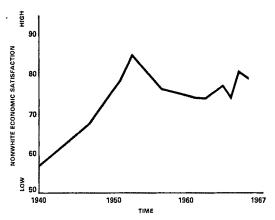


Figure 2.
Index of Nonwhite Economic Satisfaction
(Adapted from James C. Davies,
"The J-Curve...," p. 724)

Table 1. Davies's Origin and Time Sequence of Black Frustration (adapted from James C. Davies, "The J-Curve...," p. 726)

	Average Family I Average Year	ncome Divided by s of Schooling	Nonwhite Satisfaction	Nonwhite Frustration	
Year	Total Population	Nonwhite Population	Column 2 as % of Column 1	Column 3 from 100%	
1940	\$1231 = 146.3 8.4	\$ 489 = 84.3 5.8	57.5	42.5	
1947	$\frac{3031}{9.0} = 336.8$	$\frac{1614}{6.9} = 233.9$	69.4	30.6	
1950	$\frac{3319}{9.3} = 356.9$	$\frac{1869}{6.8} = 274.9$	77.0	23.0	
1952	$\frac{3890}{10.1} = 385.1$	$\frac{2338}{7.1} = 329.3$	85.8	14.5	
1957	$\frac{4971}{10.6} = 469.0$	$\frac{2764}{7.7} = 359.0$	76.5	23.5	
1960	$\frac{5620}{10.6} = 530.2$	$\frac{3233}{8.2} = 394.3$	74.4	25.6	
1962	$\frac{5956}{11.4} = 522.5$	$\frac{3330}{8.6} = 387.2$	74.1	25.9	
1964	$\frac{6559}{11.7} = 560.6$	$\frac{3839}{8.9} = 431.3$	77.0	23.0	
1965	$\frac{6951}{11.8} = 589.6$	$\frac{3995}{9.0} = 443.8$	75.3	23.7	
1966	$\frac{7436}{12.0} = 619.7$	$\frac{4628}{9.2} = 503.0$	81.2	18.8	
1967	a a	a a	78.9	20.2	

^aAbsent on original.

sociology cited by McPhail, ¹³ is not congruent with the empirical and theoretical requirements of relative deprivation theory. The requisite level of analysis has been violated in the assertion that the presence of aggregate, non-perceptual characteristics ostensibly related to black frustration is an appropriate indication that individual black people are indeed experiencing frustration.

The importance of this logical gap is best understood by looking at the psychological basis of relative deprivation theory. Relative deprivation theory claims theoretical descent from the psychological research of John Dollard and his Yale colleagues. ¹⁴ The Yale team established what has come to be known as the frustration-aggression hypothesis. This hypothesis, as it has evolved, proposes that frustration produces various responses, one of which is aggression, and if nonaggressive responses do not relieve the frustration, the probability of an aggressive response increases. ¹⁵

Relative deprivation theory has built upon this hypothesis in viewing relative deprivation as a major component of the chain of events leading to aggression. It is this deprivation that produces frustration; frustration produces aggression; aggression produces civil violence; and civil violence can be translated into political violence. ¹⁶

The language of relative deprivation theory is replete with reference to this psychological foundation. As Davies notes: "Political stability and instability are ultimately dependent on a state of mind, a mood, in a society. Satisfied or apathetic people who are poor in goods, status, and power can remain politically quiet and their opposites can revolt..." 17

Relative deprivation is, thus, an individual experience, and, despite objective, aggregate indicators, relative deprivation can only take place if people perceive themselves to be deprived. Economic indicators may demonstrate that the availability of goods, status, or power may be low or may have diminished

from some previous level, but this does not demonstrate that people perceive themselves to have been deprived and are consequently discontent.

With the exception of work by Grofman and Muller, ¹⁸ relative deprivation theorists generally use the language and theoretical foundation of individual data but perform their empirical testing with aggregate social and economic indicators. The chasm that separates objective societal conditions from subjective perceptions seems to have trapped many researchers who have made enthusiastic and confident assertions about the perceptual basis of individual behavior from theoretically remote aggregate indicators. ¹⁹

Given the above considerations, how would Davies's index appear if he had obtained individual-level data (overlooking momentarily the entire question of perceptual vs. nonperceptual data and simply looking at the difference in the unit of analysis)? From our individual data, taken from the Michigan Survey Research Center election studies from 1956–1968, Davies's operationalizations were replicated and his index was reconstructed. The data are displayed below for both whites and blacks and by region in Table 2 and Figure 3.

The above data and Figure 3 do not demonstrate anything as parsimonious as a J-curve. The index of black frustration varies without consistent pattern, and the overall impression it imparts is that the differences between blacks and whites are so trivial as to be substantively inconsequential. In four out of the seven years analyzed, blacks, contrary to the interpretation originally derived from Davies's data, receive a slightly higher income return for years of

¹⁸Bernard N. Grofman and Edward N. Muller, "The Strange Case of Relative Gratification and Potential for Political Violence: The V-Curve Hypothesis," *The American Political Science Review*, 67 (August, 1973), 514–539.

19 In addition to the sociological literature noted above in footnote 8, see, for example: Ivo K. Feierabend, R. L. Feierabend, and B. A. Nesvold, "Social Change and Political Violence: Cross-National Patterns," in Anger, Violence and Politics: Theories and Research, ed. Ivo K. Feierabend, Rosalind L. Feierabend, and Ted Robert Gurr (Englewood Cliffs, N.J.: Prentice Hall, Ivo K. Feierabend, Rosalind L. Feierabend, "Aggressive Behaviors within Polities 1948–1962: A Cross-National Study," Journal of Conflict Resolution, 10 (September, 1966), 249–271; Raymond Tanter and Manus Midlarsky, "A Theory of Revolution," Journal of Conflict Resolution, 11 (September, 1967), 264–280; and Ted Robert Gurr, "A Causal Model of Civil Strife: Using New Indices," in Anger, Violence and Politics, ed. I. K. Feierabend et al. Much to his credit, of all the authors cited, Gurr, Ibid., p. 210, is the only one to acknowledge the level-of-analysis problem.

 ¹³McPhail, "Civil Disorder Participation," p. 1059.
 14John Dollard et al., Frustration and Aggression (New Haven: Yale University Press, 1939).

¹⁵This statement is based on the elaboration of Dollard's work by Neal E. Miller et al., "The Frustration-Aggression Hypothesis," Psychological Review, 48 (July, 1941), 337-342. See also: Leonard Berkowitz, Aggression: A Social Psychological Analysis (New York: McGraw Hill, 1962), in Advances in Experimental Social Psychology, Volume 2, ed. Leonard Berkowitz (New York: Academic Press, 1965).

¹⁶Gurr, Why Men Rebel (Princeton: Princeton University Press, paperback, 1970).

¹⁷Davies, "The J-curve," pp. 86-87.

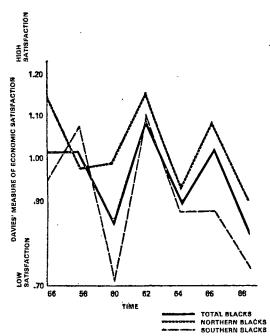


Figure 3.
Index of Black Economic Satisfaction from Individual Data

education than do whites. This is not as surprising a finding as it first appears (as we will demonstrate below), but is rather a penetrating commentary on the inappropriateness of the index as a measure of black frustration. Although income generally varies with education, it is those with lower education who receive a higher per annum income return for unit of education. Higher education yields a considerable amount more money over a working lifetime. On a per annum basis, however, it

takes many increments in years of schooling to produce modest increments in income. Black people tend to cluster at the bottom end of the education distribution where the ratio of income to education is highest. As a result, black people have higher income to education ratios. This is not to argue the absurd position that whites are frustrated relative to blacks. Rather, what is discerned from this finding is the futility of attempting to measure a perceptual concept such as frustration with aggregate, objective indicators. Moreover, it is solely because Davies's index is based on a ratio of two aggregates that the inappropriateness of the index is not readily discernible. The manner in which the aggregations conceal what is occurring can be readily observed in Table 3 below. In Table 3 the level of education is controlled by partition design, and the data indicate that it is the lower levels of education that yield the best ratio of income to years of schooling.

Having demonstrated the inappropriateness of Davies's index for testing the J-curve theory in the context of the urban riots, we attempted a refinement of the operationalization. Given relative deprivation theorists' reliance on objective data, we were interested in whether changing the unit of analysis, from aggregate to individual, and refining the measurement might yield something similar to a J-curve or other theoretically parsimonious configuration. Also, we were interested in observing if more sophisticated operationalizations of the objective but individual-level data would correspond to our perceptual measures (see below).

To accomplish this refinement in measurement, two objective measures based on the regression of income on education, after parti-

Table 2. Individual-Level Objective Data Measures of Davies's Operationalization of Black Economic Satisfaction^a

Year	Divi	Average of Each Head's Income Score Divided by Each Head's Years of Schooling Score ^b						Black Satisfaction Ratio Column 2 to Column 1		
	Total White	North White	South White	Total Black	North Black	South Black	Total Black	North Black	South Black	
1956	1.68	1.72	1.55	1.72	1.99	1.47	1.02	1.15	.95	
1958	1.62	1.62	1.61	1.66	1.60	1.74	1.02	.98	1.08	
1960	.55	.55	.51	.47	.54	.40	.85	.99	.71	
1962	1.84	1.91	1.70	2.01	2.20	1.88	1.09	1.15	1.10	
1964	.59	.59	.59	.53	.56	.52	.90	.93	.88	
1966	.54	.55	.52	.55	.59	.46	1.01	1.08	.88	
1968	.70	.70	.70	.58	.63	.52	.83	.90	.75	

^aScores are not translatable into dollar terms as they reflect the relationship between measurements on coding categories. Codes are not always comparable from year to year, but this is of no consequence since we are interested in the differences in any one year between whites and blacks and how these differences vary longitudinally. Column 3, the nonwhite satisfaction index, multiplied by 100 is similar to the nonwhite satisfaction index in Table 1.

bOnly heads of household were used.

Table 3. Education by Income Quotients with Level of Education, Race, and Region Controlled^a

	All Educational Groups ^b	Grade School	High School	College and Above	
Total white	1.84 ^c	3.10	1.53	0.86	
North white	1.91	3.13	1.57	0.85	
South white	1.70	3.00	1.43	0.88	
Total black	2.01	2.67	1.08	0.85	
North black	2.20	3.77	1.11	0.98	
South black	1.87	2.17	1.05	0.55	

^aData are from 1962 only since the issue is a cross-sectional one and not a longitudinal one. The year was chosen at random.

tioning race and region, were created. One of these is called the "differ variable," and it computes the difference between predicted income for blacks subtracted from predicted income for whites, when income is regressed against education. The second and more elaborate measure is called the "discrim variable," and it is constructed by subtracting a black individual's actual income from the income he would expect if he were white.²⁰ This

²⁰This latter operationalizations was suggested, in part, by Otis D. Duncan, "Discrimination Against

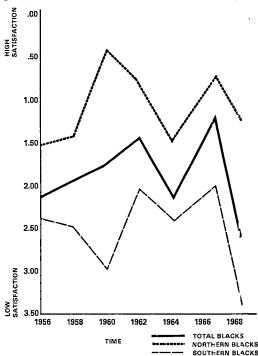


Figure 4.
Differ Operationalization of Black Economic Satisfaction

creates two objective individual-level measures of black relative deprivation. The results of these operationalizations are displayed below in Table 4 and Figures 4 and 5.

Regardless of operational differences, the trends for Figures 4 and 5 demonstrate the same time-series pattern. In neither case do we observe a J-curve. It is also important to note the divergences between the black population in each of the two regions. The national configuration is often an averaging of radically different trends between northern blacks and southern blacks. (This demonstrates the importance of looking at specific groups rather than treating the society as a monolith, or failing to make important distinctions within

Negroes," in Social Intelligence for America's Future, ed. Bertram M. Gross (Boston: Allyn and Bacon, 1969). Discrimination is measured by taking the slope and intercept of the regression equation for whites and plugging in the observed independent variable (education) scores for blacks. The resultant predicted dependent variable (income) scores for blacks are subtracted from the blacks' actual score on the dependent variable. The residual [Yp (if respondent were white) — Ya] is the measure of deprivation.

A wholly individual measure, based on differences between actual and predicted income, was desired for the "differ" operationalization. Such a measure would have been very similar to the one used for the "discrim" operationalization. In the calculation of measures of association, the type of measure is feasible. In calculating summary measures for the purpose of obtaining averages for blacks, this is not possible. In the case of any individual black, the "differ" is the residual between predicted income and actual income. The average of the residuals $\left| \sum (\hat{Y}i - \hat{Y}a)/N \right|$ however, always equals zero. The operationalization is meaningful for an individual black, but it aggregates to zero when cumulated for the group. To provide an analogous measure for the purpose of calculating an average, the average difference of predicted income for whites and blacks is calculated, given each groups' respective regression equation. Thus the average score is based on $\sum \hat{Y}i/N$ for blacks subtracted from $\sum \hat{Y}i/N$ for whites.

bHeads of household only.

^cScore is the average of each head's income score divided by each head's year of schooling score.

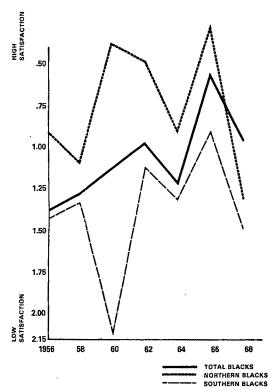


Figure 5.
Discrim Operationalization of
Black Economic Satisfaction

groups, a failure often necessitated by aggregate analysis.)

Being concerned with the urban riots, a northern phenomenon, we must focus specifically on the trends for northern blacks. Although the data (see Figures 4 and 5) do not follow the J-curve pattern, from 1958–1960 there is some indication of rising need fulfillment, and from 1960–1964 there is some indication of the loss of previous gains. But

from 1964 to 1966 there is another rise followed by a decline in 1966 through 1968. The J-curve interpretation is rejected because the rising need-fulfillment pattern is only of two year's duration, hardly commensurate with the long-term increases required by the theory; and the overall pattern, despite segments of rise and decline, is not commensurate with the J-curve.

What the data do reveal most dramatically is a series of extreme fluctuations and changes. There is neither a constant trend of improvement, deterioration, nor stability but a series of positive surges and negative declines. This pattern of instability may be causally related to the urban riots. Certainly, a wide variety of research21 has argued that political violence is a function of the ambiguity and dissonance associated with social change, even when the pattern of change is positive. Before pursuing in any detail these implications from the objective data, it is necessary to first determine if these patterns can be validated by perceptual data especially given the methodological and conceptual issues which were raised with regard to the use of even individual-level, objective data. It is to this issue that we now turn.

The Correspondence Between Objective and Perceptual Indicators of Relative Deprivation

The differ and discrim operationalizations are a marked improvement over the kinds of

²¹Murray Edelman, Politics as Symbolic Action: Mass Arousal and Quiescence (Chicago: Markham, 1971); Grofman and Muller, "The Strange Case of Relative Gratification and Potential for Political Violence"; Feierabend et al., "Social Change"; Seymour Martin Lipset, Political Man (Garden City: Anchor Books, 1963), esp., pp. 54–55; Michael Aiken et al.,

Table 4. Individual-Level, Regression-Based Objective Measures of Black Economic Satisfaction

	Diff	er Operationaliza	tion	Discrim Operationalization				
Year	Total Blacks	North Blacks	South Blacks	Total Blacks	North Blacks	South Blacks		
1956	2.12 ^a	1.50	2.37	1.35 ^b	0.87	1.40		
1958	1.95	1.40	2.47	1.26	1.07	1.31		
1960	1.77	0.43	2.95	1.10	0.36	2.12		
1962	1.53	0.81	2.03	0.96	0.53	1.10		
1964	2.15	1.46	2.40	1.21	0.91	1.31		
1966	1.25	0.72	1.99	0.55	0.29	0.91		
1968	2.50	1.35	3.40	1.31	0.94	1.47		

^aThe differ operationalization measures the difference of white predicted income minus black predicted income. The differ is computed by taking Σ Yi/N for whites minus Σ Yi/N for blacks. The operationalization is modified when based on noncumulated individual scores. See Footnote 20.

^bThe discrim operationalization is measured by taking the average of the residual between the predicted income for blacks (assuming they are white, by taking the predictions based on the intercept and slope of the white regression equation) minus black actual income.

information and measures generally available to relative deprivation researchers, since these are based on individual rather than aggregate measures. There still, however, remains the very important question of whether or not even individual-based objective measures can be substituted for perceptual measures.

This issue is far more complex than it initially appears and must be broken down into two component issues. First, there is the question of whether valid substitutions can be made on a data-point-by-data-point basis. Second, it is conceivable that the general pattern resulting from different types of data might be quite similar, although the points themselves might be unrelated for any given year. (One explanation for this phenomenon is the time gap between an event and its psycho-

Economic Failure Alienation and Extremism (Ann Arbor: The University of Michigan Press, 1968); Barrington Moore, Jr., "Revolution in America," New York Review of Books, 12 (January 30, 1969), 6-12. logical recognition.) In such instances the general theoretical interpretation provided by the two data sets might be the same, despite the specific temporal differences.

The preceptual indicators are based on three items, dealing with financial need satisfactions, from the 1956 through 1968 Michigan election studies. These items are: (1) whether the respondent (R) perceived his financial situation as being better or worse than the year before; (2) R's expectation of his future financial prospects; and (3) R's perception of his financial satisfaction. Each of these perceptual indicators was correlated with the three preceding individual-level, objective measures of relative deprivation: (1) the reoperationalized Davies index; (2) the differ operationalization and (3) the discrim operationalization. The correlation matrix for the six items - three perceptual and three objective - is displayed in Table 5 below.

Some of the data in the above matrix are presented solely for informational purposes, for we did not anticipate that all the interrelation-

Table 5. Intercorrelation Matrix of Objective and Perceptual Data

	R's Perception of the Trend of Finances		R's Perception of Future Financial Prospects			R's Perception of Financial Satisfaction			
Year	Total Black	North Black	South Black	Total Black	North Black	South Black	Total Black	North Black	South Black
				Davies's Ope	erationaliz	ation			
1968	053ª	098	024	109	086	161	.148 ^c	.219	.016 ^b
1966	.095	.021	.256	.070	223	.449			
1964	.112	.214	.051	.068	008	.106	.136	.235	.081
1962	.412	.384	.432	.025	284	.294			
1960	.374	.374	.282	.233	.204	.326	.401	.327	.479
1958	.091	.163	.018	.038	054	.176	.112	.254	093
1956	.023	132	019	140	318	012	.011	.021	.002
				Differ Oper	ationalizat	ion ^d			
1968	053	097	.012	123	094	143	.131	.216	008
1966	.149	.203	.101	.018	085	.096			
1964	.274	.359	.231	.116	076	.247	.195	.240	.179
1962	.492	.636	.355	.196	009	.383			
1960	.665	.743	.405	.259	.431	.231	.410	.414	.369
1958	.438	.623	.293	.007	275	.193	.276	-561	.052
1956	.281	.436	.195	.026	079	.053	.048	.098	.043
				Discrim Ope	rationaliza	rtion			
1968	073	131	051	154	144	200	.121	.198	.016
1966	.144	.174	.127	014	.085	.096			
1964	.232	.302	.180	.079	.058	.159	.205	.243	.189
1962	.492	.631	.378	.184	030	.353			
1960	.649	.735	.389	.264	.431	.254	.421	A17	.489
1958	.323	.496	.181	002	197	.126	.256	<i>A</i> 86	.031
1956	.201	.388	.022	004	080	013	.028	.063	.022

⁸Entries are Pearson's product moment correlations.

bLife satisfaction was used in 1968 due to change in protocol.

^cPerceptual variable unavailable for 1966 and 1962.

dSee Footnote 20 for a discussion of this operationalization.

ships would constitute an accurate test of the correspondence between objective and subjective indicators. The Davies's measure has been shown to have little value in assessing relative deprivation. Also future financial expectations are significantly different in conceptual meaning from current assessments of discrimination, and it was anticipated that future financial expectations would not correlate well with the objective measure. The main areas of interest within the matrix revolve around the correspondence between the differ and discrim operationalizations and R's perceived trend of finances and R's financial satisfaction.

In evaluating the matrix, the correlation coefficient of .25 was chosen as the minimum level for accepting a relationship between indicators.²² Even at this obviously low level, the objective operationalizations, on a point-bypoint comparison, failed to demonstrate a relationship with the perceptual indicators. In the case of the relationship between the differ and discrim operationalizations and financial satisfaction, the .25 hurdle is crossed only 40 per cent of the time for northern blacks and 20 per cent of the time for southern blacks. The relationship between the discrim variable and the perceived trend of one's finances shows little overall consistency for the total black population, despite good results generally for northern blacks. In this case the .25 level is exceeded 71 per cent of the time.

The relationships between the differ and discrim operationalizations on the one hand and the perceived trend of one's finances on the other are the best of the group, although still not very encouraging. These correlations exceed the .25 mark 71 per cent of the time for the northern black population but only 43 per cent (for differ) and 28 per cent (for discrim) of the time in the southern black population. Interestingly, the inappropriateness of the Davies's operationalization seems further underscored by its weak correspondence with the perceptual

²²Correlations of .25 in social science data are generally viewed as quite favorable. In this case, however, we have chosen to view .25 as a minimal level of acceptance because a criterion for the establishment of a valid substitute measure for different levels of analysis is being considered. Without engaging in a lengthy discourse on the philosophical issues involved in the level-of-analysis question, it is clear that such substitutions do involve an issue of validity, i.e., whether one measure is a valid substitute for another. Correlational requirements for measures of reliability and validity are far greater than for mere assertions of substantive importance. See for example, Fred N. Kerlinger, Foundations of Behavioral Research (New York: Holt, Rinehart and Winston, Inc., 1965), 444—462.

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ships would constitute an accurate test of the measures, being the most poorly related of the correspondence, between objective and subjective.

The strength of the correspondences between the differ and discrim operationalizations and the trend of finances variable is at times quite good. Note, for example, the relationship for northern blacks from 1958 through 1962. We are not sure why this correspondence exists and then radically tapers off, reaching a negative relationship in 1968. And we are reluctant to invoke any substantive explanations since the overall pattern of the data is not encouraging from the perspective of point-by-point substitutions. Interpreting the few cases that have correspondences might well tend to force systematic meaning where the overall picture seems to indicate that there is none.

Whether this lack of correspondence is also true of the trends of the subjective and objective data sets (as distinct from point-by-point comparisons) will be considered in the next section. We also wish to note that the analysis has not assessed the possibility that in more dramatic circumstances, such as full-scale revolutions, the convergence and salience of events might be so overwhelming as to be reflected in various kinds of assessments. Although our analysis does not preclude this possibility, at present, this is still an article of faith which has not been subjected to empirical verification.

A View of the Urban Riots from Perceptual Indicators

If Davies's argument is correct and if our objective data are too remote from the structure of the theory to test it adequately, our empirical observations of perceptual data should demonstrate the following results.

- (1) From 1956 to 1964 the proportion of northern blacks perceiving positive changes in the trend of their finances should increase steadily.
- (2) After 1964 the proportion of northern blacks perceiving positive changes in the trend of their finances should begin to decline continually and substantially.

The above two points express succinctly and in empirically relevant terms the essence of the J-curve as applied to the requisite conditions of the urban riots. In addition to this depiction of the rise and decline scenario, there is another equally important consideration. This relates to the untested assumption of relative deprivation theory generally and the J-curve specifically that one's current trend of need fulfillment is positively and strongly related to future financial expectations. In the context of the J-curve

this of course occurs during the period of rising gratifications. Its occurrence, however, is one of the most vital aspects of the theory. For if there is no correspondence between current levels of need fulfillment and future expectations of need fulfillment, the theory is wholly untenable. Without such correspondences an intolerable gap can not occur (see Figure 1). These observations lead to two additional considerations:

(1a) From 1956 to 1964 the trend of finances for northern blacks should be positively and strongly related to future financial expectations.

(2a) After 1964, the pre-1964 relationship between the trend of finances for northern blacks and their future financial expectations should reverse itself.

The rejection of any one of the above four statements would result in a rejection of the J-curve theory as an explanation of the black urban riots. Nonetheless, we will review the results as applied to all four statements because we are not only interested in testing the theory, but we are also concerned with the presence or absence of empirical justification for the theory's basic assumptions. Moreover, special concern surrounds statements 1a and 2a because they are also fundamental to the assumptions of relative deprivation theory generally.

Before examining the data, there is an item concerning the demarcation of time frames for rising and declining need fulfillment that merits attention and commentary. This concerns the appropriateness of looking at the post-1964 period as the likely area of decline and hinges on the interpretation of the Watts riot which occurred in August of 1965. If this event is perceived as having initiated the riot phase, then the period before 1964, it has been suggested, is the more appropriate place to look for financial reversals. Watts would then be considered as the point of the "intolerable gap," where the disparity between continually rising expectations and falling actual need fulfillment culminates in civil disorder.

Such issues might have been readily resolved if the J-curve theory had been empirically more rigorous in specifying the operationalization of the "intolerable gap." One of the difficulties with all forms of post hoc theory is that they manifest an empirical prowess greater in appearance than in fact. The requirements of a theory tested through prediction would of necessity mandate an operationalization of critical concepts. Where theory is constructed after instead of before prediction, such empirical precision is too easily overlooked. This lack of specification further complicates analy-

sis because all forms of civil disorder, even coup d'etats, are often a series of events rather than a single event. These events can occur in close temporal proximity, but, sometimes, as in the case of the riots, there is substantial temporal space between them. As to which single event among the set of events coincides with the crucial intolerable gap is an issue that is generally ignored.

Further specification by the original theory could have provided some assistance in properly assessing the role of Watts in this scenario. However, even if Watts is considered the initiator of this phase of riot activity, the two-year gap between Watts and the preponderance of riot activity strongly suggests that the downward cycle of perceived need fulfillment began about 1964, was in evidence by August, 1965, but it did not approach the intolerable gap phase until 1967.

An interpretation of Watts as coming at the bottom of the need fulfillment spiral would have to focus on the sharp decline from 1960 to 1962 in Table 6 and Figure 6. (Interpretation is restricted to the perceptual data because they constitute the best indicators.) But this analysis would be undermined by the sharp rise in need fulfillment occurring from 1962 to 1964. It would also be forced to ignore the important cycle of fluctuation and change throughout the data set, not to mention the two-year gap between Watts and the other riots. For these reasons, we have chosen to maintain 1964 as the point of demarcation between rising and declining need fulfillment in the hypotheses above.

Turning now to the data and consideration of increased need fulfillment, Table 6 and Figure 6 below demonstrate that blacks did not experience increased need fulfillment from 1956 to 1964. Instead of a trend of continual improvement, there exists a series of sharp fluctuations, not at all unlike the results obtained with the objective data. If the northern blacks are compared to northern whites, the contrast in this regard is rather marked. Across time relatively stable percentages of northern whites perceive their finances as improving, while percentages of northern blacks experiencing the same perception fluctuate dramatically.

Except for the tremendous upsurge in 1964, characteristic of all groups, northern whites show a pattern of marked stability. Excluding 1964, the range of responses varies from a low of 34 per cent to a high of 39 per cent. Given considerations of sampling error, it is not unreasonable to assume that a straight line would, with the exclusion of 1964, accurately characterize these data. Southern whites also

show a fairly stable pattern, but the fluctuation that does exist ensues from the close temporal proximity between the highest and lowest points in the longitudinal distribution. Financial optimism triumphed in 1964 but crashed disastrously by 1966. Outside of that sharp downward spiral the pattern is seemingly constant. Even with due consideration of the sharp drop from 1964 to 1966, the range for southern whites runs from a low of 29 per cent to a high of 41 per cent, with a difference of only 12 per cent.

The argument for the stability of white perceptions is best appreciated by looking at the corresponding data for the blacks. Northern blacks run a gamut of alternate high to low and low to high fluctuations, with a range from 21 per cent to 46 per cent, or five times that of northern whites. For southern blacks the pattern is similar except there is somewhat more stability, each low ebb is not necessarily followed by another high point, and the period from 1964 on is virtually constant. The range is 23 per cent, or nearly twice that of southern whites.

Interestingly, if the same longitudinal relationships are examined from the perspective of the respondents' conception of their financial satisfaction or their expectation of their future

financial satisfaction, the same phenomenon of white stability and black instability is repeated (see Tables 7 and 8 below).

The data clearly indicate that it is the northern black population that experiences the greatest instability. Even incorporating the impact of 1964 — a year which appears to have produced sharp financial optimism among all groups — the variation among whites in both the North and South is nowhere nearly as sharp as it is for blacks; and it is again the northern blacks, not the southern blacks, who illustrate the greatest instability.

Is it likely that this series of fluctuations has emerged as a consequence of systematic differences in sampling design between off-year and presidential election year studies? Or is the instability, perhaps, due to the differences in sample size between blacks and whites, with blacks demonstrating more instability simply because of smaller Ns? The pattern of fluctuation and change observed in Figure 6 and Table 6 can not be dismissed as mere aberrations in the data resulting from systematic changes in the sampling frame between presidential and nonpresidential election studies. The Survey Research Center did not use different sampling frames in the off-year and presidential year studies. From 1956 through 1960, the identical

Table 6. R's Perception of the Trend of his Financial Situation by Race and Region

	a 1956	1958	1960	1962	1964	1966	1968
Northern white							
Better	39%	34%	36%	35%	48%	37%	34%
Same	43	45	47	46	38	. 37	45
Worse	19	22	18	20	14	26	21
(Total)	101% ^a	101% ^a	101% ^a	101% ^a	100%	100%	100%
(N)	(1168)	(1165)	(1179)	(777)	(3017)	(797)	(1644)
Northern black							
Better	33%	21%	46%	21%	44%	33%	32%
Same	43	49	30	52	40	41	44
Worse	24	30	25	27	16	27	25
(Total)	100%	100%	101% ^a	100%	100%	101% ^a	101%a
(N)	(63)	(84)	(77)	(52)	(184)	(78)	(110)
Southern white							•
Better	40%	32%	35%	38%	41%	29%	33%
Same	44	46	49	47	44	45	50
Worse	17	22	17	15	16	26	17
(Total)	101% ^a	100%	101% ^a	100%	101% ^a	100%	100%
(N)	(425)	(464)	(544)	(382)	(1146)	(322)	(690)
Southern black							
Better	28%	32%	19%	17%	40%	40%	38%
Same	51	32	42	50	43	35	48
Worse	22	37	40	33	17	25	14
(Total)	101% ^a	101% ^a	101% ^a	100%	100%	100%	100%
(N)	. (83)	(76)	(91)	(58)	(234)	(57)	(132)

^aSums to more than 100 per cent because of rounding.

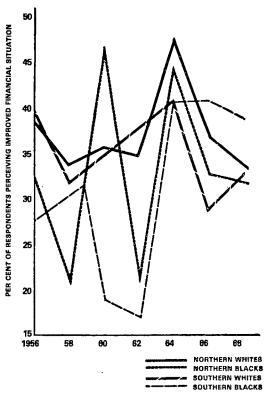


Figure 6.

Percentage of Respondents Perceiving the Trend of their Financial Situation as Improving

sampling frame was used. The stability of this design was reinforced by the presence of a large panel study for this time period. Consequently, the sharp fluctuations and changes observed in Figure 6, for the 1956 through 1960 period, can hardly be attributed to changes in the sampling frame. In 1962 the sampling design was updated and the resulting frame was used through and including 1968, making it impossible to attribute changes observed in this period to changes in the sampling design. Even the changes between 1960 and 1962 can not be attributed to the change in sampling frames, for the large metropolitan self-representing PSUs, where some two-thirds of the northern blacks reside, remained as self-representing units.23

How about the question of sample size? Can the changes be attributed to differences in sample size for blacks and whites? Do the blacks display more volatile patterns simply because their sample size is smaller? This argument is untenable because it overlooks the crucial implications of the Law of Large Numbers (Bernoulli's theorem).²⁴ The critical question is not whether the black sample is smaller or larger than the white sample. The critical question is whether the black sample, both in the North and South, is sufficiently large to meet the requirements of Bernoulli's Theorem. As Table 6 demonstrates, these well-known criteria for statistical inference are more than amply met by the sample Ns.

Although the above commentary would seem to lay this issue to rest, we decided to pursue this matter one step further. A ten per cent sample of white respondents was selected by means of a random number generator applied to each discrete data point, with region controlled. If the volatility for blacks relative to whites is purely a function of differences in sample size, then a sample of whites severely reduced in size should demonstrate the same volatile pattern as that observed for the blacks. Yet, the ten per cent sample was virtually indistinguishable from the larger sample in both the case of northern and southern whites. This finding plus the information on the sampling frames would appear to eliminate the assumption that the results could be attributed in any meaningful way to the sampling design or the differences in sample size.

Now that we have demonstrated that the observed results could not be meaningfully attributed to sample size or design, and that rising expectations did not characterize the trend of need fulfillment for northern blacks prior to 1964, is there any indication the need fulfillment declined sharply after 1964? Again, if we look at Table 6 and Fiture 6, we note a drop from 44 per cent in 1964 who perceived the trend of their finances as improving, to 33 per cent in 1966. This is, however, a smaller drop than took place in the two previous periods of decline, and the relationship stabilized in 1968. Consequently, there is no confirmation of a continual decline after 1964, as the theory would predict.

Turning now to the all-important assumption of relative deprivation theory generally that current need fulfillment is related to expectations of future need fulfillment, this is shown to be generally unsubstantiated (see Table 9). The assumption could be demon-

 2^4 The theorem is represented as follows: probability ($|f/n-p| \ge \epsilon$) $\to 0$ as $N \to \infty$; where f is the frequency of occurrence of event X among N trials, p is the probability of X, ϵ is some arbitrarily small positive number. It is generally assumed that good results are produced with N greater than 30. It also is at this point that the normality assumption of the Central Limit Theorem can be relaxed.

²³We wish to thank Leslie Kish and Irene Hess of the sampling section of the University of Michigan's Institute for Social Research for explaining the sampling design to us and clarifying this issue.

strated as warranted for northern whites, and to a far lesser extent for southern whites; however, in the case of northern blacks it is most untenable. (This result provides another indication of the importance of performing the analysis within specific subgroups as opposed to treating the society or the group as a monolith, a problem inherent in aggregate data analysis.) Consequently, this assumption which is virtually axiomatic to all forms of relative deprivation theory is not as universal as its strategic role demands. This relationship obviously varies from group to group and from circumstance to circumstance. All we can say is that sometimes, and for some groups, future expectations are a function of current need fulfillment, and at other times, and for other groups, this relationship does not hold.²⁵ Indeed, this is hardly the kind of axiomatic statement on which a science of revolution is to be built.

25Ted Robert Gurr informs us (Correspondence 12/27/74) that although he agrees that the linkage between future expectations and current need fulfillment is a fundamental assumption of relative deprivation theory, the relationship may vary within the social structure. In the course of yet uncompleted research (with Raymond Duvall) Gurr has theorized

An important implication of these results is that for blacks, as opposed to whites, perceptions of past experience are not instrumental in shaping perceptions of the future. This would suggest that although whites can rely, to some degree, on past experience as a means of interpreting the future, blacks cannot. It is probably not unwarranted to infer that this is an additional indication of the ambiguous and unstable environment which blacks inhabit.

The data further indicate that instead of the increasingly strong relationship between trend

that individuals in the middle range of attained values (need fulfillment) appear to expect more gains in the future than those at the extremes of the distributions. If Gurr is correct, this would require some critical restatement of the underlying assumptions of relative deprivation theory. Some preliminary analyses that we have undertaken demonstrate that for blacks, those at the lowest end of the continuum have the highest expectations. For example, preliminary runs from SRC's Campbell-Schuman data set indicate that young (18-35) northern, black males are most dissatisfied with their current situation and most optimistic about the future. Needless to say, the issue Professor Gurr raised is an important one that could conceivably have a major impact on relative deprivation theory.

Table 7. Respondent's Perceived Financial Satisfaction by Race and Region

	1956	1958	1960	1962	1964	1966	1968b
North (white)							
(Pretty Well) Satisfied	41%	38%	36%	а	46%	a	26%
(More-or-Less) Satisfied	42	42	42		40		65
Not Satisfied (At All)	17	20	22		14		9
• • • • • • • • • • • • • • • • • • • •	100%	100%	100%		100%		100%
N =	(1168)	(1163)	(1179)		(3012)		(1684)
North (black)							
(Pretty Well) Satisfied	29% ^b	16%	22%		31%		15%
(More-or-Less) Satisfied	32	26	26		39		65
Not Satisfied (At All)	40	58	52		30		20
	101% ^c	100%	100%		100%		100%
N =	(63)	(86)	(76)		(185)		(109)
South (white)							
(Pretty Well) Satisfied	49%	50%	45%		46%		24%
(More-or-Less) Satisfied	34	34	35		42		66
Not Satisfied (At All)	17	16	20		13		10
	100%	100%	100%		101% ^a		100%
N =	(429)	(465)	(553)		(1146)		(696)
South (black)							
(Pretty Well) Satisfied	24%	30%	17%		32%		12%
(More-or-Less) Satisfied	37	37	42		34		74
Not Satisfied (At All)	39	33	42		35		14
	100%	100%	101% ^c		101% ^c		100%
N =	(83)	(76)	(91)		(235)		(131)

^aData unavailable for 1962 and 1966.

^bIn 1968, for "Life Satisfaction" the categories are: Completely Satisfying, Pretty Satisfying, Not Very Satisfying.

^cColumn adds to more than 100 per cent because of rounding.

Table 8. R's Perception of Future Financial Prospects by Race and Region (1956-1968)

	1956	1958	1960	1962	1964	1966	1968
North (white)							
Get better	43%	41%	39%	39%	45%	37%	34%
Stay the same	47	46	50	55	45	51	55
Get worse	10	13	11	6	10	12	10
	100%	100%	100%	100%	100%	100%	99%a
N =	(1094)	(1130)	(1107)	(686)	(2884)	(700)	(1644)
North (black)							
Get better	60%	68%	54%	36%	72%	37%	27%
Stay the same	29	26	39	55	24	57	58
Get worse	10	6	8	10	4	6	15
	99% a	100%	101% ^a	101% ^a	100%	100%	100%
N =	(58)	(84)	(67)	(42)	(173)	(63)	(110)
South (white)							
Get better	44%	41%	38%	38%	40%	28%	35%
Stay the same	48	46	54	57	51	59	55
Get worse	8	13	8	5	9	13	10
	100%	100%	100%	100%	100%	100%	100%
N =	(396)	(450)	(499)	(358)	(1098)	(287)	(690)
South (black)							
Get better	49%	53%	61%	21%	60%	51%	31%
Stay the same	40	33	31	71	35	36	59
Get worse	10	13	9	8	5	13	11
	99%ª	99%ª	101% ^a	100%	100%	100%	101% ^a
N =	(77)	(75)	(81)	(48)	(224)	(45)	(132)

^aDoes not sum to 100% because of rounding error.

Table 9. Correlation Between R's Perceived Trend of Financial Fulfillment and R's Future Financial Expectations by Race and Region

_	1956	1958	1960	1962	1964	1966	1968
Northern whites	.39	.39	.39	.41	.50	.44	.44
Southern whites	.22	.45	.29	.37	.28	.54	.42
Northern blacks	.05	24	.71	.02	.34	.44	.51
Southern blacks	01	.24	.44	.33	.42	.24	.25

of finances and financial expectations, as was predicted by the J-curve theory for the period between 1956 and 1964, northern blacks exhibit an erratic pattern, with correlation coefficients ranging from a low of -.24 in 1958 to a high of +.71 in 1960. Moreover, instead of the coefficients declining after 1964, they actually increase (see Table 9).

The perceptual data indicate that in the case of each of the four hypotheses relating the J-curve to the black urban riots of the 1960s, the theory was rejected. Equally important is the observation that one of the basic axiomatic components of the J-curve theory and relative deprivation theory generally was found to be unconfirmed — that future expectations of need fulfillment are a function of current perceptions of need fulfillment.

It has been suggested that although the J-curve theory as disconfirmed by the data as an explanation of the urban riots, this is not sufficient reason for implying a general lack of confidence in the theory. This argument is based on the consideration that Davies may have erred in attempting to extend his theory beyond the parameters for which it was designed. Davies's theory, it has sometimes been asserted, was directed at revolutionary events, and the urban riots, whatever else they may be called, were not revolutionary events. The lack of confirmation in the instance of the urban riots, it has been sometimes further suggested, is simply the result of attempting to explain diverse phenomena with a single dynamic. This argument is worthy of attention for two reasons: (1) It is consonant with arguments offered in behalf of middle-range theories.²⁶ (2) In a recent communications exchange between Davies and David Snyder writing with Charles Tilly, Davies appears to have made a similar point.²⁷

Snyder and Tilly²⁸ assert that a longitudinal evaluation of political violence in France from 1830 to 1960 does not fit the J-curve. In response, Davies²⁹ argues that a long, wholecountry series analyzing various events of civil disorder is not an adequate test of his theory. Davies implies that his theory need not be expected to account for nonrevolutionary events. In light of his own work, this is a rather astounding argument. Even if one were to view Davies's application of the J-curve to the urban riots as an over-extension of the theory, such a position would hardly be commensurate with Davies's original formulation of the theory or his own previous work. As Snyder and Tilly note, "... Davies himself had applied his scheme to student protests, draft riots, the protests of American blacks in the 1960s not to mention the Pullman strike, the Dorr Rebellion, plus many other conflicts of all shapes and sizes."30

If Davies is to be consistent in his contention that Snyder and Tilly's work is irrelevant to his arguments, then it is incumbent upon him to propose a reformulation of the parameters of his own theory. Certainly, the case can be made in terms of middle-range theory that a variety of historical experiences cannot, at this stage of social science, be explained in the context of a single dynamic. In Davies's case, this reconstruction is at best difficult, for the development of the theory itself is based on a diversity of types of civil disorder. Davies's development and previous applications of the theory indicate that the attempt to explain the urban riots is wholly in accord with the theory's general formulation and application. It could not be construed as an attempt to apply the theory outside the parameters for which it was intended.

The Black Urban Riots: Some Suggestive Theoretical Issues

In the period preceding and during the riots, the black community did not experience a sustained improvement in its economic situation followed by a sharp reversal. Instead of this J-curve scenario, the black community generally and the northern black community specifically displayed dramatic ambiguity and instability in describing the trend of their finances, their financial satisfaction, and their expectations of future financial improvement. These observations were borne out by both the objective and subjective data. The ebbs and flows of the points in any one year may vary from objective data to subjective data. Nonetheless the correspondence in the overall trend remains.

Were these perceptions of instability more marked in the 1960s than in any other previous contemporary period of black history? This we do not know. Even so, the very pattern of the data suggests an alternate explanation of the riots. Here, however, we must tread with caution. Relative deprivation studies typically observe a pattern of data, often aggregate data, together with a concomitant episode of collective or political violence. The two are then interpreted as being related, with some form of relative deprivation, represented by the data configuration, being assumed as the cause of the ensuing violence. In terms of classical considerations of causality and experimental design, this type of deductive reasoning leaves much to be desired.³¹ It is, however, the basis for many of the theoretical interpretations of revolution and the underlying causal mechanism of virtually all relative deprivation studies. Sears and McConahay³² note this type of logic is also the basis for the relative deprivation approach to the study of the black urban riots.

While acknowledging the limitations of such procedures, we must also note the difficulty in obtaining the type of data that would directly link the assumed preconditions of violence with the acts of violence. This is especially true of historical studies of violence, but this problem is also prevalent in studies of contemporary episodes of violence. The research of Gurr, ³³

²⁶For a discussion of this position see Robert K. Merton, Social Theory and Social Structure (Glencoe, Ill.: The Free Press, 1957), pp. 5-11.

²⁷Davies, "Comments [on Snyder and Tilly]," *American Sociological Review*, 39 (August, 1974), 607-612.

²⁸David Snyder and Charles Tilly, "Hardship and Collective Violence in France, 1830–1960," *American Sociological Review*, 37 (October, 1972), 520–532.

²⁹Davies, "Comments," pp. 607-610.

³⁰ Snyder and Tilly, "Comments [On Davies],"

American Sociological Review, 39 (August, 1974),

³¹ For a discussion of causal inference in classical experimental design, see Donald T. Campbell and Julian C. Stanley, Experimental and Quasi-Experimental Design for Research (Chicago: Rand McNally, 1963).

³²David O. Sears and John B. McConahay, *The Politics of Violence* (Boston: Houghton Mifflin, 1973), pp. 90-91, footnote 1.

³³Gurr, Why Men Rebel.

Davies,³⁴ and the Feierabends,³⁵ to name but famous study of suicide, Emile Durkheim⁴⁰ a few of the most regarded scholars working in this area, have manifested these inherent and often unavoidable difficulties.

These limitations should not preclude incisions into the problems of collective and political violence. They must, however, caution us about the limitations of our inferences, and make us aware that we are perhaps more engaged in an exploration enterprise than in "confirming" hypotheses.

With such caveats in mind, we are struck by the suggestiveness of our own data. The data demonstrate that the black community generally, and the northern black community particularly, experienced extreme fluctuation and ambiguity in its perceptions of the trend of its finances, its financial satisfaction, and its expectations of financial improvement. These extreme fluctuations and ambiguities of black perceptions might have led to the urban riots of the 1960s.

The pattern of data fits both the theoretical writing by Feierabend et al.³⁶ based on observations from aggregate data, and the work by Grofman and Muller³⁷ based on individual data. Feierabend et al. see constant flux and its attendant uncertainties as producing frustration. As they note, "uncertainty is a special quality of expectations. Ambiguity as to whether the future will bring disaster or salvation should be considered a distressful experience, adding to the present sense of frustration. Only in the case of disaster is certainty more likely to be judged as more frustrating than uncertainty."38 Frustration, of course, leads to aggression; and aggression, under the proper circumstances, will lead to political violence.

Similar observations have been made by Grofman and Muller. They found that "the greatest potential for political violence is manifested both by individuals who perceive negative change ... and by individuals who perceive positive change..., while those who perceive no change manifest the least potential for political violence."39

The theoretical and empirical foundations for these observations are anchored in a long tradition of social science research. In his noted that anomie could be produced by social disruption irrespective of whether it resulted from economic disaster or an abrupt transformation leading to power and wealth.

Ambiguity, fluctuation, and uncertainty as conditions which contribute to one's susceptibility to be mobilized for revolt have been addressed in the works of Barrington Moore, Jr., 41 Michael Aiken, 42 Seymour Martin Lipset, 43 Norman Cohn, 44 and Murray Edelman.45 In a speculative essay, Edelman has applied this interpretation directly to the black urban riots. He argues that man's expectations of the distant future influence the cognitions that explain his current situation. The important expectations of status and security come from governmental acts. The inconsistent fluctuation between responsiveness and nonresponsiveness to black needs by the government produced alienation, fear, and anger. In this climate, Edelman notes, a precipitating incident can easily touch off violence. Moreover, he goes on to argue, the greatest ambiguity existed for northern blacks.⁴⁶

The interpretation suggested by our perceptual data is highly congruent with these theoretical statements that fluctuation, change, and ambiguity create situations highly conducive to violence. Edelman's suggestion that ambiguity is greatest among the northern blacks is also confirmed by our perceptual data.

What can be said with some confidence is that the black community generally and the northern black community specifically experienced a great deal of perceptual uncertainty with regard to their financial situation and their expectations of their future financial circumstances. We can also argue that there is at least a substantial set of theoretical and empirical work to indicate that such conditions have a high probability of precipitating political violence. It is at least plausible to note that the pattern of fluctuation and change and the attendant perceptual ambiguities may be components of the causal nexus that culminated in

³⁴Davies, "The J-curve."

³⁵Feierabend et al., "Social Change."

³⁶ Ibid.

³⁷Grofman and Muller, "The Strange Case of Relative Gratification.'

³⁸Feierabend et al., "Social Change," p. 637.

³⁹Grofman and Muller, p. 514.

⁴⁰Emile Durkheim, "Anomie and Suicide," in Sociological Theory, ed. Lewis A. Coser and Bernard Rosenberg (New York: MacMillian, 1970), p. 529.

⁴¹Moore, "Revolution in America."

⁴²Aiken et al., Economic Failure, Alienation and Extremism.

⁴³Lipset, *Political Man*.

⁴⁴ Norman Cohn, The Pursuit of the Millennium (New York: Harper Torchbooks, paperback, 1961), p.

⁴⁵Edelman, Politics as Symbolic Action.

⁴⁶*Ibid.*, pp. 20-21.

is no less plausible nor less buttressed by empirical evidence than most of the current explanations of the riots. The difficulty resides in the linkage between the pattern of the data on the one hand and the violence on the other. Until it is possible to bridge this chasm in a causally meaningful fashion, our results must remain suggestive avenues for further research.

Summary

The purpose of this research was to test the J-curve theory in the context of the black urban riots by using both perceptual and objective indicators. The results demonstrate that the J-curve theory is not a valid explanation of the riots. Several empirical, methodological, and conceptual problems were also noted. These basically revolved about the regrettable tendency of researchers to test a theory based on individual perception with aggregate and objective data.

The research demonstrated that: (1) Aggregate indicators can be highly misleading substitutes for individual indicators. The J-curve's use of an aggregate index to measure black frustration was found, upon examination of the same measure based on individual data, to be highly misleading. Moreover, the inappropriateness of the operationalization was concealed by the process of aggregation. (2) Objective indicators cannot be used as direct substitutes for perceptual indicators, even when both are based on individual data. There is, however, some indication from our analysis that while substitutions are unwarranted on a point-by-point basis, substitutions may be justified when based on the overall trend of the data. The generality of this conclusion, however, awaits further confirmation. (3) Regardless of the nature of the data configuration, societies cannot be viewed as monoliths. It is imperative that specifications and distinctions be made for various groups. As our data show, trends for the total black population often obscured crucial divergencies between northern and southern blacks. Relative deprivation-based theories that treat societies as monoliths may well conceal as much as if not more than they reveal. If statements such as "revolutions are most likely to occur when things get better" are to have any theoretical meaning, then it must be demonstrated for whom things have gotten better. Is it indeed the revolutionary class? When unspecified data demonstrate that things improved for the society as a whole, can we be sure that things did not simultaneously get worse for the revolutionary class? Is it the rising

the black urban riots. Certainly, this argument expectations resulting from overall improvement that caused the revolution? Or is it an increasing gap between the revolutionary class which experiences no improvement, and the rest of the society that experiences increased need satisfaction? The monolithic approach cannot assist the student of revolution in resolving such questions and may well lead to deceptively simple answers. (4) The critical assumption of the J-curve theory and relative deprivation theory generally that future expectations of need fulfillment are a function of current need fulfillment trends was shown to be largely unsubstantiated. The data did indicate modest correlations for these two variables for northern whites, and to a lesser extent for southern whites and southern blacks. There was no basis for this assumption when applied to the crucial northern black community. Since this assumption is one of the axioms of the J-curve, its consequent lack of verification should at least raise the question of how the axioms of the J-curve might be restructured to accommodate this information.

To students of revolution who have used relative deprivation type theories, this research advises caution. Such caution ensues from a firmer recognition of the problems of analysis of historic revolutionary events that derive from data which are of necessity based on aggregate, objective indicators. Such analysis might be thoroughly misleading, as this research has demonstrated. This factor has even caused some researchers to forsake relative deprivation theory as an explanatory device. Such rejection is not based on theoretical limitations but on the inability to fit relative deprivation theory to appropriate data. At the same time, if our observation is correct that even in the absence of point-by-point correlations between objective and subjective indicators, there is consistency between the overall trends, relative deprivation theory might still have great promise. It remains for research to be directed at this problem through simultaneous analysis at both the individual and aggregate levels.

Finally, we propose some avenues for further research on the substantive issue of the etiology of the black urban riots. The cycles of fluctuation and change that emerged from our analysis of perceptual data fit an extensive theoretical literature on the causes of civil disorder and revolution. Although we are reluctant to follow many of our colleagues across the inferential chasm that depicts a causal relationship when a diagrammatic configuration of relative deprivation exists in the same temporal space as an act of violence, the congruence of our findings with other theoretical

Equality and Human Need

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Equality and Recognition

The good or the valuable is revealed to man by God; or, it arises out of man's rational awareness of the requirements of his nature and of the place of that nature within the cosmos; or, it is a reflection in man's consciousness of the unfolding dialectic of history; or, it is a human response to environmental and human contingencies of reinforcement; or, it originates simply in man's fiat or will. Whether from God, nature, reason, history, necessity, environment, culture, or will; whether destiny, discovery, or invention, there is no understanding of value without recourse to human nature. From Aristotle to scientific social scientist there is on this point no dispute. Where Aristotle notes that "it is the peculiarity of man, in comparison with the rest of the animal world, that he alone possesses a perception of good and evil, of the just and the unjust, and of other similar qualities...," Milton Rokeach confirms, "man is the only animal that can be meaningfully described as having values."2

This paper is in part an exploration of how and whether it may be said that value arises from human need, how the concept of human need itself can be understood, and, specifically, how and whether the value of equality may be said to arise from some particular human need. If, in commencing this inquiry we begin with Thomas Hobbes, it is not because Hobbes is widely celebrated for his egalitarianism. If we turn to Leviathan for a defense of equality, it is not because we anticipate a treasure-trove of hitherto untapped and even unsuspected argu-

ments in its behalf. Instead, it is because we find in Hobbes a way of thinking about equality and human need which with various turns and emphases continues to the present. That way of thinking is also the subject of this inquiry.

The argument for the natural equality of mankind found in the thirteenth chapter of Leviathan can scarcely be condensed; it occupies barely two paragraphs. It is so characteristically Hobbesian that its basic components struggle and vanity - are as well the underpinnings of the entire structure of the Leviathan. Of course we are not enjoined to display toward our fellows that equal regard which follows from our being all children of the same God, created in His image. Nor in any precise sense do we find a variant of the argument Locke will use later as his justification of natural equality: that we are all members of the same species, born to the same rank and possessed of the same faculties, that, in brief, all men are men as all cats are cats and dogs, dogs. And we certainly do not find any claim about the sameness or identity of men; Hobbes is not so foolish as to deny the evident inequality of men in regard to their natural endowments, chiefly of mind and body. What we do find is the claim that, taken all in all, these natural differences do not count for very much. As Hobbes declares, "... when all is reckoned together, the difference between man and man, is not so considerable, as that one man can thereupon claim to himself any benefit, to which another may not pretend, as well as he."3 Lest anyone mistake his meaning, he immediately remarks that natural strength is no bar to another's undertaking a hostile action, for the weaker can easily overpower the strong-A

³Thomas Hobbes, *Leviathan*, ed. Michael Oakeshott (New York: Collier, 1962), p. 98.

¹ Aristotle, Politics, 1253a15.

²Milton Rokeach, *The Nature of Human Values* (New York: Free Press, 1973), p. 20. On the problem of the synonomous use of the terms "good" and "value," see Martin Diamond, "The Dependence of Fact Upon 'Value," *Interpretation*, Volume 2/3 (Spring, 1972), 229–231.

er either through guile or, what amounts to the same thing, by combining with others. The argument for the natural equality of mind is a bit more subtle: Hobbes recognizes that most men are vain enough to think themselves wiser than their fellows and concludes from their evident satisfaction with this state of affairs that each is content with his share and therefore the equal of every other.

Whatever may be said about Hobbes's argument, he clearly does not look upon the equality of men as a brute fact, as a simple description of their natural condition in the same way that we may say men are in nature along with streams, plants, other animals. To appreciate this is to understand the curious and otherwise inexplicable hesitancy Hobbes shows when he comes to set forth his ninth law of nature, the one against pride: that every man acknowledge every other as his equal by nature. Our needing a law of nature to affirm an already existing natural condition (no stone needs to be told to fall at the rate described by the law of gravitation) points to a certain ambiguity about the status of equality, one that Hobbes readily acknowledges. He tells us: "If nature therefore has made men equal, that equality is to be acknowledged: or if nature has made men unequal; yet because men that think themselves equal, will not enter into conditions of peace, but upon equal terms, such equality must be admitted."4 The point is not that men are free to break laws of nature as stones are not, for if that were all that was at issue there would be no need to intrude the surprising admission that perhaps, after all, nature has not made men equal. What is decisive, then, is that, whatever the truth about natural equality, it is men's opinions about themselves and of each other that are politically determinative. Of course, men are so constituted by nature that they weigh, balance, and compare themselves with every other, but it is no conclusion of nature that the scales are in equilibrium. So far from fact is this the case that a convention backed by the awesome power of Leviathan must be invoked to assure the conditions of civil peace; the law of nature is contrary to our natural passion propelling us toward selfpreference and pride.⁵ Egalitarianism has little support in nature, but it is the necessary precondition for that political life which is to remedy the defects of nature. The "men that think themselves equal" are being induced by Hobbes to so think by an argument allegedly founding equality in nature.

Whatever, then, the truth about the equality of men by nature (and that truth is ultimately ambiguous), the origin of the requirement of political (i.e., conventional) equality lies in the fact that each man is constantly scrutinizing the gaze of the other to determine whether that other values him as he values himself. If not, he is driven to every extremity to extract from that other the proper sign of recognition, "for every man looketh that his companion should value him at the same rate he sets upon himself,"6 In fact, the rate a man sets upon himself is most often greater than an objective appraisal would warrant. In competing in this way for honor and dignity, it is inevitable that that competition degenerate into a war of every man against every other.

Still, if Hobbes's assertion that the natural condition of war is an inference from the passions is valid, an individual's overevaluation of himself cannot be the result of mere caprice. Rather, it must arise from the necessities of his nature. We need only remind ourselves that men are so constituted that they must toil ceaselessly in fulfillment of their desire; the means to accomplish this is what Hobbes defines as their power. In the final analysis. power is essentially social, determined far less (if at all) upon an objective appraisal of a man's natural abilities than upon an estimation of these by others. If men compete for honor and dignity, they compete for public or private signs that others assign them so much value or worth. For the value of a person is nothing other than what some other would be willing to pay for that person's "power." The value or worth of a person is therefore not absolute. "but a thing dependent on the need and judgment of another.... And as in other things, so in men, not the seller but the buyer determines the price. For let a man, as most men do, rate themselves at the highest value they can; yet their true value is no more than it is esteemed by others."8 Men must set a high value upon themselves and extract from others the recognition or acknowledgement of that value if they are to have any fair prospect of "obtaining those things which a man from time to time desireth,"9 that is, any prospect of felicity. The competition for honor and dignity, for recognition, is a basic human need.

It is not my intention to follow Hobbes as he moves from consideration of man as the

⁴*Ibid.*, p. 120. Italics mine.

⁵Ibid., p. 129.

⁶Ibid., p. 99. Cf. also p. 131.

⁷*Ibid.*, pp. 72–73.

⁸ Ibid., p. 73. Italics mine.

⁹*Ibid.*, p. 55.

matter, the raw material, of that artificial man, Leviathan, to the construction or articulation of the commonwealth itself, nor to inquire whether the conventional equality he insists upon as the precondition for political life is consistent with the equality of all but the sovereign. For my purposes it is sufficient only to have shown how the theme of equality enters political life (in Hobbes) through the theme of recognition. The law of nature affirming equality is in reality an hypothesis of reason counteracting natural inequality and the natural disposition toward self-preference and pride. It is civil society, not nature, that is the true locus or ground of equality. The drive for recognition dissociates men; equality reconciles them, makes possible that concord that is the health of the body politic. The ultimate justification for equality in Hobbes is thus an awareness of the urgent need to suppress the drive for recognition (or its consequences) if men are to associate in a way that guarantees them survival and reasonable comfort. The justification is utilitarian, one which satisfies those needs or passions man may be said to share with all other creatures — (the natural drives clustered around the need for preservation), while jettisoning or sacrificing that need which is man's alone - (the peculiarly human but nevertheless still natural need of recognition). Hobbes establishes equality by abstracting humanity from political life, or by reducing men to mere animal nature. 10 It cannot surprise us, therefore, that the argument for equality in Hobbes does not derive from any recognition of man's common humanity or brotherhood or that it is not associated with any claim for the equal dignity to which men are entitled by virtue of their common humanity.

Implicit in all that Hobbes has said is the banal if melancholy conclusion that acknowledgment of a basic human need does not necessarily lead to measures that insure the fulfillment of that need. The human condition may very well frustrate its fulfillment. Things being what they are, it may indeed be the case that we must exchange our quest for honor and dignity for a loaf of bread. Moreover, being human is what makes possible the transmutation of human needs into animal needs, or what makes it possible for us to accept at all Hobbes's description of man as a description of ourselves. Hobbes is not simply asking us to check his reading of mankind against our own;

¹⁰Cf. John H. Schaar, "Some Ways of Thinking About Equality," *Journal of Politics*, Volume 26 (November, 1964), 877–879.

more importantly, he is inducing us to replace our reading with his own by altering the way in which we read ourselves. But it is impossible to believe that a basic human need can be made simply to disappear — sublimate it, rechannel it, even suppress it, it will still be there readying itself to force its appearance. As Horace reminds us: "You may drive out Nature with a pitchfork, yet she will always hasten back." 11

Because recognition is a phenomenon of the psyche, Hobbes ultimately deals with it by attempting to reshape the opinions that men have of each other, and thus the way they confront each other. This need not be the only way to take account of recognition, though we might think it the most consonant with the nature of the phenomenon itself. Locke is also concerned with the same phenomenon and similarly makes equality the precondition of genuine political life, though, as noted above, on the basis of an argument different from that of Hobbes. But here the relationship of equality to recognition is more oblique precisely because it was Locke's intention to deflect the concern with recognition from an obsessive preoccupation with the look of another to a concern for material well-being. Instead of contending with one another for affirmation of their status, men will busy themselves with the acquisition and enlargement of their properties. Locke thus shifts the locus of the struggle for recognition from the political to the private sphere, hoping thereby to achieve that civil peace and prosperity Hobbes thought to accomplish by the suppression of recognition altogether. Of course, this is only to deflect not to abolish recognition, and, in the event, men come to place a value upon themselves in relation to others by the size of their properties. That this had political consequences Locke both understood and welcomed.

On the other hand, Rousseau saw in any vestige of a struggle for recognition the seeds of moral and political corruption. It was Rousseau who understood that equality as a precondition of political life, if seen only as the possession by every citizen of the equal right to participate in the formulation of public policy, could very well coexist with inequality of social and economic condition and hence of status. These, in turn, would make a mockery of the equal rights themselves. Consequently, Rousseau bent every effort to so transform the nature of man while reconstituting him as citizen that the very sense of self and of particularity upon which the drive for recognition feeds would be

¹¹Horace, Epistles, Bk. 1, epistle x, 1.24.

obliterated. 12 Citizens thus emptied of their particular selves would be truly equal one with another. Indeed, with Rousseau, the view emerges that egalitarianism, far from being the precondition of political society is in fact the end or goal of political society, a position even more visible in Marx. But whatever permutations and combinations the concept of equality underwent in the course of the two centuries succeeding Hobbes, what is common to them all is the primacy of recognition as a basic human need, if not the basic human need. Whether the conflict is seen as one between superior and inferior, master and slave, individual and citizen, lord and serf, capitalist and proletariat; and whether the resolution is Leviathan, the General Will, the dialectic of History, or the victory of the proletariat - the underlying theme is the unrelenting obsession with the regard of another. Surveying the course of political philosophy since Hobbes, one may indeed say with Professor Leo Strauss that the course is marked by the assumption that man as man is thinkable "... as a being that is guided by nothing but a desire for recognition."13

We may note, however, two distinct and opposite dispositions toward the desire for recognition. There is, first, as in Hobbes, a concentration upon the deleterious consequences to human life of the necessity to be recognized. In early formulations, the very concept of a state of nature inhabited by solitary human atoms coming together only out of dire necessity suggests the superiority of a condition in which men, having no or no lasting associations with others, are not under any such constraints as the desire for recognition imposes. There men may live free from the anxiety generated by the concern with the regard of another. They are free, moreover, to live their own lives, taking as the measure of their life what is peculiarly their own, uncontaminated by any necessity to disguise them-

12 See Jean Jacques Rousseau, The Social Contract and Discourses, trans. G. D. H. Cole (New York: E. P. Dutton, 1950), pp. 18-19, 38-39, 307-308; Emile, trans. Barbara Foxley (New York: Everyman, 1966), pp. 7-8. Cf. also John Charvet, "Individual Identity and Social Consciousness in Rousseau's Philosophy," in Hobbes and Rousseau, ed. Maurice Cranston and Richard S. Peters (Garden City, N.Y.: Anchor, 1972), pp. 462-483; and John Charvet, "The Idea of Equality as a Substantive Principle of Society," Political Studies, Vol. 17, No. 1 (March, 1969), 6-7.

13 Leo Strauss, On Tyranny (Ithaca, Cornell, 1968), p. 205. The full quote is: "Both doctrines [i.e., Hobbes's and Hegel's] construct human society by starting from the untrue assumption that man as man is thinkable as a being that lacks awareness of sacred restraints or as a being that is guided by nothing but a desire for recognition."

selves or their being in order to please others. Even Rousseau's solitary, stupid, unimaginative savage has one basic redeeming feature; his life is an articulation of amour de soi, of that proper love of himself that arises from and is attuned to his own needs and to the sentiment of his own being. When conditions force him first to associate with others and then to form with them a civil society, this original, authentic self-love is subverted into amour-propre, vanity, that in which he derives his opinion of himself from the opinions of others. The process is ineluctable, there is no going back, but such reflections as these cast a pall over political life. Life together is purchased at the sacrifice of one's own.14

A wholly different attitude toward the phenomenon of recognition, one closer to the contemporary insistence upon esteeming another not in terms of his "price" but of his intrinsic worth as a "person," has its origins in the thought of Hegel and Marx. Here recognition is seen not only as a basic human need but also as the primary agency through which the process of becoming human is initiated and carried forward. In Marx, at least, it becomes as well the ultimate goal or quest of the human condition. A condition in which man does not recognize another human being as a human being, i.e., as his equal, is still a condition of mere nature, one in which man's true humanity is subordinated to necessity. Marx writes of the history of mankind as an evolutionary development of natural needs into human needs, the latter characterized precisely by and culminating in the recognition of human beings as human beings. 15 The delineation of genuine sexual relations in his Early Writings is a case in point. Such relations tell us "how far man's needs have become human needs, and consequently how far the other person, as a person, has become one of his needs, and to what extent he is in his individual existence at the same time a social being."16 Thus, far from seeking to subvert or even to rechannel the need for recognition (as Hobbes and Rousseau each in his own way attempts), the philo-

¹⁴There is, of course, an analogue in classical thought to Rousseau's depreciation of political life. But, whereas classical philosophy rejects political life in favor of the philosophic life, Rousseau does so in favor of the self, of the sentiment of one's own existence.

¹⁵ See Nicholas Lobkowicz, Theory and Practice (Notre Dame: University of Notre Dame Press, 1967), p. 356, and also, Shlomo Avineri, The Social and Political Thought of Karl Marx (Cambridge: Cambridge University Press, 1970), pp. 88-89.

¹⁶Cited in Avineri, p. 90.

sophical as well as political task is to facilitate and bring to final fruition the human destiny hidden in this peculiarly human need.

It may be difficult to discern the roots of today's concern with "transparency" and "person-to-person encounter" in Hegel's description of the archetypical struggle to the death between two individuals, each striving to extract from the other the recognition of the value he places upon himself; still, the basic need to be recognized in oneself as a person is common to both. In fulfilling this need, Hegel declares, man achieves that radical break with nature, in particular with his animal nature, that makes possible his humanity.17 What distinguishes man from all other sentient creatures is his self-consciousness, his awareness of himself as an I, as a Self, as something apart from and therefore not identifiable with his natural desires. This arises when and as man desires this or that object not as an animal desires for its own sake an object of warmth, food, or protection, but for the sake of the other "human" desires which have become attached to or embodied in the object. Thus a child may desire to keep himself clean, not for his own sake, but for the sake of experiencing the love of his parent that has become identified with the parental injunction about cleanliness. As Kojève explains: "It is human to desire what others desire, because they desire it. Thus, an object perfectly useless from the biological point of view (such as a medal, or the enemy's flag) can be desired because it is the object of other desires."18 Only such desires are truly human desires, and action undertaken in fulfillment of such desires constitutes human reality. We may recall Marx's illustration of sexuality. If I desire only the body of another, I remain at the level of animal desire; if, however, I desire the desire of another or the desire embodied in another person, I may be said to love as a human being loves. But to recognize such an embodied desire is to recognize it as emanating from a consciousness like my own; I do not simply respond to it as I might to an object in nature which either satisfies or frustrates my desires. And in "recognizing" the other as human, I confirm myself as human. But lest the other treat me as indistinguishable from any other

object in nature, and lest I thus lose the tenuous grasp of my own humanity, I will go to any lengths to extract from him a sign of my humanity. ¹⁹ As Kojève goes on to say: "Without this fight to the death for pure prestige, there would never have been human beings on earth. Indeed, the human being is formed only in terms of a Desire directed toward another Desire, that is — finally — in terms of a desire for recognition." ²⁰

There is a controversy among the interpreters of Hegel about whether the struggle for recognition will eventuate in a condition in which there are no longer any masters or any slaves, no longer any ruling or being ruled, in which all are equal. Certainly the Hegelian analysis affirms at least a kind of abstract equality, that of the reciprocal recognition of two centers of self-consciousness, and, by extension, of all such centers. Ultimately, this is the source of Hegel's affirmation of the equality of men as men: "It is part of education, of thinking as the consciousness of the single in the form of universality, that the ego comes to be apprehended as a universal person in which all are identical. A man counts as a man in virtue of his manhood alone, not because he is a Jew, Catholic, Protestant, German, Italian, etc..."21 It seems also the case that such equality is by no means to be translated literally into political life:

Men are made unequal by nature, where inequality is in its element, and in civil society that right of particularity is so far from annulling this natural inequality that it produces it out of mind and raises it to an inequality of skill and resources, and even to one of moral and intellectual attainment. To oppose to this right a demand for equality is a folly of the Understanding which takes as real and rational its abstract equality and its "ought-to-be."22

Whether, then, the struggle for recognition will terminate in a homogeneous world state, as Kojève avers, or whether a form of abstract equality (say, of "equal rights") will coexist in any future Hegelian state along with hierarchies of rule and of class division, as I believe, is not a controversy into which we can enter here.²³

¹⁷In what follows, I am indebted for the exposition of Hegel's section in the *Phenomenology* on Mastery and Slavery to Alexandre Kojeve, *Introduction to the Reading of Hegel*, ed. Allan Bloom (New York: Basic Books, 1969), pp. 3-30.

¹⁸Kojève, p. 6. See also John Plamenatz, *Man and Society* (New York: McGraw-Hill, 1963), Vol. II, p. 152.

¹⁹Kojève, pp. 9, 11.

²⁰Kojève, p. 7.

²¹Hegel's Philosophy of Right, trans. Thomas M. Knox (Oxford: Oxford University Press, 1949), No. 209, p. 134; also fn. to No. 270, pp. 168–169.

²²Ibid., No. 200, p. 130.

²³See the controversy between Strauss and Kojève in Strauss, On Tyranny, pp. 154-156, 183-184, 221-226. Cf. Plamenatz, pp. 155-156; George A. Kelly, "Notes on Hegel's 'Lordship and Bondage," in

It is important to note, though, that while contemporary scholars may depart in their analyses from the "heroic" form in which Hegel depicts the battle for recognition, many continue to affirm the absolute primacy of the phenomenon itself. Kojève, for example, maintains that "... the desire to be 'recognized' in one's reality and in one's eminent dignity (by those whom one 'recognizes in return') is actually, as I believe, the ultimate motive of all emulation among men. . . . "24 Plamenetz, who is himself critical of the Hegelian understanding of the form in which the struggle takes place, nevertheless adds, "I should not quarrel with Hegel for insisting that the desire for recognition is the pre-eminently human desire...."25 Finally, John Charvet in an analysis of Rousseau rejects the Social Contract because Rousseau therein suppresses "that element in man, namely his amour propre or desire for the respect of others and of himself, which constitutes his specifically human characteristic. . . . "26

As we have already seen, there is no unidirectional inference from recognition to equality, and the opposing pulls - toward equality or inequality - are not reconciled easily, if at all. For some, the logic behind the drive for recognition presses toward inequality, and extreme measures are indicated for the suppression of the drive in order to assure a measure of political equality. For others, suppressing the drive for recognition is equivalent to the suppression of that which is taken to be the hallmark of our humanity, and, moreover, it is precisely with the full realization of recognition that men attain to a genuine sense of equality. But even in regard to the genuine or truly human form of equality, the concern with recognition reflects also a fundamental ambivalence as we sometimes look to the recognition of our common humanity and sometimes to recognition of the concrete actual person that stands before us. As we shall see below, it is this latter demand that assumes for us the status of the primary moral and political obligation.

Value and Basic Human Need

Whether recognition is thus seen as the precondition for or the terminus of civil society, and however ambiguous the inference from recognition to equality, it is challenging that for three centuries it has been characterized as the basic human need, the ultimate motive, the pre-eminently human desire, the specifically human characteristic. Without neglecting the relationship of equality and recognition, it is appropriate to turn now to a closer look at the concept of basic human need. Political philosophy has traditionally been engaged in the search for the genuinely human, for whatever it is that constitutes human nature. It has been guided by the not unreasonable conviction that political society must in some way take its bearings from the genuinely human. It is hard to imagine how it could be otherwise. Among contemporary writers, Christian Bay explicitly calls for a "political science that deals with basic human needs as well as overt desires and other observable aspects of behavior."27 But Bay's proposals nevertheless reveal (as we might expect from one who balks at the inference from "fact" to "value") a curious ambivalence toward these basic human needs. Far from concluding that the identification of such needs determines the legitimate values of political life, Bay remarks instead that "the student of politics, once he has adopted a conception of human needs, should proceed from there to make explicit his inferences about political objectives and his choice of commitments with the utmost care."28

Not everyone, of course, is as hesitant as Bay in concluding value from need. Some, like Professor Edgar Bodenheimer, look upon basic needs as comprising the ontological structure or makeup of man and derive therefrom the values appropriate to human life. In a penetrating and suggestive article, "Philosophical Anthropology and The Law," Bodenheimer argues on the basis of modern psychological evidence and "a look at the contemporary scene" that the most important values that the structure of law is designed to protect and promote are security, liberty, and equality, and seeks to demonstrate that these values "have not been arbitrarily singled out as lodestars of legal regulation but that they have deep ontological roots in the

Hegel: A Collection of Critical Essays, ed. Alasdair MacIntyre (Garden City: Anchor, 1972), pp. 193, 214-217; and Sidney Hook, "Hegel Rehabilitated?" in Hegel's Political Philosophy, ed. Walter Kaufmann (New York: Atherton, 1970), pp. 60-63.

²⁴Alexandre Kojeve, "Tyranny and Wisdom," in Strauss, On Tyranny, p. 151.

²⁵Plamenatz, p. 188.

²⁶Charvet, "Equality," p. 8.

²⁷Christian Bay, "Politics and Pseudopolitics: A Critical Evaluation of Some Behavioral Literature," in Behavioralism in Political Science, ed. Heinz Eulau (Chicago: Aldine-Atherton, 1969), p. 112.

²⁸Ibid., p. 115. Italics mine.

constitution of human beings."29 But in the conclusion to the article, Bodenheimer acknowledges that his argument "depends on the tenability of two basic theses which are by no means generally accepted today."30 Of these, the first "is the proposition that man has a biological essence, consisting of physiological, psychological, and noetic constituents which cannot without peril be discarded in the legal regulation of human affairs," and, moreover, that "it is assumed ... that men have certain basic needs, over and above the most pressing physiological needs, which ought to be taken into account by lawmakers in the interest of the health of the social body."31 The second thesis is the related one "that the basic needs of men form an ontological foundation for the values protected by the legal order."32 Bodenheimer refers to the well-known list of basic needs identified by Abraham Maslow - physiological needs, the needs for safety, love and belongingness, esteem needs (the locus of our present concern with recognition and equality), the needs for self-actualization, knowledge and understanding, and also aesthetic needs - and clearly shares with Maslow the view that these needs are not "cultural artifacts," nor are they the expression of subjective and emotive responses to reality. The needs described (which form the "link to social and legal goal values") are "empirical traits of men which are not deemed to be subjective and idiosyncratic."33

Still, it is notorious that any positive and uniform identification of these empirical traits has escaped most investigators. In his book, Human Nature in Politics, James C. Davies treats us to a survey of what psychologists from Freud to Maslow have identified as the basic human needs, sometimes also described as wants, drives, or instincts. We cannot repeat the whole inventory. Suffice it to say that the list progresses from the eros and death instincts of Freud; to the thirty-two identified by William James: the seven major and several minor ones of social psychologist William McDougall; some ten or eleven described by Graham Wallas in his work on Human Nature in Politics including such exotic ones as the desire for combat and for adventure; the twelve "viscerogenic" and twenty-eight "psychogenic" needs listed by Henry Murray of the Harvard Psychological

Clinic; and finally, again, the several categories of needs identified by Maslow.³⁴ One might conclude from this embarrassment of riches only that the difficulty of the enterprise has so far precluded any final identification of the basic human needs. But while Davies himself admits that "knowledge of human motivation is not yet adequate to establish the psychological equivalent of a periodic table of the elements," it may be that the enterprise itself is thrown into question when he adds, "the garment of culture conceals the needs of the organism."³⁵

The difficulty is the familiar one of the observer attempting to shed the garment of his own culture in order to reach those human needs which allegedly underlie his own as well as all other cultures. This difficulty is not insuperable, as any traveler to a foreign clime may testify. Yet perhaps we have already conceded too much. For the unexamined premise here is that there are human needs which have been decorously or otherwise covered over by the cloaks of culture but that the needs themselves persist in some fixed or unaltered form. The premise is only another restatement of the aspiration that lay behind the concept of the state of nature: strip man of his cultural or political garments (hypothesize the condition of man in his precivil state), and he will stand revealed as the naked shivering self he is. Whereas Hobbes and Locke postulated the existence of "natural man" prior to his entry into civil society, modern day investigators locate natural man in civil society or within any given culture. But this is less of an advance than might appear on the surface. Whether natural man precedes or persists in civil society, he is not in any fundamental sense a creature of society. This is not simply to affirm that men are human only in a social condition but, more importantly, that there is no being human apart from society and thus what it means to be human is ineluctably already a product; not of man and society, but man-society. This is of course Rousseau's criticism of the concept of the state of nature: whatever that creature is which exists prior to civil society, it is not man, not a human being. Man becomes human; man is his history.36

²⁹Edgar Bodenheimer, "Philosophical Anthropology and the Law," *California Law Review*, Vol. 59, No. 3 (May, 1971), 662.

³⁰ Ibid., p. 679. Italics mine.

³¹ Ibid., pp. 679-680.

³²*Ibid.*, p. 680.

³³ Idem., and see also fn. 73.

³⁴ James C. Davies, *Human Nature in Politics* (New York: John Wiley, 1963), pp. 6-7. Cf. Rokeach, pp. 11, 14, 20.

³⁵ Davies, p. 7.

³⁶See Steven Lukes, *Individualism* (New York: Harper Torchbooks, 1973), pp. 150-152, and also Marvin Zetterbaum, "Human Nature and History," in *Human Nature in Politics*, (NOMOS XVII), ed. J.

scribed, the unabating search for basic human needs as if existing in some uncontaminated state must prove futile. Investigators have affirmed the cultural substrate even of the socalled biological drives:

Physiological disequilibrium, at least in human organisms, is so early and so thoroughly modified by social learning that pure or 'primary' needs do not exist except in theory.... At no point in the progression from the generalized tensions which include genital responsiveness, to the complex attitudes of fear, shame, or guilt over sexual behavior, can we isolate 'primary' sex needs from the matrix of social learning within which it develops. Like hunger, thirst, oxygen-deficit, or temperature imbalance, the disequilibrium of sex begins in a social context and is never separated from it.37

To put it another way, as Melford Spiro makes us aware, no one would categorize a society in which the activities of the members were centered exclusively around the search for food and mates and the avoidance of enemies as a human society.³⁸ A society is identifiable as human only when such drives articulate with an awareness of some comprehensive pattern that defines for its members what food is acceptable and what prohibited, who may qualify as a mate, and who is to be regarded as the enemy. Only in such a society does man become human. As Spiro adds: "Society is not only necessary for the survival of man, qua organism, it is crucial for the emergence of man, qua human. To develop a mind and a self it is necessary that the infant live in a human society and acquire its cultural heritage...."39 If the locus of becoming human shifts from organismic needs to society, it is no wonder that a search is undertaken for the invariant mechanisms or "structures" by which this is accomplished in every society, no matter what the "content" to which these structures give rise. Thus, for example, every society, it is alleged, is characterized by some comprehersive

Roland Pennock and John W. Chapman (New York: New York University Press, 1977), pp. 225-249.

If being human has the character just de- pattern or "world-view" which specifies not only what needs are to be met, indeed what are to be seen as needs at all, but also the manner in which these needs are to be fulfilled; and the manner assumes as much importance as the need itself.

Still, the search for invariant structures does not quite prepare us for the reappearance, in scarcely disguised form, of the need to be "recognized." It is thus surprising to find Spiro describing the process of becoming human as invariably and inevitably involving the awareness of and development of an individual self. "Self-awareness plays an enormous role in human behavior, for to most people their own selves are the objects of greatest value ... [and] regardless of the source, the integrity of the self must be defended against . . . threats to its esteem....'40 Having been warned against the danger of discussing the individual in isolation from society, we are nevertheless treated, once again, to the central priority of awareness of self to the individual. It seems, indeed, that we cannot escape the notion that the recognition and defense of self is a basic human need, if not the basic human need. Nevertheless, we cannot help wondering if, in the account just given, the investigator is not himself a prisoner of his own societal world view, one wherein the centrality of the self is sustained by a long historical, religious, and philosophical tradition. If indeed it is the case that the world view of each society is determinative, must it not determine not only the way an individual experiences himself but also whether he experiences himself as a self at all? And if he does not, what could possibly be the meaning for him of a struggle for recognition or of the need to defend himself against threats to his esteem?

If we seek empirical confirmation of the existence of a basic human need for recognition in all societies however different their worldviews - that is, if the issue is to be resolved by cross-cultural studies - we may be surprised to learn that the very concept of basic human need may have to be jettisoned. Dorothy Lee, for example, reports that in some societies the Trobrianders, the Lovedus, and the Wintus among others - no sense of superiority or inferiority is experienced because the sense of comparison that makes such judgments possible is altogether lacking. Of course, these cultures have not attained a perfect state of equality of condition, nor are their citizens identical to one another. Differences in functions and achieve-

[&]quot;Norman Cameron and Ann Margaret, cited in Melford E. Spiro "Human Nature in Its Psychological Dimensions," American Anthropologist, 56 (February, 1954), 19-30, at p. 24. On the cultural components of such concepts as time, space, motion, object, and people, see Jules Henry, Pathways to Madness (New York: Vintage, 1973), pp. 11-24. Cf. Karl Jaspers, "The Search for Being," in The Existentialist Tradition, ed. Nino Langiulli (Garden City, N.Y.: Anchor, 1971), pp. 167-168.

38Spiro p. 25 37 Norman Cameron and Ann Margaret, cited in

³⁸Spiro, p. 25.

³⁹ Ibid., p. 24. Cf. p. 27.

⁴⁰Ibid., p. 28. Cf. Rokeach, pp. 14-15.

ment exist here as in other cultures, but individuals are not evaluated in terms of these roles or accomplishments. Value accrues to the individual simply because of the uniqueness of the person. Lee believes she has found a species of equality in such societies which "derives from the recognition of the right to be different, noncommensurate, unique; from the valuing of sheer being."41 Paradoxically perhaps, in these societies there is no sense (sometimes not even the word) of a unique "self." If it is difficult for us to conceive of the members of a society acknowledging (recognizing?) the uniqueness and hence value of each of the members of that society without resorting to the concept of self, it may be that, since Hobbes, we have been unable to think of self or even individual except in the context of a sense of self extracted from or wrenched from the unwilling hold of the other. Lee maintains:

Equality was the solution appropriate to a culture where self is opposed to other, where society had to be protected from the encroachment of the individual; the solution of a society which holds an either-or conception of duality. There are societies, however, where dualism is complementary, so that the terms of the duality are not opposed nor measured against each other, nor seen as discrete units. 42

Lee's argument might be thought thus to undercut the hypothesis of a universal need for recognition in the Hobbesian and Hegelian sense while nevertheless affirming a purely formal (contentless) basic human need for recognition, one whose specific make-up is supplied by each separate culture. But to draw this conclusion would be to misunderstand the force and direction of Lee's own comprehension of the relation of value to need. For, in keeping with a widely held view, she notes that "everywhere, in every society, the culture offers its peculiar codification of reality, its peculiar avenues of experience."43 Moreover, the value premises of a society are not mere glosses superimposed upon some underlying foundation of invariant basic human needs. In an article significantly entitled, "Are Basic Needs Ultimate?", Lee argues that the concept of basic needs as the cause of behavior is entirely erroneous, that, in fact, needs are derivative, not primary. What are seen or experienced as needs in a given culture are seen or experienced as such because of the overarching value structure of that society, its worldview. ("To the Hopi, corn is not nutrition; it is a totality, a way of life."44) Taught in our society to esteem individualism, privacy, and sense of self, we generate the need for individualism, privacy, and sense of self, and we structure our familial, social, and political experiences to fulfill these "needs."45 On this view, the simplistic understanding of human behavior arising in response to needs - that is, to some biologically given, unidirectional impulses, tensions, or deficiencies - is wholly inadequate to the phenomena. Nor will the ever expanding number or categories of needs serve to remedy the defect. Lee concludes that "culture is not ... 'a response to the total needs of a society'; but rather a system which stems from and expresses something had, the basic values of the society."46 From this perspective, the Hobbesian-Hegelian need for recognition is but a reflection of the worldview from which Hobbes or Hegel (more accurately, their culture) saw man, one in which the struggle to achieve identity and self-consciousness occupied the very center. Hardly a basic human need at all.

If basic human needs derive from value; if value is not, after all, either derivative from, an expression of, or even a transformation of some constituent human need, then we are compelled to wonder how we may escape the conclusion of an ineluctable moral relativism. For we are forced to wonder about the sources or the status of the comprehensive worldviews or value premises that structure for each culture its view of reality and of human need. Time and space do not permit us to rehearse the arguments said to undermine each of those supports mentioned in the first paragraph of this paper that might provide for value a foundation independent of chance or man's arbitrary will. We are familiar enough with contemporary discussions of the legitimacy of so-called value judgments where recourse is taken, finally, to an unsupported and unsupportable moral postulate. Such recourse has the character of and frankly proclaims itself as a secular act of faith. Justifications for equality as the primary human value often appear in just such a guise. Surveying the support for the concept of the dignity of man from its biblical origins to the

⁴¹Dorothy Lee, "Equality of Opportunity as a Cultural Value," in Lee, *Freedom and Culture* (Englewood Cliffs: Prentice-Hall, 1959), p. 44.

⁴² Ibid., p. 48. Italics mine. See also in the same volume, "The Conception of Self Among the Wintu Indians," pp. 131-132, 134.

⁴³*Ibid.*, p. 46.

⁴⁴Dorothy Lee, "Are Basic Needs Ultimate?", Journal of Abnormal and Social Psychology, 43 (July, 1948), 391–395, at p. 393.

⁴⁵Idem.

⁴⁶Ibid., p. 394.

present, Steven Lukes concludes: "In general, this idea of the dignity of the individual has the logical status of a moral (or religious) axiom which is basic, ultimate and overriding, offering a general justifying principle in moral argument." As such, it would be beyond any possible verification, serving instead as the source from which moral arguments may proceed. But we could not then avoid inquiring about the primacy of this rather than of some other moral axiom.

We may conclude the present section with just one contemporary instance of this type of moral reasoning. "Equality," Sir Isaiah Berlin tells us, "is one value among many; the degree to which it is compatible with other ends depends on the concrete situation, and cannot be deduced from general laws of any kind; it is neither more nor less rational than any other ultimate principle; indeed, it is difficult to see what is meant by considering it either rational or non-rational."48 Equality does not flow from any rights inherent in being a man; it is not a necessary logical deduction from Christian theology; it is not an "inalienable" element in the "ultimate structure" of reality.⁴⁹ It is however "one of the oldest and deepest elements in liberal thought, and is neither more nor less 'natural' or 'rational' than any other constituent in them. Like all human ends it cannot itself be defended or justified, for it is itself that which justifies other acts - means taken toward its realization."50 Still, the impoverishment of such a view is evident when, inconsistently, Berlin warns against the thesis that "the commandment to treat all alike in like situations needs no independent argument to support it."51 For, we must ask, what could possibly constitute the substance of an argument in favor of even this aspect of equality when we have already been told that the belief in equality itself rests neither in God, in nature,

47Lukes, p. 51. Cf. Schaar, "Some Ways": "The older, pre-eighteenth century idea of a common humanity, often denied but never forgotten, was a moral assertion based not upon a generalization of experience but upon a postulate of reason and an exercise of faith" (pp. 876-877).

48 Isaiah Berlin, "Equality as an Ideal," in Justice and Social Policy, ed. Frederick A. Olafson (Englewood Cliffs, N.J.: Prentice-Hall, 1961), p. 150. See also The Sociology of Georg Simmel, trans. and ed. Kurt H. Wolff (New York: The Free Press, 1950), pp. 73–74; and also Pepita Haezrahi, "The Concept of Man as End-in-Himself," in Kant: A Collection of Critical Essays, ed. Robert P. Wolff (Garden City, N.Y.: Anchor, 1967), pp. 302, 311–313.

nor in reason, and that, in fact, it "cannot itself be defended or justified." About all we have left to stand on is the honor deriving from Joseph Schumpeter's dubious distinction between ourselves and the barbarian: "To realize the relative validity of one's convictions and yet stand for them unflinchingly is what distinguishes a civilized man from a barbarian." 52

Recognition and Political Life

The point of traversing the maze of the doctrine of recognition, whatever its meanderings from Hobbes to Rousseau to Hegel and Marx, was that it seemed to promise a way of grounding one value, equality, in what was characteristically and undeniably a basic human need. The doctrine of recognition had, moreover, the twin virtues of, on the one hand, not relying upon extrarational sources for its support, and, on the other (once the doctrine was shorn of its original orientation in Hobbes) of avoiding the disposition to reduce human things to the merely natural. What was sought was a human value deriving from a human need.53 It promised also at least partially and modestly to meet the requirements that Herbert Spiegelberg set forth for a fully adequate defense of human equality: "...a complete study of the human structure, of human values, of human rights and duties, of human dignity and destination, and, moreover, a determination of their relative significance within the framework of human nature."54 Not only, however, must we concur with Professor Schaar that "our age has been unable to construct a philosophical anthropology, a general answer to the question, what is man"?55 but also the wreckage that surrounds us forces us to doubt the wisdom of having undertaken the inquiry in the first place. It might not have been fruitless if we had had only to relinquish the search for the basic human need which justifies equality; what is disconcerting is the conclusion that no human value can ever be said to derive from any basic human need.

⁴⁹Berlin, p. 128.

⁵⁰Ibid., p. 150.

⁵¹*Ibid.*, p. 148.

⁵²Joseph Schumpeter, Capitalism, Socialism and Democracy (New York: Harper Torchbooks, 1962), p. 243. It is precisely this sentence that Berlin quotes approvingly in his Four Essays on Liberty (London: Oxford University Press, 1969), p. 172.

⁵³Cf. Schaar, "Some Ways," p. 881.

⁵⁴Herbert Spiegelberg, "A Defense of Human Equality," *The Philosophical Review*, Vol. LIII, No. 2 (March, 1944), 101–124 at p. 106. Cf. Alfred Schutz, "Equality and the Meaning Structure of the Social World," in *Aspects of Human Equality*, ed. Lyman Bryson et al. (New York: Harper, 1956), p. 37.

⁵⁵Schaar, "Some Ways," p. 875.

The sole positive conclusion that emerges is, simply: there are "n" ways of becoming human. Indeed, the only defining characteristic of humanity is its infinite capacity to vary the ways of becoming human. Paradoxically, it might be thought that we could elicit from this conclusion a requirement of respect (recognition) for all those who display this characteristic, that is, a respect for human beings as human, and so arrive again at a ground for the value of equality. 56 Despite an understandable weariness over the discovery and proclamation of one more indisputable cogito from which our understanding of all human things would flow, there is still an almost irresistible attraction in the comprehensiveness of the conclusion itself. The new cogito practically forces itself upon us; it is faithful to the reality of being human; it readily accommodates itself to the phenomena we have been examining; it dissolves the paradoxes and difficulties over the relation of need to value; and, as a quite unexpected bonus, it provides as well (out of the ground of recognition) an objective foundation for the requirement of equality.

We may, however, have stumbled upon fool's gold. Our new cogito answers the question "what is man?" with "man is the creature that varies his way of being"; but it must be painfully obvious that this revelation falls far short of what we should wish from a fully developed philosophical anthropology. It is in the first instance spare and appallingly abstract. It directs us to the source of human values but reveals nothing about the means of differentiating among them, and so leaves us wholly in the dark as to our rights and duties. In a formal way it locates for us the special source of our human dignity, but is silent about our destination and about our obligation, if any, to the rest of the cosmos. It is simultaneously an invitation to innovation and revolution and a defense of what has already come into being, for what has come into being is just as much a way of being human as that which seeks to replace it. And, finally, it is either meaningless or self-contradictory, or both, in that it provides no way of distinguishing (or of approving or rejecting) as human or nonhuman any of the "n" ways of being human. It is not inconsistent

56See, e.g., Philip Selznick, "Natural Law and Sociology," in Natural Law and Modern Society (Cleveland: World Publishing, 1963), pp. 168-169.

with the *cogito* to live a human life indistinguishable from the animal, or to live in preparation for the divine. It is as human to choose to live among your fellows as it is to live alone. It is as human to live a life of philosophy as of politics; a life of "Peace, Good Will, Mutual Assistance and Preservation" as of "Enmity, Malice, Violence, and Mutual Destruction." It is as human to recognize your fellows as your equals as it is to deny that equal recognition.

Still, our short-lived cogito bears a marked family resemblance to other attempts to set forth whatever it is in virtue of which we recognize others as our equals. Respecting others for this capacity to construct a way of life for themselves is a way of looking at equality that has both scriptural and secular, principally Kantian, overtones. The classic account of the struggle for recognition in Hobbes and in Hegel contained, as noted above, a crucial ambiguity. In its archetypical form, the struggle is between two individuals each seeking to extract from the other the recognition of himself not as "human being" but as this particular person embodying the particular value he places upon himself. But in the development of the theme of recognition from Hobbes to Rousseau and Kant and on through Hegel all particularity is lost sight of as the emphasis shifts from the concrete to the common. The equality that is sought or defended is the equality of everyone as a person, as a human being, entitled thereby to equal treatment before the law, equal participation in the making of law, equality of opportunity, in general, "equal rights." The original drive for recognition is thus transmuted by Kant and Hegel into a search for the universal self or ego or reason or will in virtue of which each individual is a human being. In this process, the concrete unique individualized particular self shrivels in succumbing to the universal self.⁵⁷ As Williams remarks of Kant:

Kant's view not only carries to the limit the notion that moral worth cannot depend on contingencies [i.e., empirical factors] but also emphasizes, in its picture of the Kingdom of Ends, the idea of respect which is owed to each man as a rational moral agent — and, since men are equally such agents, is owed equally to all, unlike admiration and similar attitudes, which are commanded unequally by men in propor-

⁵⁷Sentiments proclaiming the absolute value of the individual can also be found in Hegel. See *The Philosophy of Hegel*, ed. Carl J. Friedrich (New York: Modern Library, 1954), p. 20.

tion to their unequal possession of different kinds of natural excellence. . . . 58

Our cogito has thus a quite respectable ancestry. It, too, abstracts from the empirical complexity of life to reach that which is thought to be true of every human being - the capacity to vary one's way of life - and thus does not require admiration for the constructions or ways of life themselves. It recognizes the equality of "personhood" rather than that of this or that person.

But if our cogito can lay claim to a decent lineage, it and all its ancestors are in serious difficulty. Consider the approach Steven Lukes pursues in his recent study, Individualism. 59 According to Lukes, when we admire, praise, or reward an individual we do so on the basis of some acknowledged accomplishment or achievement; if, on the other hand, we respect another human being, we do so without regard to any specific accomplishment and even in the absence of any.60 In offering such respect, we are not acknowledging some inner urge for self-confirmation through recognition by the other, but we are, instead, responding to the awareness we have of the characteristics that that individual has in common with all others. What we respect are potentialities or capacities for reflection, acts of courage or justice, aesthetic sensitivity and the like. Lukes is correct, I believe, in maintaining that once one has accepted this mode of argument and has thus already abstracted some pure inner "core" from the actual content of human experience. questions of degrees of capacity or of actualization are irrelevant.61 He admits that the accounts of the essences of these common human characteristics have differed widely: "The Christian ground for equal respect is that all are equally children of God, while the Kantian argument is that they all have free and rational wills and are members of the Kingdom of Ends. Some have suggested that there are empirical features which all human beings share which provide grounds for treating them with equal

respect...."62 He himself offers up three such empirical characteristics: "the capacity for autonomous choice and action, the capacity to engage in valued activities and relations that require a private space, and the capacity for self-development..."63 It is too awful to contemplate beginning our inquiry anew by substituting "capacities" for basic human needs. We have little assurance of any more sanguine results. The inventory Lukes offers partly overlaps that put forward by Bernard Williams in his study of the idea of equality.⁶⁴ Williams's list, in turn, has been criticized by Schaar as neither indisputable nor terribly clear, and not even particularly useful.65 Certainly we have no wish to repeat the litany of capacities as we did that of basic human needs.

It was not, however, my intention to review the list of human capacities proffered by Lukes. Rather, it was to consider, as implied in his preference for "capacities" over, say, "wants," "motives," or "interests," that other aspect of the drive for recognition — the need to be recognized in all one's concreteness, not simply as a representative unit of the human species. Lukes joins those who have characterized the philosophers of the eighteenth century as excessively prone to abstraction in their contemplation of humanity.66 His objections parallel Schaar's criticism of the "liberal and constitutional theorists of the modern era" who created a "theoretical everyman" bearing little if any relevance to real men, to "flesh and blood resident[s] of the world." Schaar writes:

This theoretical everyman is conceived as having but one, or, at most, a few attributes which are relevant to the construction of a political order. He is a generalized and diminished man, reduced to fear (Hobbes) or will (Kant) or interest (Madison) or the passion for distinction (John Adams) or pleasure-pain (Bentham). He is an elementary stimulus-response mechanism, a quantum of energy which is set into motion under specifiable conditions and directed toward specifiable ends. 67

Simmel hammers the point home with great force and clarity: "... Man in general, man as such, is the central interest of the period; not historically given, particular, differentiated man. Concrete man is reduced to general man: he is the essence of each individual person. . . . In terms of the pure concept of mankind, all

⁵⁸Bernard Williams, "The Idea of Equality," in Philosophy, Politics, and Society, (Second Series), ed. Peter Laslett and Walter G. Runciman (Oxford, Blackwell, 1962), p. 115. But see Williams's demurrer, pp. 116-117, and cf. Charvet, "Idea of Equality," pp. 11-13; Haezrahi, p. 294 and fn. 5; Spiegelberg, p. 108; Simmel, pp. 72-73; Pierre Hassner, "Immanuel Kant," in History of Political Philosophy, (2nd ed.), ed. Leo Strauss and Joseph Cropsey (Chicago: Rand McNally, 1972), pp. 559, 561. 58Bernard Williams, "The Idea of Equality," in

⁵⁹See fn. 36 above.

⁶⁰Lukes, p. 126.

⁶¹ Idem. Cf. Schaar, "Some Ways," p. 880.

⁶²Lukes, p. 126.

⁶³Lukes, p. 133.

⁶⁴Williams, pp. 112-114.

⁶⁵ Schaar, "Some Ways," p. 880.

⁶⁶Lukes, pp. 146ff.

⁶⁷Schaar, "Some Ways," p. 885.

men are essentially alike. Compared with this general element, all differentiated individuality is something external and accidental."68

As we shall see below, the objection of these writers is not to the use of abstraction as such, but to a mode of abstraction that is false to the reality of being human. Still, as' we know from Burke, a concept's falsity is no bar to its being useful, even essential. So, Lukes acknowledges, "...there is no doubt that historically, the abstract conception of the individual represented a major moral advance. It was a decisive step in the direction of a universalist ethics when human beings first came to be regarded as the possessors of certain rights and claims, simply in virtue of being human."69 Schaar has similarly called attention to the very positive benefits which accrue from considering all men as equal in certain respects and in thus establishing a public realm as "the area of uniform and equal behavior ... in order that [all men] will be able to express their inequalities in the private realm." The public realm so created demarks a carefully circumscribed arena in which citizens are accountable to the state for their actions; beyond this arena, they are free from governmental oversight. This particular postulate of equality thus legitimates the traditional liberal sanctuary of the private sphere. Moreover, Schaar adds, the postulate of equality allows us to conduct our affairs with all the anonymous others with whom we come in contact without having to consider each individual in his or her own uniqueness. 71 Justified by the abstract principle of equality, we impartially apply a uniform rule.

Still, such benefits are not without their costs. Like Tocqueville before him, Schaar fears that the real, full, many-faceted individual will begin to take on the characteristics of the narrow, abstract, diminished man of the public realm and so become transformed by the very instrument thought essential for his preservation. Taught, say, to consider ourselves in the public realm solely as predatory pursuers of economic interest, we come to behave just this way in the private sphere.⁷² There is, additionally, concern for the character of public life itself; it, too, of course, is altered, di-

minished, by being populated with "abstract men" who precisely because they are abstractions, and insofar as they remain abstractions, cannot matter to us or become the objects of our affection.⁷³ While Lukes recognizes the contribution the concept of the abstract individual makes to equal treatment before the law and freedom from arbitrary handling, indeed recognizes these as "crucial and indispensable gains," he nevertheless insists that "if we are to take equality and liberty seriously, they must be transcended."⁷⁴

We need not wonder whether the inspiration behind the appeal to abstract man was noble, ideological, or utilitarian. What is finally decisive is that such an appeal is theoretically false. It is false, so the familiar argument goes, because it conceives of human nature as constituted by needs, instincts, drives, motives, whatever, which are in their essence unaffected by social and historical change. But such a view, according to Lukes, is emphatically denied by those who see society as fundamentally constitutive of the individual. A "sociological apperception" rejects the theory of the abstract individual whether conceived of as existing in some precivil condition or as persisting within society itself.75 Instead of abstract man, we must speak of a social self. As may be readily seen, these are also the arguments, noted above, of those who deny the concept of basic human need. Taken together, these arguments render meaningless the search for that "essential self" (by whatever name it goes) that is thought to define for each of us our common humanity, and in virtue of which we affirm the equality of every human being. It is inconceivable that a legitimate justification for the value of equality could arise from a view of man that is demonstrably false, false to the reality of being human, false to the very possibility of being human. 76

I have argued elsewhere that the concept of the "social self" is not free of its own internal

⁶⁸Simmel, pp. 67-68. See also, *ibid.*, pp. 58-84.

⁶⁹Lukes, p. 149.

⁷⁰Schaar, "Some Ways," p. 886.

⁷¹Ibid., p. 887. Cf., ibid., p. 884.

⁷² Ibid., p. 895. See also Schaar's description of the political, p. 889. Cf. Marvin Zetterbaum, Tocqueville and the Problem of Democracy (Stanford, Calif.: Stanford University Press, 1967), pp. 142-144.

⁷³Schaar, "Some Ways," p. 890.

⁷⁴Lukes, pp. 152-153. Italics mine.

⁷⁵Ibid., pp. 146, 151, 152.

⁷⁶Cf. Schaar, "Some Ways," p. 877–879, and the following quotation at p. 893: "The constitutionalists... have offered a specifically political way of thinking about equality, a way which owes little to observation or to the moral sense. Their everyman is a construct, not an empirical portrait.... We cannot, without vast intellectual confusion and surprising practical consequences, deal with the construct either as an empirical generalization, or as a full guide to the making of public policy, or as an ethical justification for equal treatment." See also Charvet, "Individual Identity," p. 483; Charvet, "Idea of Equality," p. 13.

difficulties.77 In the context of the present inquiry, however, I wish simply to accept the view that the idea of the "abstract" individual is not tenable theoretically and to speculate on what the transcendence of such a view would entail for political man. It is apparent that the direction in which this view is to be transcended is toward that too maligned or neglected other aspect of the drive for recognition: the desire to be seen by the other not as a human being but as this particular individual here and now, to be recognized by the other not in virtue of any capacity as a moral agent or of the freedom to alter one's way of life but as this specific human being with these specific virtues and these specific faults - as a lover longs to be loved by his beloved for himself alone. It would be erroneous to locate the source of this aspiration solely in the contemporary concern with alienation, our experience of ourselves as a mere cipher among all those anonymous others, our failure to discover and to preserve a sense of self either in our everyday existence or in our public life. Alienation accentuates and makes all the more poignant the human desire to be recognized in one's concrete reality - a desire from which not even the philosopher's quest, for all its alleged self-sufficiency, is, I believe, immune. 78 No claim can possibly be offered here (especially in view of all that has preceded in the present paper) to have established this as a permanent human characteristic, need, or aspiration. Whether that can indeed be done on the basis of a fully articulated philosophical anthropology is something I am not prepared to say. But, as Socrates observed a very long time ago, it is not always necessary to have proved the existence of something before entering upon a discussion of it.⁷⁹ In any event, it is this other feature of the drive for recognition, one that balks at the effacement of the specific individual before some concept of the "everyman," that is at the heart of the demand for equality today.

What is at issue here, it must be noted, is not the venerable problem of the application of a "standing Rule ... common to every one of that Society" to a particular individual or case, for it is precisely the appropriateness of the standing rule that is in question or, more exactly, the status of the standing rule itself. That we are each entitled to equal rights, that we "should all be treated in a common or average way, so that the minimal conditions of a common life are made available to all..."80—these propositions rest finally on some claim of a common humanity. How will these fare if we are urged now to direct our attention away from the common to the real individual before us?

We need hardly pause to cavil at the prospects of instant bureaucratic intimacy, the counterfeit hello of the checkout clerk, the mass-produced personalized letter, the mandated air of deep personal concern of the local official. No one accepts these as genuine signs of recognition. But we do well to wonder whether the rejection of the common might not become the invitation for subjection to "the inconstant, uncertain, unknown, Arbitrary Will" of another. As Schaar has pointed out, there never has been a public realm populated with "abstract" men - we have always to deal with real men, men with strongly held views, passions, predilections, strengths, and weak-nesses.⁸¹ Certainly American political life has as often been a government of men as one of law, if such a distinction is meaningful at all except in theory. But a politics of real men operating within the constraints of a framework of common law is surely very different from such a politics devoid, say, of the safeguard of a constitutional structure. Nor is just any sense of the "common" threatened by the attacks upon abstract individualism - rather, it is precisely that common which promised to secure for each the equal regard of the law. If some maintain the difficulty if not the impossibility of defending equal treatment on the basis of some alleged characteristics we all share,82 how much more difficult will it now become to treat with minimal decency the lazy as well as the industrious, the foolish as well as the rational, the sinner as well as the saint? For we are assured that the abstracted common which obliged us to treat our fellows as our equals is theoretically untenable. Moreover, as noted above, it is precisely this sense of the common - in the form of the postulate of abstract equality - that has traditionally been the

⁷⁷ Marvin Zetterbaum, "Self and Political Order," Interpretation, Vol. 1, No. 2 (Spring, 1971), 240-246. See also, Lukes, pp. 152-153.

⁷⁸See the controversy on this point between Strauss and Kojève, in Strauss, *On Tyranny*, pp. 165-175, 218-221.

⁷⁹ Plato, Republic, 472d.

⁸⁰ John H. Schaar, "Equality of Opportunity, and Beyond," in Contemporary Political Theory, ed. Anthony de Crespigny and Alan Wertheimer (Chicago: Atherton, 1970), p. 151. Cf. Schaar, "Some Ways," p. 077

⁸¹ Schaar, "Some Ways," pp. 891-892.

⁸² Ibid., p. 881. But cf. Schaar, "Equality of Opportunity," p. 152.

safeguard of that private sphere where individuals may nourish their freedom and uniqueness. What is now to preserve this sanctuary from the onslaught of the "sociological apperception"? While the private sphere remains even for Lukes⁸³ an indispensable element of genuine individuality, it is the legitimacy of this private sphere that is called into question by the attacks upon abstract individualism.

According to an ancient tradition, the study of political things appropriately commences, as in the Politics, with the family, with marriage and the household. This tradition is continued by Martin Buber. While Buber has of course long been identified with the need for personto-person encounter or dialogue, for meeting between real individuals, he has also resisted the temptation to confuse such encounter with political life. Buber looks upon marriage, real marriage as an encounter with otherness, one in which the otherness, the particular being, of the partner is affirmed. Marriage thus leads "to insight into the right and legitimacy of otherness and to that vital acknowledgment of many-faced otherness"84 which is the essential characteristic of social or political life. But while marriage may be a preparation for the otherness of the polity, Buber insists that "there is a difference between the private sphere, to which marriage belongs, and the public sphere of existence." In the private sphere a man may identify himself in all his concreteness with the members of his family in all their particularity. When he says, "we" of this family, "he means not merely the whole, but also the single persons recognized and affirmed by him in their particular being." But with respect to the public sphere, identification is qualitatively different, for such identification (this is "my" country; these are "my fellow citizens") cannot "embrace the concrete persons in a concrete way."85 Buber is even more insistent upon the difference when he comes to develop his concept of the interhuman, of genuine person-to-person relationship:

We may speak of social phenomenon wherever the life of a number of men, lived with one another, bound up together, brings in its train shared experiences and reactions. But to be thus bound up together means only that each individual existence is enclosed and contained in a group existence. It does not mean that between one member and another of the group there exists any kind of personal relation. They do feel that they belong together in a way that is, so to speak, fundamentally different from every possible belonging together with someone outside the group.... In no case, however, does membership in a group necessarily involve an existential relation between one member and another.86

The recognition and affirmation of concrete otherness which may obtain in marriage or friendship, then, is different from the recognition of generalized otherness of political life. To confound the two is to place demands upon the polity which often strain the limits of even the best marriages. It is to confound the polity with an encounter group.

We can legitimately ask whether the political can be understood in any other way than as the locus of the common, of something abstracted from each of the individuals encompassed within a given regime. When, in speaking of the transcendence of the concept of abstract individualism, Lukes says that it "can only be achieved on the basis of a view of unabstracted individuals, in their concrete, social specificity...,"87 he is not, as he is himself aware, abandoning all abstraction. In fact, he contrasts his abstraction, the concept of the person, with that of abstract individualism which sees the human being as having certain invariant features that determine his behavior.88 The person, on the other hand, is viewed as having certain capacities, as mentioned above, whose degree and form of realization "is left open for investigation." The concept of the abstract individual is part of a closed system; the concept of the person permits the particularities of the individual to be seen and recognized. But to ground the value of equality in the capacities of every individual for selfdevelopment, etc., is of course to abstract such capacities from the individuals concerned. We see individuals differently, then, but we nevertheless see them as having something in common. And we certainly do not affirm the equality of all men with respect precisely to their individual differences.

Perhaps this is only another way of saying that the demand for recognition in public life may take the form of respect, after all, for something understood as common to all — for equality in being, in simple otherness, or in

⁸³Lukes, p. 132.

⁸⁴Martin Buber, Between Man and Man (New York: Macmillan, 1965), p. 61.

⁸⁵ Ibid., p. 62. Cf. Aristotle, Politics, 1261b16-1262a14.

⁸⁶Martin Buber, The Knowledge of Man (New York: Harper Torchbooks, 1965), pp. 72-73.

⁸⁷Lukes, p. 153.

⁸⁸*Ibid.*, p. 146.

⁸⁹ Idem.

some yet-to-be-discovered characteristic of human beings. But the demand for equality which emerges out of the recognition of concrete individuality is not a legitimate aspiration of political life. If equality cannot be derived from the one aspect of the drive for recognition—the common—it is wholly out of place with respect to the other—the particular. It is of the latter that Schaar has truly noted, "notions of equality have no relevance." "It is easy," he

reminds us, "to abstract a part from the whole and define that part in terms that make it commensurable with the same parts abstracted from other whole men.... But when it comes to talking about whole men and about man, the concept of equality is mute. Then there is only the mystery of being, the recognition of self and others." 90

90 Schaar, "Equality of Opportunity," p. 152.

Collective Choice, Separation of Issues and Vote Trading*

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The final outcome of a legislative or committee procedure is a bundle of positions, one on each of various issues. Instead of choosing among such position-bundles directly, legislatures and committees vote on each issue separately.

What effect does this separation of issues have on collective choices?

To investigate this question, I will construct a relatively abstract model of collective choice separated by issues. The main assumption of the model is (roughly) that at least some of the issues voted on separately are really separate in the sense that they can be decided independently of one another. Beyond that, the model is quite general. Nothing is assumed, for example, about the procedure used to decide each issue. It might be simple majority rule; then again, it might be something totally outlandish, perverse or undemocratic.

In a way, vote trading (log rolling) unseparates separate issues, combining positions on distinct issues to form single legislative packages. I will examine the effects of vote trading and of its absence. Assuming the model, if there exists a feasible outcome not dominated by any other — one to which no other feasible outcome is preferred by a majority, or

*I presented earlier drafts of this paper at a Conference on the Foundations of Political Economy, University of Texas at Austin, February 1975, and at the annual meetings of the Public Choice Society, Chicago, April 1975. I profitted from the comments of the discussants, Peter Fishburn, Russel Hardin and Joe Oppenheimer at Austin, and Steven Brams at Chicago.

I am grateful to Joe Oppenheimer for stimulating discussions on the topic on various occasions, and to Martin Baily and Peter Bernholz for discovering a number of important errors. I owe a special debt of gratitude to Bernholz. This paper grew out of an extensive, extremely fruitful correspondence with Prof. Bernholz, occasioned by his paper, "Log-Rolling, Arrow-Paradox and Decision Rules," Kyklos, 27 (November, 1974), 49-62.

by whatever counts as a winning coalition — then that outcome will automatically be chosen in the absence of vote trading. If some majorities (or whatever) fail to coalesce to protect the common interests of their members, then it turns out to be rather easy for organized minorities, trading votes, to frustrate the wishes of these disorganized majorities, ensuring the collective choice of an outcome dominated by every other feasible outcome. Otherwise, the chosen outcome must be undominated if any is, and a reasonable choice set or solution concept can be defined for the case where every outcome is dominated.

The assumptions of the model are presupposed (along with much else) by many less abstract models of democratic processes, including spatial models. Somewhat similar models have been investigated by Kadane, Oppenheimer² and Bernholz.³ Mine is considerably more general, and the lessons I draw from its consequences are very different. For example, Oppenheimer and Bernholz have argued that, assuming their models, rational vote trading necessitates a "voting paradox" situation. That is not generally true, although a weaker result can be proved.

The Model

There are n issues, $\omega_1, \ldots, \omega_n$, each a set of alternative positions. The positions constituting each ω_i are feasible, not merely conceivable.

¹Joseph B. Kadane, "On Division of the Question," Public Choice, 13 (Fall, 1972), 47-54.

² Joe A. Oppenheimer, "Relating Coalitions of Minorities to the Voter's Paradox, or Putting the Fly in the Democratic Pie," paper delivered at the Annual Meeting of the South West Political Science Association, San Antonio, March 30-April 1, 1972.

³Peter Bernholz, "Log-Rolling, Arrow-Paradox and Cyclical Majorities," *Public Choice*, 15 (Summer, 1973), 87-102; and Bernholz, "Log-Rolling, Arrow-

In any interesting or realistic example, $n \ge 2$ and each ω_i has at least two members. Typically, indeed, each ω_i has just two members: passage and defeat of some motion. While these assumptions hold in all examples used below, there is no need to include them in the model.

Intuitively, a feasible outcome is a bundle of positions, one on each of the n issues. Formally, the set Ω of (feasible) outcomes may be defined as the Cartesian product of $\omega_1, \ldots, \omega_n$:

DEFINITION.
$$\Omega = \underset{i=1}{\overset{n}{\times}} \omega_i$$
.

Let P be the collective preference or dominance relation among feasible outcomes. It is a binary relation on Ω . Beyond that, what is it?

We might interpret P as the relation of simple majority preference of some legislature, committee or other voting body. Formally, this means there are, say, v voters (legislators, committee members), with binary preference relations ρ_1, \ldots, ρ_v on Ω , such that:

(1) xPy if there are more than $\frac{\mathbf{v}}{2}$ members i of $[1,2,\ldots,\mathbf{v}]$ for which $\mathbf{x}\rho_i\mathbf{y}$.

This is an appropriate way to characterize P when issues are decided by simple majority rule.

Much more generally, we might interpret P as the relation of unanimous preference by any one of certain winning coalitions of voters. This means there is a set W such that:

(2) W is a non-empty family of non-empty subsets of [1,2,...,v], and xPy if $x\rho_i y$ holds for some $c \in W$ and for every $i \in c$.

This is an appropriate way to characterize P when every issue is decided by the following winning coalition rule: Choose the position voted for by all the members of some winning coalition, if such a position exists; otherwise, choose randomly.

In all examples below, (1) holds, and issues are decided by simple majority rule. But (1) is not one of the assumptions of the model, which is so general that it does not even include (2).

In order to *interpret* the model and its consequences in political terms, however, it is necessary to make some assumption about the interpretation of P. To this end I will assume that P satisfies (2); I will also assume that issues are decided (in effect) according to the winning coalition rule.

Paradox and Decision Rules," cited in acknowledgment note above.

The model comprises just two assumptions. The first says P is separable or noncomplementary with respect to the issues $\omega_1, \ldots, \omega_n$:

ASSUMPTION I. Suppose $1 \le i \le n$, $z, w \in \omega_i$, and $x_i, y_i \in \omega_i$, j = 1, 2, ..., n. Then if

$$(x_1, \dots, x_{i-1}, z, x_{i+1}, \dots, x_n) P$$

 $(x_1, \dots, x_{i-1}, w, x_{i+1}, \dots, x_n),$
 $(y_1, \dots, y_{i-1}, z, y_{i+1}, \dots, y_n) P$
 $(y_1, \dots, y_{i-1}, w, y_{i+1}, \dots, y_n).$

This means, roughly, that if one position on a given issue (say ω_i) is collectively preferred to another, that is independent of the positions taken on the other issues $(\omega_1,\ldots,\omega_{i-1},\omega_{i+1},\ldots,\omega_n)$. So, besides being voted on separately, ω_1,\ldots,ω_n are really separate issues in the sense that they can be decided independently of one another.

Assumption I obviously holds if voter preferences (ρ_1, \ldots, ρ_v) are similarly separable and (2) holds.

To be sure, both voter preferences and collective preferences can easily fail to be separable with respect to common issues, witness:

EXAMPLE 1. A committee must vote on two motions:

p - Spend \$1,000 on a party. t - Spend \$1,000 on a trip.

So there are two issues, passage vs. defeat of p and passage vs. defeat of t, hence four possible outcomes:

pt - p and t both pass.

 $p\bar{t} - p$ passes and t is defeated.

 \overline{pt} - p is defeated and t passes.

 $\overline{pt} - p$ and t are both defeated.

One committee member prefers a \$1,000 party to a \$1,000 trip, prefers either activity to neither, and prefers having neither activity to spending more than \$1,000 (the whole budget) altogether. So she prefers $p\bar{t}$ to $\bar{p}t$, both of these outcomes to $\bar{p}t$, and all three of these to pt. Since she prefers $p\bar{t}$ to $\bar{p}t$ but does not prefer pt to $\bar{p}t$, her preferences are not separable. And if a majority share her preferences, then P, interpreted as majority preference, is not separable either.

With $\omega_1, \ldots, \omega_n$ interpreted as all the issues voted on, Assumption I is so strong as to be inapplicable to virtually any real-world committee or legislature. But this assumption is quite weak and realistic if $\omega_1, \ldots, \omega_n$ are interpreted simply as some n issues or other (for some $n \ge 2$). Although P is not likely to be

separable with respect to all the issues voted on, it could easily be separable with respect to some two or more of them.

P is a relation among outcomes, not among positions on any issue. But there is a natural way to define, for each issue ω_i , a separate collective preference relation Pi among the alternative positions constituting ω_i :

DEFINITION. For i=1, 2, ..., n, xP_iy iff x,y $\epsilon \omega_i$, and there are z_1, \ldots, z_n such that

$$(z_1, \ldots, z_{i-1}, x, z_{i+1}, \ldots, z_n) P$$

 $(z_1, \ldots, z_{i-1}, y, z_{i+1}, \ldots, z_n).$

This equates the following two conditions:

- (i)
- (ii) $(z_1, \ldots, z_{i-1}, x, z_{i+1}, \ldots, z_n) P$ $(z_1,\ldots,z_{i-1},y,z_{i+1},\ldots,z_n)$ for some $z_1, \ldots, z_{i-1}, z_{i+1}, \ldots, z_n.$

It would have been no less natural to equate (i)

(iii)
$$(z_1, \ldots, z_{i-1}, x, z_{i+1}, \ldots, z_n)$$
 P
 $(z_1, \ldots, z_{i-1}, y, z_{i+1}, \ldots, z_n)$ for all
 $z_1, \ldots, z_{i-1}, z_{i+1}, \ldots, z_n$ belonging to
 $\omega_1, \ldots, \omega_{i-1}, \omega_{i+1}, \ldots, \omega_n$, respectively.

Happily, according to Assumption I, (ii) and (iii) are equivalent.

I will sometimes say that one outcome (member of Ω) dominates another when the first bears P to the second, and that one member of a given issue, say ω_i , dominates another member of ω_i when the first member bears pi to the second. Outcomes and positions on issues are stable if undominated.

Let m_1, \ldots, m_n be the positions that would be taken on the n issues in the absence of any

vote trading involving $\omega_1, \ldots, \omega_n$. The second assumption of the model says each mi directly or indirectly dominates every other position on the ith issue:

ASSUMPTION II. For i=1,2,...,n and for each $x\neq m_i$ in ω_i , there is a finite P_i -chain from m_i to x_i; that is, for some k, there are x_1, \ldots, x_k such that $m_i P_i x_1 P_i x_2 P_i \ldots x_{k-1}$ $P_i x_k$ and $x=x_k$.

This condition is automatically met when, as is normally the case in real-world committees and legislatures, there are just two positions on each issue - passage and defeat of some motion - and no ties.

I have not assumed that any or every m; is stable. Consistently with the model, some or every m; might be dominated. Since every other P has the form of a linear ordering:

position on every issue is dominated according to Assumption II, if some m; is dominated as well, then there is no stable position on the ith

Stable Outcomes in the Absence of Vote Trading

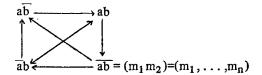
Suppose m_1, \dots, m_n are all individually stable. Then, assuming the model, must Ω contain a stable outcome? If not, can Ω contain a stable outcome? If so, can the stable outcomes, when they exist, be characterized in an illuminating way? And can we predict that one of them will be chosen, at least when there is no vote trading?

First, Ω need not contain a stable outcome, even when m_1, \ldots, m_n are all individually stable, witness:

EXAMPLE 2. Three voters, Messrs. 1, 2 and 3, must vote on two motions, a and b, so that there are two issues, passage vs. defeat of a and passage vs. defeat of b, hence four possible outcomes: ab, ab, ab and ab. Messrs. 1, 2 and 3 rank these outcomes in order of preference as follows:

<u>Mr. 1</u>	<u>Mr. 2</u>	Mr. 3		
аБ	āb	ab		
ab	ab	āb		
ab	ab	а Б		
āb	$a\overline{b}$	ab		

As in subsequent examples, voter preferences are linear and separable, and P is the relation of simple majority preference. P has this cyclic form:



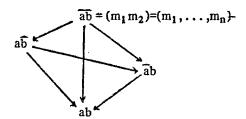
Evidently, there is no stable outcome, although each mi is stable.

Second, although Ω need not contain a stable outcome, it can contain one, witness:

EXAMPLE 3. Five voters rank the same four outcomes in order of preference as follows:

Mr. 1	Mr. 2	Mr. 3	Mr. 4	Mr. 5
$a\overline{b}$	$a\overline{b}$	āb	āb	ab
ab	ab	ab	ab	ab
ab	ab	ab	ab	ab
āb	āb	а Б	а Б	ab

Interpreted again as simple majority preference,



Here, ab is the one and only stable outcome.

Notice that the unique stable outcome in this example is also the outcome, (m_1, \ldots, m_n) , chosen in the absence of vote trading. This is no accident. In general, when there exists a stable outcome, it must be (m_1, \ldots, m_n) , because every other outcome is perforce dominated. Indeed, every other outcome is directly or indirectly dominated by (m_1, \ldots, m_n) :

CONSEQUENCE 1. Suppose $x \in \Omega$ and $x \neq (m_1, \ldots, m_n)$.

Then yPx for some y. Indeed, there is a finite P-chain from (m_1, \ldots, m_n) to x; that is, for some k, there are x_1, \ldots, x_k such that (m_1, \ldots, m_n) $Px_1Px_2 \ldots x_{k-1}Px_k$ and $x_k=x$.

Proof. By induction on the number of dimensions in which x differs from (m_1, \ldots, m_n) . Say x differs from (m_1, \ldots, m_n) in the i^{th} dimension (at least). Let x_i be the i^{th} component of x. Then $x_i \neq m_i$. For any $y \in \omega_i$, let x(y) be the result of replacing x_i in x by y. So, unlike x, $x(m_i)$ has the same i^{th} component as (m_1, \ldots, m_n) . By inductive hypothesis, there is a finite P-chain from (m_1, \ldots, m_n) to $x(m_i)$. So it suffices to show that there is a finite P-chain from $x(m_i)$ to $x=x(x_i)$. But by Assumption II, there is a finite P_i-chain from $x(m_i)$ to $x=x(x_i)$ and $x=x(x_i)$ but that $x=x(x_i)$ for some $x=x(x_i)$ for $x=x(x_i)$ such that $x=x(x_i)$ for some $x=x(x_i)$ for $x=x(x_i)$ such that $x=x(x_i)$ for some $x=x(x_i)$ for $x=x(x_i)$ finite $x=x(x_i)$ for $x=x(x_i)$

$$y_1 P_i y_2 P_i \dots y_{k-1} P_i y_k.$$
 (*)

I will show that $x(y_1), x(y_2), \ldots, x(y_k)$ constitute a P-chain from $x(m_i=x(y_1))$ to $x=x(x_i)=x(y_k)$. What must be established, then, is that $x(y_j)Px(y_{j+1}), j=1,2,\ldots,k-1$. But by (*) and the definition of P_i , there are z_1,\ldots,z_n such that:

$$(z_1, \ldots, z_{i-1}, y_j z_{i+1}, \ldots, z_n) P$$

 $(z_1, \ldots, z_{i-1}, y_{i+1}, z_{i+1}, \ldots, z_n).$

Hence, by Assumption I, $x(y_i)Px(y_{i+1})$, q.e.d.

⁴More or less this result was proved, in a less general form, by Kadane, "On Division of the Question."

So the outcome chosen in the absence of vote trading has got to be stable if any is. This is an interesting result about issue-separated voting under the model, inasmuch as most election rules (plurality, run-off, Borda, etc.) do not guarantee the choice of a stable outcome even when one exists (and voters vote sincerely). In fact, the very oldest problem of social choice theory — the principal problem addressed by Borda, Condorcet, Dodgson and Nanson — is precisely to find a voting rule that picks a stable outcome when one exists (and voters vote sincerely).⁵

Stable Outcomes and Vote Trading

Even when (m_1, \ldots, m_n) — the outcome chosen in the absence of vote trading — is uniquely stable, vote trading can lead to the choice of a different outcome.

In Example 3, $\overline{ab} = (m_1, m_2)$ would be chosen in the absence of vote trading. But suppose Messrs. 1 and 3 and they alone trade votes, Mr. 1 agreeing to vote for b in return for Mr. 3's agreement to vote for a. Then the chosen outcome is ab.

That seems anomalous. Far from being stable, ab is dominated by every other outcome. Yet Messrs. 1 and 3 have evidently behaved rationally in trading votes, bringing about what is for them a better outcome (ab) than would be chosen (ab) in the absence of vote trading.

What has happened is that an organized minority (Messrs. 1 and 3) has frustrated the wishes of the disorganized majority (Messrs. 2, 4 and 5). Had Messrs. 2 and 4 also traded votes, Mr. 2 agreeing to vote against a in return for Mr. 4's agreement to vote against b, they would have blocked the effect of the first trade, insuring the choice of $\overline{ab}=(m_1,\ldots,m_n)$, which both of them prefer to ab - as does Mr. 5. If cooperation costs are such as to permit Messrs. 1 and 3 to trade votes while preventing Messrs. 2 and 4 from trading votes, then the will of the majority is frustrated in a striking way. It is perhaps an awareness of this type of situation that explains many of the complaints we hear about democratic governments serving special interests rather than the public interest.

The example can be generalized: If (m_1, \ldots, m_n) - the outcome chosen in the absence of vote trading - is stable, and if cooperation costs do not keep winning coali-

⁵On the early history of social choice theory, see Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1963), Part II.

tions from cooperating, then vote trading cannot block the choice of (m_1, \ldots, m_n) .

Proof: Suppose (m_1, \ldots, m_n) is stable and yet there is some chance of bringing about an outcome x, other than (m_1, \ldots, m_n) , by vote trading. By Consequence 1, however, there is a finite P-chain from (m_1, \ldots, m_n) to x. For the sake of illustration, say:

$$(m_1, \ldots, m_n)$$
PyPzPx.

Then there are winning coalitions Cm, Cy and Cz whose members prefer (m_1, \ldots, m_n) to y, y to z and z to x, respectively.

Since there is a chance of choosing x, the members of Cz have some incentive to agree to vote for the positions constituting z, thereby bringing about the choice of z instead of x. But then, there is a chance of choosing z, and so the members of Cy have an incentive to agree to vote for the positions constituting z, thereby bringing about the choice of z instead of x. But then, there is a chance of choosing z, and so the members of Cy have an incentive to agree to vote for the positions constituting y, thereby bringing about the choice of y instead of z. And this gives the members of Cm as an incentive to agree to vote for m_1, \ldots, m_n , thereby bringing about the choice of (m_1, \ldots, m_n) instead of y. Since (m_1, \ldots, m_n) is stable, no winning coalition has any incentive to block the choice of (m_1, \ldots, m_n) .

That our ability to predict the choice of a stable (m_1, \ldots, m_n) depends upon cooperation costs being small is a significant limitation. Cooperation costs can be great. They include:

- Information costs. These are the costs of making potential cooperators aware of the opportunity to benefit from cooperation.
- Transaction costs. These are the costs of working out an agreement.
- Fidelity costs. These are the costs of cooperating with partners who are less than fully trustworthy.
- Opprobrium costs. These are the psychic costs of being involved in an arrangement one finds (somewhat) distasteful.

Vote Trading and the Paradox of Voting

Peter Bernholz⁶ and Joe Oppenheimer⁷ have argued that vote trading entails the "paradox of voting," assuming a model like mine but less

⁶Bernholz, "Log-Rolling, Arrow-Paradox and Cyclical Majorities," and "Log-Rolling, Arrow-Paradox and Decision Rules."

⁷Oppenheimer, "Relating Coalitions of Minorities to the Voter's Paradox."

general. This point has recently been aborated in the exchange among Koehler,⁸ Bernholz⁹ and Oppenheimer.¹⁰

What is the "paradox of voting"? The label is sometimes applied to any nontransitivity in the collective preference relation (our P) or the corresponding relation of collective indifference — the relation between outcomes not related by P in either direction. Bernholz and Oppenheimer apply the label just to collective preference cycles (P-cycles). This makes their thesis much more interesting. The thesis becomes more interesting still if the label "paradox of voting" is applied just to situations in which none of the feasible outcomes (no member of Ω) is stable. ¹¹

The following seems to be the most interesting interpretation (though not necessarily the most favorable) of the Bernholz-Oppenheimer thesis:

Suppose Assumptions I and II hold, P satisfies (2), and issues are decided according to the winning coalition rule. Then it is advan-

⁸David H. Koehler, "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence," American Political Science Review, 69 (September, 1975), 954–960; and "Vote Trading and the Voting Paradox: Reply," American Political Science Review, 69 (September, 1975).

⁹Peter Bernholz, "Logrolling and the Paradox of Voting: Are They Really Logically Equivalent?" American Political Science Review, 69 (September, 1975), 961–962.

10 Joe A. Oppenheimer, "Some Political Implications of 'Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence," "American Political Science Review, 69 (September, 1975), 963-966.

The thesis stated in this paper and in Bernholz, "Logrolling, Arrow-Paradox and Decision Rules," is more general than that of Oppenheimer, "Relating Coalitions of Minorities to the Voter's Paradox," and Bernholz, "Logrolling, Arrow Paradox and Cyclical Majorities." Koehler in "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence," restates the argument for the special three-voter example of William H. Riker and Steven J. Brams, "The Paradox of Vote Trading," American Political Science Review, 67 (December, 1973), 1235–1247, then makes a questionable attempt to generalize it.

Bernholz, "Logrolling and the Paradox of Voting: Are They Really Logically Equivalent?" points out that Koehler's general formulation is too general and that Koehler falsely assumes there are no P-cycles if there are no three-element P-cycles. Koehler, "Vote-Trading and the Voting Paradox: Reply" responds by citing a trivial fact of elementary logic: there are no P-intransitivities if there are no three-element P-intransitivities. True. But not every P-intransitivity is a P-cycle.

11If nothing in Ω is stable, there must exist a P-cycle. But not conversely. A P-cycle can coexist with a stable outcome. Of course, if there is a P-cycle, then, even if Ω itself has a stable member, some *subset* of Ω must lack a stable member.

tageous for some voters to trade votes with one another on issues drawn from among $\omega_1, \ldots, \omega_n$, only if Ω contains no stable outcome.

This is simply false. It remains false if we replace the clause " Ω contains no stable outcome" by "P is cyclic" or even "there is some nontransitivity in P or in the collective indifference relation." And the thesis remains false even if we further weaken it by restricting it to particularly simple, realistic cases, in which: each issue comprises just two positions; each voter has a linear, separable preference ordering; P is the relation of simple majority preference; each issue is decided by simple majority rule; and there are no ties on any issue and no collective indifferences between outcomes.

In Example 3, Messrs. 1 and 3 alone might trade votes (to their advantage but to the disadvantage of Messrs. 2 and 4), or Messrs. 2 and 4 might trade votes to block the effect of the first trade, and yet $\overline{ab} = (m_1, \ldots, m_n)$ is stable. Indeed, P is a linear ordering, so that there is no collective nontransitivity of any sort.

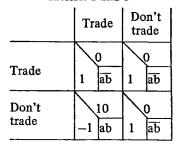
To be sure, if Messrs. 1 and 3 alone trade votes, this can only be because cooperation costs have prevented the remaining majority — or at least Messrs. 2 and 4 — from cooperating. But we cannot save the thesis by restricting it further to cases where cooperation costs are zero.

In Example 3, suppose voters are fully informed of all relevant features of the situation and fully rational in the sense of maximizing expected advantage, and suppose cooperation costs are zero. Then Messrs. 2 and 4 (at least) must trade votes, although there are no collective nontransitivities of any sort.

To see why, suppose, on the contrary, that Messrs. 2 and 4 did not trade votes. Then they must have either decided definitely not to trade votes or else adopted a mixed strategy involving some likelihood of not trading. Either way, it is then to the expected advantage of Messrs. 1 and 3 to trade votes themselves, inasmuch as cooperation costs are zero. Therefore, since they are fully informed and rational, Messrs. 1 and 3 will certainly trade votes. But then, since cooperation costs are zero, it is to the advantage of Messrs. 2 and 4 to trade votes after all. And so, since they are fully informed and rational, Messrs. 2 and 4 will certainly trade

The situation is depicted in the following payoff matrix, in which vote trading is a dominant strategy for all concerned:

Messrs. 1 and 3



Messrs. 2 and 4

Notice that the vote trade involving Messrs. 1 and 3 is opposed by a majority, while the trading involving Messrs. 2 and 4 is supported by a majority in order not to overturn but to sustain the choice of $\overline{ab} = (m_1, \ldots, m_n)$. The Bernholz-Oppenheimer thesis is true when restricted to instances of vote trading supported by majorities — or whatever count as winning coalitions — in order to overturn (m_1, \ldots, m_n) :

Suppose Assumptions I and II hold, P satisfies (2), and issues are decided according to the winning coalition rule. Then Ω contains no stable outcome if there is a potential vote trade involving issues drawn from among $\omega_1, \ldots, \omega_n$ such that the members of some winning coalition all prefer that trade (i.e., the outcome issuing therefrom) to (m_1, \ldots, m_n) . 12

As it happens, the converse also is true. Once correctly stated, the Bernholz-Oppenheimer thesis holds in both directions:

Suppose Assumptions I and II hold, P satisfies (2), and issues are decided according to the winning coalition rule. Then Ω contains no stable outcome if, and only if, there is a potential vote trade involving issues drawn from among $\omega_1, \ldots, \omega_n$ and preferred to (m_1, \ldots, m_n) by all the members of some winning coalition.

Proof: Suppose the members of some winning coalition all prefer some vote trade, involving issues drawn from among $\omega_1, \ldots, \omega_n$, to (m_1, \ldots, m_n) . Let x be the outcome resulting from that trade. Then $xP(m_1, \ldots, m_n)$. But by Consequence I, every outcome other than (m_1, \ldots, m_n) is dominated. Hence, every outcome is dominated.

Conversely, suppose no outcome is stable. Then $xP(m_1, \ldots, m_n)$ for some x. Therefore, the members of some winning coalition c all prefer x to (m_1, \ldots, m_n) . By agreeing to vote for the positions constituting x, the members of

12See Thomas Schwartz, "On the Possibility of Rational Policy Evaluation," *Theory and Decision*, 1 (January, 1970), 1-25; and "Rationality and the Myth of the Maximum," *Nous*, 7 (May, 1972), 97-117.

c can insure the choice of x rather than (m_1,\ldots,m_n) . But (m_1,\ldots,m_n) is the outcome chosen in the absence of any vote trading involving $\omega_1, \ldots, \omega_n$. Consequently, by agreeing to support the positions constituting x, the members of c (some of them, anyway) must have engaged in a vote trade involving issues drawn from among $\omega_1, \ldots, \omega_n$, and so must prefer that vote trade to (m_1, \ldots, m_n) .

As you will see in the final section of this paper, Assumptions I and II are essential to the revised thesis. We cannot drop either assumption without making the thesis false. Still, we can weaken Assumption II, as follows:

ASSUMPTION II*. For $i=1,2,\ldots,n$ and for each $x \neq m_i$ in ω_i , $y P_i x$ for some $y \in \omega_i$.

With Assumption II replaced by II*, we can no longer deduce Consequence 1. But we can deduce:

CONSEQUENCE 1*. Suppose $x \in \Omega$ and $x\neq (m_1,\ldots,m_n)$. Then yPx for some y.

And this is all I actually used to demonstrate the thesis.

One should be cautious about drawing from this thesis any lessons concerning the nature of vote trading, the nature of stability in collective choice, or the likelihood of there being no stable outcome (of the "paradox of voting"), for three reasons:

First, the thesis is conditional upon Assumptions I and II*.

Second, the thesis concerns only a special type — even if an especially prevalent type — of vote trading.

Third, even among vote trades of this type, only those that involve issues drawn from among $\omega_1, \ldots, \omega_n$ are said to imply the nonexistence of a stable outcome in Ω . The thesis says nothing about vote trades involving issues other than $\omega_1, \ldots, \omega_n$.

True, we can interpret $\omega_1, \ldots, \omega_n$ as all the issues voted on, in which case no vote trade can involve other issues. But that would make Assumption I (separability) so strong as to be virtually inapplicable in the real world.

On the other hand, Assumptions I and II (or II*) are plausible if $\omega_1, \ldots, \omega_n$ are interpreted as special interest or local interest issues issues concerning river and harbor improvements, for example. And it is realistic to suppose there is some vote trading, even of the special type lately remarked, involving such issues.

When No Outcome Is Stable

stable outcome, it must be (m_1, \ldots, m_n) , and it must be chosen unless cooperation costs keep the members of some winning coalition from cooperating. But, of course, there need be no stable outcome. Even (m_1, \ldots, m_n) might be dominated. What then happens? This question is partly answered by the following consequence of the model:

CONSEQUENCE 2. If $xP(m_1, ..., m_n)$ and $x\neq (m_1,\ldots,m_n)$, there is a unique undominated P-cycle γ - i.e., a unique P-cycle γ such that nothing in Ω - γ bears P to anything in γ and $(m_1, \ldots, m_n) \in \gamma$.

Proof. By Consequence 1, there is a finite P-chain from (m_1, \ldots, m_n) to x, i.e., for some k, there are y_1, \ldots, y_k such that (m_1, \ldots, m_n) $Py_1Py_2P \dots y_{k-1}Py_k$ and $y_k=x$. Hence, since $y_k^P(m_1, \dots, m_n)$, the set $[(m_1, \dots, m_n),$ y_1, \ldots, y_k] is a P-cycle. So there is at least one P-cycle containing (m_1, \ldots, m_n) . Since all such P-cycles overlap in at least one element, and since the union of overlapping P-cycles must itself by a P-cycle, the union - call it U - of P-cycles containing (m_1, \ldots, m_n) is also a P-cycle containing (m_1, \ldots, m_n) . I will show that U is a unique undominated P-cycle. Since U is a P-cycle, it suffices to show that U is undominated and that no other P-cycle is undominated.

To prove U undominated, suppose yPz for some $z \in U$; I will show that $y \in U$. If $y=(m_1, \ldots, m_n)$, $y \in U$ by construction. Otherwise, by Consequence 1, there is a P-chain from (m_1, \ldots, m_n) to y. But since yPz for some $z \in$ \dot{U} , and \dot{U} is a P-cycle containing (m_1, \ldots, m_n) , there is another P-chain from y back to (m_1, \ldots, m_n) . Therefore, there is a P-cycle containing both y and (m_1, \ldots, m_n) , and thus, by construction, $y \in U$.

To prove that no other P-cycle is undominated, suppose α is an undominated P-cycle; I will show that $\alpha=U$. By Consequence 1, there is a P-chain from (m_1,\ldots,m_n) to some member of α . Thus, since α is undominated, $(m_1, \ldots, m_n) \in \alpha$, so that α is a P-cycle containing (m_1, \ldots, m_n) , whence, by construction, $\alpha \subseteq U$, and it suffices to show that $U \subseteq \alpha$. Suppose not. Then since $\alpha \subseteq U$ and U is a P-cycle, something in $U-\alpha$ must bear P to something in α , contrary to the undominated character of α . Hence, $U \subseteq \alpha$, q.e.d.

Suppose cooperation costs do not prevent winning coalitions from cooperating, and suppose (m_1, \ldots, m_n) is dominated. What can we predict about the chosen outcome? We can certainly predict that the chosen outcome will Given Assumptions I and II, if there is a belong to the unique undominated P-cycle -

call it γ – of whose existence we are assured by Consequence 2.

For suppose, on the contrary, there were some chance of choosing an outcome x for α to γ . By Consequence 1, there is a finite P-chain from (m_1, \ldots, m_n) to x. For the sake of illustration, say:

$$(m_1, \ldots, m_n)$$
PyPx.

Then there are winning coalitions Cm and Cy whose members prefer (m_1, \ldots, m_n) to y and y to x, respectively. Since there is some chance of choosing x, and the members of Cy can afford to cooperate to get a better cutcome, the members of Cy have an incentive to agree to vote so as to bring about the choice of y rather than x. But then, there is a chance of choosing y, and so the members of Cm have an incentive to support (m_1, \ldots, m_n) .

Because (m_1, \ldots, m_n) is dominated, the chain does not stop here. There is a winring coalition whose members have an incentive to block the choice of (m_1, \ldots, m_n) in favor, say, of z; another winning coalition whose members have an incentive to block the choice of z in turn; and so on.

Because γ is a cycle, the chain never ends, and it is hard to make any certain prediction about the final choice among members of γ . But because γ is undominated, the chain never leads back outside γ , and so we can certainly predict that the chosen outcome will belong to γ .

This suggests defining the choice set or solution set from Ω as follows:

DEFINITION.
$$\Gamma = \begin{cases} [(m_1, \dots, m_n)], & \text{if nothing bears P to} \\ (m_1, \dots, m_n); & \text{the unique undominated P-cycle,} \\ & \text{otherwise.} \end{cases}$$

Example 3 casts doubt on the possibility of saying anything very interesting about the final choice when cooperation costs prevent some winning coalitions from cooperating. Otherwise, the final choice must certainly belong to Γ .

There are various ways this prediction might be refined, depending on circumstances. I examine some refinements later. But I know of no decisive reason for generalizing any of these refinements to all relevant situations, i.e., for postulating any of them as part of a general model of collective choice separated by issues.

Comparison with Known Solution Concepts

According to one well-known proposal, the choice set or solution set from Ω is the set $C(\Omega)$ defined by this condition:

BINARY CHOICE PROPERTY (BICH). $C(\Omega) \equiv [x \in \Omega]$, $y \neq x$ for $n \in y \in \Omega$.

This equates $C(\Omega)$ with the set of stable outcomes — with the *cor*, in game theoretic terms. The trouble is, there may be no stable outcome, as Example 2 illustrates. Even (m_1, \ldots, m_n) might be dominated.

To get round this difficulty, I have proposed elsewhere 12 that we 'generalize' BICH as follows:

GENERAL OPTIMAL CHOICE AXIOM (GOCHA). $C(\Omega) \equiv$ the union of minimum undominated subsets of Ω ,

where:

an undominated subset of α is any nonempty subset β of α for which nothing in α $-\beta$ bears P to anything in β

and:

a minimum undominated subset of α is one that is not a proper superset of another.

Like BICH, GOCHA defines $C(\Omega)$ in terms of P. Like BICH, GOCHA equates $C(\Omega)$ with the set of stable outcomes when P is acyclic. Unlike BICH, GOCHA guarantees the *non-emptiness* of $C(\Omega)$, independently of any properties of P—hence even if no outcome is stable.

It can be shown that $C(\Omega)$ must satisfy GOCHA if it is to represent any of certain important types of choice process. For example, assuming certain rather mild conditions, it is known that $C(\Omega)$ must satisfy GOCHA if $C(\Omega)$ comprises just those elements of Ω that can be chosen from Ω by any of a wide range of common (and not so common) serial (sequential) choice processes.¹³

The significance of GDCHA for present purposes is this: GOCHA implies that $C(\Omega)=\Gamma$. In other words, the suggested choice set Γ , defined earlier, is also the set defined by GOCHA. This follows from the fact that Γ is the one and only minimum indominated subset of Ω , which is trivial to prove.

13 Thomas Schwartz, "Serial Collective Choice," paper presented at the annual meeting of the Public Choice Society, New Haven, Ap:il 1974.

the fraction

Is Γ Too Inclusive?

Some would reject Γ as an appropriate choice set on the ground that it is *too inclusive* when (m_1, \ldots, m_n) is dominated. Take Example 2. There, (m_1, \ldots, m_n) is dominated, and P is the whole set Ω of feasible outcomes.

But the mere fact that Γ can sometimes encompass all of Ω is not a decisive reason for rejecting Γ as an appropriate choice set. It may just happen, in some cases, that every feasible outcome has some chance of being chosen, even apart from cooperation costs.

Nor is it necessary that Γ exhaust Ω . If (m_1, \ldots, m_n) is stable, Γ is the unit set $[(m_1, \ldots, m_n)]$. And even if there is no stable outcome, Γ still need not comprise all of Ω , witness:

EXAMPLE 4. Three voters have the following preferences among eight outcomes:

Mr. 1	Mr. 2	<u>Mr. 3</u>
а Б с	abc	abc
abc	abc	abc
abc	abc	$abc=(m_1,\ldots,m_n)$
abc	abc	abc

Here, $\Gamma \neq [(m_1, \ldots, m_n)]$ and $\Gamma \neq \Omega$. Rather, $\Gamma = [\overline{abc}, \overline{abc}, abc, abc]$.

What will doubtless distress many theorists is that Γ can contain *Pareto inefficient* outcomes — outcomes to which other outcomes are unanimously preferred — as in this example:

EXAMPLE 5. Three voters have the following preferences among eight outcomes:

<u>Mr. 1</u>	Mr. 2	Mr. 3
abc	abc	\overline{abc} -(m ₁ ,,m _n)
abc	abc	abc
abc	аБс	abc
abc	abc	ab c

Here, Ω is one big P-cycle. So $\Gamma = \Omega$. But fully four of the eight outcomes in $\Gamma = \Omega$ are Pareto inefficient: abc, abc, abc and abc. In fact, one of these outcomes, abc, is Pareto dominated by (i.e., unanimously dispreferred to) none other than $abc = (m_1, \ldots, m_n)$.

It is not obvious to me, however, that we should automatically predict the rejection of Pareto inefficient outcomes, hence not obvious that they should be banished from our solution set. They are, in a sense, unanimously opposed. But not in the sense that voters can unanimously agree on an alternative. Nor even in the sense that voters can unanimously agree to reject them. It would be unwise for Mr. 1 to agree to reject abc, for example, unless his preference for abc over abc were comparatively very intense or abc were especially likely to be chosen after the rejection. And if unanimous opposition is enough to reject an outcome, why isn't opposition by any winning coalition enough? After all, a winning coalition has the power to enforce any choice. When Γ is cyclic, however, to reject every member of Ω opposed by some winning coalition is to reject every member of Ω .

There are rationales for predicting the rejection of Pareto inefficient outcomes. I discuss some in the next section. None, I think, is applicable in all legislative situations.

Tullock¹⁴ and Riker and Brams¹⁵ have argued, more positively, that vote trading can indeed lead to an outcome Pareto dominated by (m_1, \ldots, m_n) , making every voter worse off than he would be in the absence of vote trading. They offer examples in which, although there are many issues, voters trade votes on relatively few issues at a time, thereby allowing themselves to incur external costs from other trades by other voters on other issues. Because these external costs outweigh the benefits of trading for each voter, the outcome achieved is Pareto dominated by (m_1, \ldots, m_n) .¹⁶

Besides cases in which an outcome in Γ is Pareto dominated by (m_1, \ldots, m_n) , there also are cases in which (m_1, \ldots, m_n) itself is Pareto dominated by another outcome in Γ , achievable by vote trading. So vote trading can make every

¹⁴Gordon Tullock, "Problems of Majority Voting," Journal of Political Economy, 67 (December, 1959), 571–579, reprinted in Freedom and Authority: An Introduction to Social and Political Philosophy, ed. Thomas Schwartz (Encino, Ca.: Dickenson, 1973), pp. 295–303.

¹⁵ Riker and Brams, "The Paradox of Vote Trading."

¹⁶Tullock's argument is criticized by Anthony Downs, "In Defense of Majority Voting," Journal of Political Economy, 69 (April, 1961), 192–199, reprinted in Freedom and Authority, ed. Schwartz, pp. 304–313, and defended against Downs's criticism by Gordon Tullock, "Reply to a Traditionalist," Journal of Political Economy, 69 (April, 1961), 200–203.

voter better off than he would be in the absence of vote trading.

EXAMPLE 6. Our old friends, Messrs. 1, 2 and 3, must vote on three measures — a, b and c — to which they assign the following dollar values:

	Mr. 1	Mr. 2	Mr. 3
a	500	500	-1000
b	-1000	500	400
c	400	-1000	500

Observe that each measure benefits two voters at a considerable cost to the third. In each case, the net cost to the nonbeneficiary, measured in dollars, is greater than the combined net benefit to the two beneficiaries — owing, perhaps, to transfer costs. Each voter ranks the eight feasible outcomes according to their total dollar values for him:

Mr. 1	Mr. 2	Mr. 3
abc	ab c	abc
abc	abc	$abc=(m_1,\ldots,m_n)$
abc	abc	abc
abc	abc	abc
abc	abc	abc

Here, $\Gamma = \Omega$, and (m_1, \ldots, m_n) =abc is Pareto dominated by abc. In fact, abc is the *anly* Pareto-inefficient outcome. But if Messrs. 1, 2 and 3 trade votes, each agreeing to vote against all three measures, they can ensure the choice of abc, making every voter better off than he would be in the absence of vote trading. (Of course, other outcomes also can be achieved by vote trading.)

Paring Down \(\Gamma\)

Since Γ can be distressingly large, one might try to pare it down by deleting outcomes fulfilling certain exclusion conditions, such as Pareto inefficiency, when Γ is a P-cycle.

An attractive proposal is to delete those outcomes opposed by the largest winning coalitions, or those outcomes dominated by the largest number of other outcomes, or those outcomes with the largest combined voter-opposition. That is, one might propose paring down Γ by deleting those outcomes x that fulfill one of the following three conditions (provided, of course, that this does not exclude all members of Γ):

- (XC₁) There exists a c ϵ W such that $y\rho_i x$ for some $y \epsilon \Gamma$ and every $i \epsilon$ c, and such that, for all $c' \epsilon W$, if $z\rho_i w$ for some z, $w \epsilon \Gamma$ and all $i \epsilon c'$, then c is no smaller than c'.
- (XC₂) For every $y \in \Gamma$, $[z \mid zPx]$ is no smaller than $[z \mid zPy]$.
- (XC₃) For every $y \in \Gamma$, $[c \in W \mid \text{for some } z, z\rho_i x \text{ for every } i \in c]$ is no smaller than $[c \in W \mid \text{for some } z, z\rho_i y \text{ for every } i \in c]$.

Notice that (XC₁) and (XC₃) are automatically satisfied by *Pareto inefficient* outcomes.

Why predict the rejection of outcomes fulfilling one of these maximum opposition conditions? Maybe because the voters in question are elected legislators who wish to minimize opposition within the larger electorate by supporting minimally opposed (or maximally popular) platforms.

But even if the legislature were a fair microcosm of the larger electorate, size of opposition within the legislature would not necessarily reflect size of opposition within most legislative constituencies. Nor is it likely that all legislative behavior can be explained by the very simple goal of minimizing constituent-opposition. Besides, small winning coalitions sometimes reflect small but very well organized blocs of constituents, making outcomes they oppose unattractive to legislators concerned to keep their jobs, hence no less reasonable candidates for exclusion from the solution set. Anyway, the "voters" in our model might not be elected legislators.

My point is not that opposition-minimization (or support-maximization) models of legislative behavior are, in any sense, Bad Things. It is just that they are inapplicable in many collective choice situations characterized by Assumptions I and II.

More interesting, I think, is the proposal that we banish from Γ those outcomes x preferred to others only by winning coalitions that prefer yet other outcomes to x:

(XC₄) For every $c \in W$, if there is a $y \in \Gamma$ such that $x\rho_i y$ for all $i \in c$, then there is a $z \in \Gamma$ such that $z\rho_i x$ for all $i \in c$.

Suppose Γ is a P-cycle and an element x of Γ satisfies (XC₄). Why couldn't x be the chosen outcome? Since Γ is a P-cycle, xPy for some y ϵ Γ , and so the members of some winning coalition c all prefer x to y. Why couldn't the members of c support x to block the choice of y? One might argue that it would be irrational for the members of c to support x, for this

reason: Because x satisfies (XC₄), there is a third outcome, z, which the members of c all prefer to x, and hence to y as well. So, in agreeing to support x, the members of c would be making a less advantageous agreement than they might have made. They would be better off agreeing to z. That is why one might predict the rejection of x.

A nice feature of (XC_4) is that it is satisfied by all Pareto inefficient members of Γ , so that the deletion from Γ of outcomes satisfying (XC_4) insures the deletion of Pareto inefficient outcomes. To prove this, suppose x is a Pareto inefficient member of Γ , i.e., $x \in \Gamma$ and some $y \in \Gamma$ is unanimously preferred to x. Let c by any winning coalition whose members all prefer x to another outcome. It suffices to show that there is yet a third outcome preferred by all the members of c to x. But y is such an outcome.

Can we predict the rejection of outcomes satisfying (XC₄)? Upon what model of voter behavior might such a prediction be based?

The above argument that it would be irrational for the members of any winning coalition to support an outcome satisfying (XC₄) seems to rest on the following model of rational voter behavior: A rational voter will not accept less than the best voting agreement he can get; he will not make any agreement if a better agreement is available.

But if winning coalitions always overlap somewhat (as, for example, majorities always do), then this model precludes any rational agreement by any winning coalition to support any outcome, when (m_1, \ldots, m_n) is dominated. To see why, let x be any outcome, assume the above model of rational voter behavior, and assume that winning coalitions always overlap somewhat: I will show that no winning coalition Cx can rationally support x if (m_1, \ldots, m_n) is dominated. By Consequence 1, if (m_1, \ldots, m_n) is dominated, then every outcome is dominated, whence yPx for some y. So the members of some winning coalition Cy all prefer y to x. If the members of Cx all agree to vote for x, some of them would be accepting less than the best agreement they could get, contrary to the assumed model: Those members of Cx who also belonged to Cy could make a better agreement by agreeing with the other members of Cy to support y rather than x. Hence, there is no winning coalition whose members can rationally agree to support x.

On the other hand, one might reasonably predict the rejection of outcomes satisfying (XC₄) in circumstances in which voters have a very strong incentive to make binding agreements and stick to them — even at the cost of

foregoing a more desired outcome. For suppose the members of some winning coalition c make a binding agreement to support x. And suppose x satisfies (XC₄). Then the members of c all prefer another outcome, y, to x. While their binding agreement prevents them from breaking faith with one another, it does not prevent them from deviating from the agreement in order to support y, since they would presumably all consent to such a deviation. To deviate from an agreement is not to violate it if all parties consent. The most binding of agreements allow deviation by mutual consent. Consequently, y will probably be supported in place of x. In general, a binding agreement to support an outcome is likely to be deviated from when, and only when, the outcome satisfies (XC₄).

Circumstances might make it possible to pare down Γ in any of a number of ways. And instead of refining our prediction about the final choice by paring down Γ , we might do it by means of a nicely humped probability distribution over Γ . Either way, appropriate refinements seem to depend on circumstances. I do not think they can be incorporated in a widely applicable model of collective choice separated by issues.

The Desirability of Vote Trading; Relaxing the Model

I conclude on a normative note. Assume the model we have been examining. In the absence of vote trading, (m_1, \ldots, m_n) is the chosen outcome. If vote trading occurs, the chosen outcome might still be (m_1, \ldots, m_n) — neither an advantage nor a disadvantage. It might also be some other member of Γ — no disadvantage from my point of view, though no advantage either. But vote trading could instead lead to an outcome foreign to Γ , as in Example 3, and that is surely a serious disadvantage of vote trading.

Before concluding that vote trading is an undesirable practice, bear in mind three things:

Thing 1. Although vote-trading can lead to a Pareto inefficient outcome in Γ (which does not distress me), so can the failure to trade votes: (m_1, \ldots, m_n) , the outcome chosen in the absence of vote trading, can be Pareto dominated by another outcome, achievable through vote-trading, as in Example 6.

Thing 2. It would be extremely costly, perhaps impossible, to prevent vote trading. It is a well-entrenched practice. It is hard to detect. You can engage in it in the privacy of your bedroom. It is sometimes tacit, or implicit.

On the other hand, since the worst anomalies occur when cooperation costs prevent some winning coalitions from trading votes, the cheapest way to preclude them may simply be to minimize such costs: to make vote trading easier rather than harder, to facilitate the practice rather than penalize it.

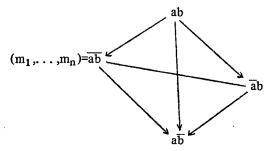
Thing 3. The argument that vote trading is never socially advantageous and sometimes socially disadvantageous rested on Assumptions I and II. But these are strong assumptions. The real utility of vote trading becomes apparent only when these assumptions are relaxed.

Suppose we drop Assumption I, allowing nonseparable collective preferences, and therewith nonseparable voter preferences. Then vote trading might actually be necessary to prevent a patently anomalous collective choice, as in this example:

EXAMPLE 7. Three voters have the following preferences among four outcomes:

Mr. 1	Mr. 2	Mr. 3
ab	ab	ab
ab	ab	ab
ab	ab	ab
а Б	āb	aŪ

Interpreted as simple majority preference, P is a linear ordering, but nonseparable — contrary to Assumption I. It looks like this:



I would contend that ab — which dominates every other outcome — should be the final choice. In the absence of vote trading, however, Messrs. 1 and 2 would doubtless vote against b, while Messrs. 1 and 3 voted against a, insuring

the choice of \overline{ab} rather than ab. But if Messrs. 2 and 3 traded votes, Mr. 2 agreeing to vote for b in return for Mr. 3's agreement to vote for a, the result would be the "socially best" ab, which Messrs. 2 and 3 both prefer to ab.

Now let us retain Assumption I but drop Assumption II. As in all previous examples, let us make the realistic assumption that there are just two positions on each issue: passage and defeat of some motion. Then the force of Assumption II — as well as II*— is that there is no tie on any issue. Allowing ties, vote trading might again be needed to prevent anomalous results, witness:

EXAMPLE 8. Two equal-size blocs of voters, I and II, have the following preferences among four outcomes:

Bloc I	Bloc II
а Б	ab
ab	ab
ab	$\overline{ab}=(m_1,\ldots,m_n)$
āb	ab

A motion is automatically defeated unless a majority favor it, hence defeated in the event of a tie. With P interpreted as simple majority preference, the only collective preference among the four outcomes is that of ab to ab. Indeed, ab is unanimously preferred to ab. So, if any outcome should not be chosen, it is ab. But ab is precisely the chosen outcome in the absence of vote trading. If, however, the two blocs traded votes, Bloc I agreeing to vote for b in return for Bloc II's agreement to vote for a, then the chosen outcome would be ab.

These examples prove, by the way, that Assumptions I and II (or II*) are essential to Consequence 1 and to the Bernholz-Oppenheimer thesis (as formulated here). In each example, there is a stable outcome other than (m_1, \ldots, m_n) (which itself is dominated), contrary to Consequence 1. In each example, a majority prefer some vote trade to (m_1, \ldots, m_n) ; and yet, contrary to the conclusion of the Bernholz-Oppenheimer thesis, Ω contains a stable outcome — indeed, P is transitive.

Demographic, Social-Psychological, and Political Factors in the Politics of Aging: A Foundation for Research in "Political Gerontology"*

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A prediction of the role that political activity among the aged will play in the future political system may be based upon a number of factors which are already known. This article explores three sets of factors as supportive of the argument that the American political system should expect increasing demand pressure articulated by the older segments of the population.

The first section will consider current and future demographic aspects of the older population: the size of future cohorts of older people, their structural relationship in the population to younger people, and their changing demographic composition. These factors provide insights into the magnitude of age-oriented issues in the future political system. The second section critically examines the disengagement theory of social and political participation which predicts that the processes of aging precipitate a withdrawal from such participation. The discussion will evaluate disengagement theory with data from studies of political attitudes and behavior. The third section considers the issue of whether or not political conservatism is in fact an inherent characteristic of older persons - a popular belief in American political mythology.

These sets of factors are particularly relevant for the near future of American politics. The "baby boom" of the 1940s represents a "bulge" in the flow of population groups in contemporary American society. Surviving members of this age cohort, consequently, will represent a "gerontology boom" in the first decades of the next century. The demographic, social-psychological, and political factors explored here bear upon the question of whether this large group of citizens is likely to represent, in the aggregate, a significant actor in the future of American political conflict.

Current and Projected Demographic Aspects of the Older Population

A primary factor in anticipating the future of an age-based politics is that of population numbers. One of the most important sociopolitical facts documented by the 1970 population census is the continuing increase in the size of the older population in the United States (typically defined as people aged 65 and older). Data on the age distribution of the national population are available since the year 1870, at which time the 65+ group was about 1.2 million, representing only 2.9 per cent of the population. This proportion has exhibited an uninterrupted increase, being 4.1 per cent in 1900, 6.8 per cent in 1940, and 9.9 per cent in 1970. By 1970 the national population had reached more than 203 million with an older population of 20 million. "Thus, a hundred years of variable growth produced by 1970 a total population five times as large, a middleaged population nine times as large, and an older population an unprecedented 17 times as large."1

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¹Herman B. Brotman, "The Older Population Revisited: First Results of the 1970 Census," Facts and Figures on Older Americans, Number 2, Administration on Aging, Washington, D.C., SRS-AOA Publication No. 182, 1971, p. 1. A detailed analysis of the last three decennial censuses further indicates the expanding relative growth rate of the older population. The percentage growth rates for the population under 65 in 1950-1960 and 1960-1970 were, respectively, 17.1 per cent and 12.5 per cent. For the population over 65 the growth rates were 34.7 per cent and 21.1 per cent. Thus, even in the context of a general decline in the growth rate for the total population, the growth rates for the 65+ group remained almost twice as large as the growth rates for the under-65 population.

Table 1 presents the growth in the percentage of the 65+ population group, as compared with the 18-64 and under-18 groups. In terms of absolute numbers, the increase between 1960 and 1970 in the older (65+) population was 3.5 million, from 16.5 million to 20 million. Yet this net increase severely understates the growth of the older population in this ten-year period. The increase in the actual number of persons who joined the ranks of the elderly is a better indicator of the "social policy" issue represented by aging, and anticipates the potential political impact of age-related issues on the political system. Between the 1960 and 1970 censuses, approximately 14.2 million persons passed their 65th birthday. When this is combined with the 16.5 million older persons previously enumerated in the 1960 census, the total number of older persons alive sometime during the 1960-1970 decade was almost 31 million. Thus the total magnitude of the social and political issues repreber of older persons various policies and programs deal with) is greater than that illustrated by net growth figures.

In addition, older people are living slightly

sented by the older population (e.g., the num-

In addition, older people are living slightly longer. In 1929 a person of age 65 could expect to live, on the average, 12.2 more years, a figure which increased to 14.4 by 1959. By 1971, a person who had reached age 65 could expect to live an additional 15.1 years.

Thus, these data suggest (a) that there are more older persons in the national population, (b) that this group represents an increasing proportion of the total population, and (c) that this increasing number of individuals is spending a longer part of its life-cycle in old age. Consequently, to the degree that one considers the evolving role of age factors in politics, recognition should be given to the various changes in the age structure of the population.

When portrayed in yet a different form, these demographic data suggest that age distributions have the potential of precipitating political conflict between the work force and those population groups who are economically dependent. Demographers employ the "dependency ratio" as a summary index describing the statistical relationship between population groups. Simply stated, the dependency ratio is the number of people in a "dependent" age category divided by the number of people in the "supporting" age category. In much demographic analysis, the 18-64 age group is defined as the work-force population, and those

³Henry S. Shryock and Jacob S. Siegel, *The Materials and Methods of Demography*, Rev. ed. (Washington, D.C.: U.S. Government Printing Office, 1975), p. 235.

2 Ibid.

Table 1. Basic U.S. Census Data for Three Age Groups, 1930-2050

	1930	1940	1950	1960	1970	2000	2020	2050
(Number given in thousands)								
Young (under 18) ^a	47,609	45,306	47,079	64,202	69,644	72,429	78,767	79,166
Work force (18-64)	68,533	77,344	91,953	98,560	113,502	163,159	188,374	191,633
Old (65+)	6,633	9.019	12,294	16,560	20,065	28,842	40,261	49,295
Total	122,775	131,669	151,326	179,323	203,212	264,430	307,402	320,094
(percentages)					, ,			
Young (under 18)	38.8	34.4	31.1	35.8	34.3	27.4	25.6	24.7
Work force (18-64)	55.8	58.7	60.8	55.0	55.9	61.7	61.3	59.9
Old (65+)	5.4	6.9	8.1	9.2	9.9	10.9	13.1	15.4

^aFor 1930 and 1940, the data are for the age groups 0-19, 20-64, and 65+.

Sources: (1930-1940) U.S. Bureau of the Census, U.S. Census of Population: 1940. Characteristics of the Population, Part I, Table 7, p. 26. (1950-1970), U.S. Bureau of the Census, Statistical Abstract of the United States: 1972, Table 37, p. 32. (2000) (based on Series E projections) Herman B. Brotman, "Projections of the Population to the Year 2000," Statistical Memo #25; Department of Health, Education, and Welfare, Administration on Aging, June 1973, p. 3. (2020, 2050) (based on Series E and Series W respectively) prepared by Dr. David M. Heer, Population Research Laboratory, University of Southern California, February 1974.

under 18 and over 65 are considered to be the dependent population. Of course, everyone in the 18-64 group is not actually working, just as everyone in the 65+ group is not in fact dependent. Nonetheless, for the purposes of estimating those future conditions for which a dependency ratio is useful, the index can be computed directly from the published census age distributions. The logic of the dependency ratio can be used to compute alternative index measures, depending on whether the numerator contains just the young, just the old, or both groups. In this discussion, the old-age dependency ratio (i.e., 65+/18-64) is used.

Table 2 presents the trend in the dependency ratio for the years 1930 through 1970, and includes the estimated dependency ratios through the year 2050. These data indicate substantial increases in the proportion of the older population as compared to the supportive working population. The dynamics of the dependency ratio depend, of course, on the relative sizes of population cohorts born at different times. The baby boom of the 1940s becomes the gerontology boom of the period 2000-2010. During the time that the proportionally large baby boom cohort is in its productive working years, especially when older people during these years constitute a relatively smaller cohort, the relationship between supports and demands may not produce conflict. But when the social and political systems encounter the gerontology boom at a time when the working population has relatively decreased, age-based allocation claims may easily become the focal point for substantial political conflict.

The dependency ratios based on the projected population estimates emphasize the potential salience of issues of social and financial allocation likely to arise in the future. The decreases in the birth rate that have occurred recently will mean that a continually diminishing work force will be responsible for supporting a continually increasing older population. As the data in Table 1 and Table 2 indicate, the dramatic change comes not in the period ending in the year 2000, but in 2020 and 2050. The population estimates for these latter years are based on zero population growth birthrates, or the "replacement" fertility rate of 2.1 babies

per woman. Given these population projections for the years 2020 and 2050, the dependency ratios based upon the projections are .213 and .257, as compared to the ratio of .177 for 1970.⁴

Dependency ratios such as these become increasingly important as one considers the demands which the older population is likely to press upon the rest of society. Medical care, transportation, housing, and basic economic well-being all represent needs which the supportive sectors of society will be called upon to finance. The example of the Social Security system serves to further illustrate the significance of these dependency ratios. As the system moves more completely from a pay-asyou-go deferred-wages program to a revolvingfund, retirement-income provision program financed out of the general revenue funds, the burden upon the active labor pool of supporting the expanding retired population will increase.5

The magnitude of the potential conflict

⁴Although all population projections are estimates subject to error, certain elements of these projections suggest their likely stability. For example, all people who will be in the 65+ age group in 2000 and 2020 are already born. Estimates of future births may be made either on the assumption of high fertility or low fertility. The United States is, and recently has been, witnessing decreased fertility, a trend that is expected to be maintained for the next ten to twenty years. Consequently, the estimates used here are based upon the low fertility assumption of 2.10 births per woman also known as replacement rate – i.e., the assumption on which the U.S. Census Bureau's Series E projections are based. See: Neal E. Cutler and Robert A. Harootyan, "Demography of the Aged," in Aging: Scientific Perspectives and Social Issues, ed. Diana S. Woodruff and James E. Birren (New York: Van Nostrand Company, 1975), pp. 31–69. A nationally distributed news story reported that a recent Census Bureau study found the fertility of American women in 1976 at a historical low. "The fertility rate now stands at 1.8 children per woman — exactly half the 1957 level." The report also found that there are now "7.6 million fewer American children aged 13 and under than there were in 1970, even though there are more women of childbearing age." United Press International, "U.S. Fertility Rate Drops to Record Low," Los Angeles Times, February 11, 1977, p. 23. The continuation of this pattern would mean that Tables 1 and 2 underestimate the proportion of older persons in the population.

⁵For discussion of the "economics of the intergenerational relationship," see James N. Morgan,

Table 2. Old Age Dependency Ratios, a 1930-2050

1930	1940	1950	1960	1970	2000	2020	2050
.097	.118	.133	.167	.177	.177	.213	.257

^aDependency Ratio = 65+/18-64; based on demographic data in Table 1.

suggested by the dependency ratios, however, is increased if changes evolve in the age definition of the dependent population. All of the population estimates on which the dependency ratios in Table 2 have been computed are based on a retirement age of 65. Yet there is evidence that the average retirement age of the population is declining and will become even lower as regulations are changed by the Social Security system and other pension systems, and by the personal selection of early retirement options by increasing numbers of workers.

The potential impact of changes in retirement age is indicated by a re-examination of the 1970 census data using age 60+ as defining "old" rather than age 65+. The 1970 dependency ratio would increase from .177 to .273, the latter ratio being more extreme than the ratio indicated in Table 2 for the year 2050 using the 65+ population. Consequently, the social, economic, and political consequences become greater as one considers (a) the increase in the numbers and proportions of older people, (b) their increasing longevity, and (c) a lowering of the age of retirement or dependency.

To this point we have considered the possible political impact of the changing age structure of the population only in terms of absolute numbers and relative proportions. A significant aspect of demographic analysis

"Economic Problems of the Aging and Their Policy Implications," paper prepared for the Gerontological Society Conference on Public Policy Assessment of the Conditions and Status of the Elderly (Santa Barbara: February, 1975); Juanita Kreps, "The Economics of Intergenerational Relationships," in Social Structure and the Family: Generational Relations, ed. Ethel Shanas and Gordon Streib (Englewood Cliffs: Prentice-Hall, 1965), pp. 267–289.

⁶Robin Jane Walther, "Economics and the Older Population," in Woodruff and Birren, pp. 336-351.

which affects political considerations, however, is that of the *composition* of various population groups. Of particular importance to the anticipation of political activity among the aged is the trend in educational level which characterizes successive age cohorts in the United States, since education has been seen to be a major predictor of political activity.⁷

It is well known that the average educational level of the American public has been steadily rising over the past fifty years. This general rise is "caused" by the addition to the population of new cohorts of people who are more highly educated than the cohorts which preceded them, and the dying off of older cohorts who were, as a group, less educated than the cohorts now entering the population system. The implications of these cohort changes for the politics of the aged may be drawn from the age distribution of education presented in Table 3.

In this table each of six national presidential election samples has been divided into high and low education groups, the definition of "high education" being the completion of high school. The data for the 65+ group reveal substantial changes. The older group in 1952 was educated at the beginning of the century when mass public education was not yet widely available. As more educated cohorts enter the system and mature, the average educational level of older persons continuously increases, as is seen by contrasting the 65+ groups in 1952 and 1972.

Finally, we can estimate what the educa-

⁷For example, see Seymour Martin Lipset, *Political Man: The Social Bases of Politics* (New York: Doubleday & Company, 1960), Chapter 6; Lester W.Milbrath and M. L. Goel, *Political Participation*, 2nd ed. (Chicago: Rand McNally, 1977), Chapter 4; Sidney Verba and Norman H. Nie, *Participation in America* (New York: Harper and Row, 1972), Chapter 6.

Table 3. Educational Composition of Selected Age Groups

Age Group	1952		1956		1960		1964		1968		1972	
	Low	High										
21-24	45	55	33	67	39	61	30	70	19	81	14	86
25-34	47	53	34	66	36	64	29	71	21	79	22	78
35-44	58	42	48	52	36	64	34	66	34	66	26	74
45-54	67	33	59	42	47	53	50	50	39	61	40	60
55-64	76	24	65	35	68	32	62	38	55	45	55	45
65+	81	19	78	22	71	29	67	33	.71	29	70	30
Total	62	38	51	49	48	52	45	55	41	59	38	62

[&]quot;Low" education is defined as no education through incomplete high school; "High" education is complete high school (or equivalence) and higher.

Source: Each year was taken from the University of Michigan Center for Political Studies presidential election national survey. The data were provided by the Inter-University Consortium for Political and Social Research through the USC Political and Social Data Laboratory.

tional level of old people will be at the beginning of the next century. The youngest age group in the 1972 sample will all be at least 65 years old by the year 2016, so that by the year 2020 the older population will be a much better educated group than older people of yesterday and today. Whereas only 19 per cent of the 65+group in 1952 had a "high" level of educational attainment, by 2020 86 per cent of the 65+group will have a high education — a rather substantial reversal. While not all educated people will participate in politics, the potential for involvement and activity will certainly be greater for tomorrow's older people than it has been in the past. 8

Participation vs. Disengagement and the Future Politics of Aging

Despite the evidence of substantial growth in the older population, the question remains whether this large and increasingly well-educated population group will in fact participate in politics. One of the widely considered theories in social gerontology argues that as individuals grow old they gradually disengage from active social participation. If in fact older people disengage from, among other things, political activity, then the demographic factors discussed above are not as predictive of the political involvement of old people as might otherwise be expected.

Formulated in the early 1960s, disengagement theory was based upon the observation of a gradual behavioral and psychological withdrawal by older individuals from the various activities which they had pursued in the younger and middle years. As proposed by Cumming and Henry, 9 the theory argues that the older person naturally or inevitably withdraws his or her emotional and actual involvement from the neighborhood, from the family, and from the world of politics. The disengagement process was considered to be natural or inherent because of the inevitability of death and the onset of physiological inabilities. As the individual experienced, or perceived himself to experience, a decline in energy and ability, the withdrawal of emotional involvement in the external world became a means of increasing

⁸Bernice L. Neugarten, "Age Groups in American Society and the Rise of the Young-Old," Annals of the American Academy of Political and Social Science, 415 (September, 1974), 187–198; Bernice L. Neugarten, "The Future and the Young-Old," The Gerontologist, 15 (February, 1975), Part II, 4–9.

⁹Elaine Cumming and William Henry, *Growing Old* (New York: Basic Books, 1961).

psychological well-being or morale. ¹⁰ Furthermore, since the gradual processes of disengagement affect the individual's peripheral involvements first, and then his more personal and central involvements, disengagement, for most people, would affect politics early in the process.

The main criticism of the theory centers on the inevitability of disengagement. Do all individuals in fact wish to disengage from social and political activity? Is such disengagement really a reflection of internal processes, or do social, environmental factors which are not a part of the individual's own aging play a part? The disengagement hypothesis has in fact been severely questioned on both empirical and theoretical grounds. There is evidence, for example, that for some individuals high morale is associated with activity rather than being correlated with withdrawal as was predicted by disengagement theory. Theoretically, several gerontologists have noted that the disengagement hypothesis does not sufficiently consider the external social environment in which the aging individual finds himself - a youngoriented environment that is often hostile to old people and to the symbols of aging. 11

Consequently, new theories of aging are being proposed. Atchley, 12 for example, proposes "continuity theory" as a more general approach to the understanding of the many ways in which people develop in their older years. Continuity theory argues simply that there is a continuity of personality and social orientations between the middle years and old age. Individuals who were highly engaged in society may continue to be highly engaged; those who have had less active patterns of social activity in their earlier life would be expected to evidence less activity in old age.

Recent psychological research in human development has also cast doubt on the scientific validity of any "natural" psychosocial changes associated with the processes of aging. The documentation for many such age-related cognitive and personality gradients is found in cross-sectional studies. In his "General Develop-

¹⁰Vern L. Bengtson, *The Social Psychology of Aging* (New York: Bobbs-Merrill, 1973), p. 44.

¹¹ George Maddox, "Activity and Morale: A Longitudinal Study of Selected Elderly Subjects," Social Forces, 42 (December, 1963), 195-204. A theoretical rebuttal to the inevitability of disengagement is presented in: Arnold M. Rose, "A Current Theoretical Issue in Gerontology," The Gerontologist, 4 (March, 1964), 46-50.

¹² Robert C. Atchley, The Social Forces in Later Life (Belmont, Calif.: Wadsworth Publishing Company, 1972), p. 35.

mental Model," however, Schaie demonstrates that cross-sectional studies of personality do not accurately describe ontogenetic processes. ¹³ Indeed, the application of his cross-sequential research designs to a range of psychological variables has revealed the existence and explanatory importance of the generational cohort dimension of personality patterns within the population. ¹⁴ Thus we may conclude that the apparent age-related changes described for various social and psychological variables are in some cases the artifactual consequence of the inappropriate use of cross-sectional research designs.

As disengagement theory and its presumed behavioral consequences have been studied, discussed, and criticized, an important area of inquiry has concerned the distinction between "personal disengagement" and "societal disengagement." The word disengagement implies that a relationship between two actors is being dissolved. Typically, however, disengagement research has examined only the individual, and not the society from which he is ostensibly disengaging. Yet investigation of society's role in this process has revealed that in fact it is at least as true that society is withdrawing from the older person.

Indeed, Kuypers and Bengtson argue that it is precisely the "social labeling" of symbols of aging by society which accounts for the negative image which many old people have of themselves, and the subsequent withdrawal of old people from social activity. They note that in the course of normal aging, the older person experiences various events which withdraw the usual sources of feedback supportive of an individual's self-image — the loss of role, the loss of spouse, the ambiguity of social information about aging, and the weakening of reference group support. "This feedback

13K. Warner Schaie, "A General Model for the Study of Developmental Problems," *Psychological Bulletin*, 64 (August, 1965), 92-107.

14Paul B. Baltes, "Longitudinal and Cross-Sectional Sequences in the Study of Age and Generation Effects," Human Development, 11 (No. 3, 1968), 145–171; K. Warner Schaie and Gisela V. Labouvie-Vief, "Generational vs. Ontogenetic Components of Change in Adult Cognitive Behavior: A Fourteen-Year Cross-Sequential Study," Developmental Psychology, 10 (May, 1974), 305–320; K. Warner Schaie and Iris A. Parham, "Stability of Adult Personality Traits: Fact or Fable?", Journal of Personality and Social Psychology, 34 (1976), 146–158; K. Warner Schaie and Charles R. Strother, "A Cross-Sequential Study of Age Changes in Cognitive Behavior," Psychological Bulletin, 70 (December, 1968), 671–680; Diana S. Woodruff and James E. Birren, "Age Changes and Cohort Differences in Personality," Developmental Psychology, 6 (March, 1972), 252–259.

vacuum creates a vulnerability to, and dependence on, external sources of self labeling, many of which communicate a stereotyped negative message of the elderly as useless and obsolete." 15

Thus, the individual's apparent withdrawal from social participation may reflect not so much inevitable psychodynamic processes, but more simply a realistic response to society's constriction of the "opportunity structure" for participation. Some studies indicate that when the opportunity structure is opened to participation, older persons do not necessarily disengage. Carp, for example, studied a group of older persons as they moved from one residential facility to another. 16 The respondents' desire for engagement in three kinds of activity was measured: paid work, volunteer work, and leisure activity. The opportunities for each of these was limited in the first residential environment, but were expanded in the second residence. The results indicated that in the second environment the older respondents did engage in those activities which fit with their predispositions and expressed a high degree of satisfaction and happiness in the situation of increased engagement.

A second example focused on engagementdisengagement patterns among retired professors. Academic life is one of the few areas of livelihood which has an institutionalized postretirement role - that of the emeritus professor - thus providing an opportunity structure for continued activity and engagement beyond the official retirement age. In their study of retired professors, Roman and Taietz found the following: 41 per cent were engaged in work at the same university; 13 per cent were professionally engaged elsewhere; 24 per cent were in ill health; and 22 per cent had disengaged from both university and profession. 17 When the percentages are recomputed deleting those in ill health who could not make a decision about engagement, the results of the study show that 71 per cent of the professors were engaged in their profession and only 29 per cent were disengaged. As Atchley comments on the study: "These data suggest that the frequency

¹⁵ Joseph A. Kuypers and Vern L. Bengtson, "Social Breakdown and Competence: A Model of Normal Aging," Human Development, 16 (No. 3, 1973), 182.

¹⁶Frances M. Carp, "Person-Situation Congruence in Engagement," The Gerontologist, 8 (Autumn, 1968), 184-188.

¹⁷Paul Roman and Philip Taietz, "Organizational Structure and Disengagement: The Emeritus Professor," *The Gerontologist*, 7 (September, 1967), 147-152.

of disengagement is very much a product of the opportunity for continued engagement.... This finding is all the more revealing since almost half of Roman and Taietz's sample was over seventy-five years old." 18

Research on political behavior is especially useful in evaluating any connection between aging and disengagement. Unlike other areas of social activity, no formal or legal rules require people to withdraw from political activity at a given age. In fact, legislatures and courts are often cited as prime examples of institutions in which age offers no functional barrier. ¹⁹ Data on political orientations and behavior also suggest that disengagement is not an inevitable consequence of the aging process.

Table 4 presents an age distribution of voter participation by sex and education. The patterns disclose a general increase in participation associated with age, with little evidence of disengagement from this form of political activity among the males. Whether the decline in voting by females is due to "aging" per se, to widowhood, or to patterns of generational difference in female political activity is not ascertainable from these data. Nonetheless, the old-age decrements in voter participation are not as significant as are the differences associated with sex and education. Indeed, Verba and Nie's study of political participation in the

United States demonstrates that when statistically precise controls for education and income are employed, there is no noticeable decrease in age patterns of voting or general participation.²⁰

A second example concerns age patterns in self-reporting of political interest. Table 5, which presents data describing those who indicated a "great deal" of interest in politics, reveals quite the opposite of disengagement from politics among older respondents. As the authors of the study conclude, "the highest reported interest is consistently at age 60 and higher, and the difference between the middle-aged and the elderly is pronounced at most educational levels for both sexes." ²¹

As a final example, Table 6 presents an age distribution of self-reports of political awareness, aggregated across ten national Gallup surveys in the 1950s and 1960s.²² As in the previous examples, these data yield no evidence of substantial disengagement associated with age. Sex and educational differences are greater than age differences, and although some reduction in awareness is reported for the total samples in the 60+ group, these decrements are not substantial.

²⁰Verba and Nie, Participation in America, Chapter

Table 4. Voting in the 1964 Presidential Election, by Age and Education (Percentage voting)

		·	Years of	Education			
	Eleme	mentary High School		School	College		
Age	0-7 Years	8 Years	1-3 Years	4 Years	1-3 Years	4 Years or More	Total
Males		,					
21-24	14	30	34	56	70	80	53
25-44	43	58	65	76	82	87	71
4564	63	79	81	88	88	93	79
65+	66	7 9	80	88	91	88	74
Total 21+	58	73	69	80	82	88	73
Females							
21-24	22	21	33	55	69	78	52
25-44	34	52	58	76	85	87	68
45-64	52	69	75	84	87	92	74
65+	46	63	72	76	82	93	61
Total 21+	45	63	63	76	83	89	68

Source: Anne Foner, "The Polity," in Aging and Society, Volume III: A Sociology of Age Stratification, ed. Matilda White Riley, Marilyn Johnson, and Anne Foner (New York: Russell Sage, 1972), pp. 115-159, Table 4.1, as adapted from Current Population Reports, 1965, P-20, No. 143, pp. 16-19.

¹⁸ Atchley, The Social Forces in Later Life, p. 223.
19 For example, see the discussion in Neal E. Cutler and John R. Schmidhauser, "Age and Political Behavior," in Woodruff and Birren, pp. 374-406.

²¹Norval D. Glenn and Michael Grimes, "Aging, Voting, and Political Interest," *American Sociological Review*, 33 (August, 1968), p. 570.

²²Norval D. Glenn, "Aging, Disengagement, and Opinionation," *Public Opinion Quarterly*, 33 (Spring, 1969), 17-33.

Table 5. Self-Reported Interest in Politics (Percentage reporting "great deal" of interest)

	Ages				
Years of School Completed	21-39	40–59	60 and Up		
Males	-				
8	18.2 (33)	15.2 (112)	26.8 (97)		
9–11	20.8 (72)	21.2 (99)	31.1 (45)		
12	23.1 (173)	26.6 (139)	28.6 (42)		
All educational levels	27.4 (456)	24.9 (502)	29.5 (298)		
Females					
8	5.3 (38)	14.4 (111)	21.2 (104)		
9–11	18.5 (130)	21.1 (90)	26.2 (42)		
12	16.1 (192)	25.5 (165)	48.2 (56)		
All educational levels	19.0 (496)	22.7 (520)	34.5 (330)		

Glenn and Grimes report that they inflated the sample by about 100 per cent through a weighting procedure used to increase representativeness. Therefore, the reported N's (in parentheses) are about twice the number of respondents represented.

The data are from Gallup Survey 637 (1960); data are for white respondents.

Source: Norval D. Glenn and Michael Grimes, "Aging, Voting, and Political Interest," American Sociological Review, 33 (August, 1968), 570, Table 3.

In sum, the examples drawn from the realm of political behavior do not support the disengagement theorists. Clearly, it should not be expected that all individuals inevitably withdraw their emotional and behavioral support of social activity as a function of age. Recent research demonstrates that the opportunity structure of a particular domain of activity is a factor which substantially influences whether or not older persons who are predisposed to continued activity will in fact manifest sustained activity. Since there are no formal or legal age barriers to political activity, engagement or disengagement becomes a matter of individual choice. These data suggest that poli-

tical activity appears to *increase* rather than decrease with age, and that this general pattern continues at least into the sixth decade of life.

Age, Conservatism, and Political Demands

When we add the open opportunity structure provided by politics to the increasing numbers and changing educational levels of successive birth cohorts there is reason to expect substantial political activity from the older groups within the population. Yet the question arises whether or not older people are likely to make demands upon the government for the allocation of resources. One of the most

Table 6. Average Percentage of "Yes" Responses to Eleven "Have-You-Heard-or-Read" Questions, by Age. Sex, and Education

Years of School Completed	21-39	40–59	60 and Up	
Males				
7–8	56.2	57.8	61.4	
1-3 of high school	61.5	70.2	70.8	
4 of high school	70.5	72.8	77.3	
All educational levels	69.0	68.3	65.6	
Females				
7–8	41.8	45.7	51.1	
1-3 of high school	52.5	54.6	55.6	
4 of high school	59.1	65.2	62.6	
All educational levels	57.9	57.8	53.0	

Examples of the questions are: "Have you heard or read anything about the trouble in the Formosa area?" (1955) and "Have you heard or read about the Black Muslims?" (1963). The ten surveys from which the questions come are Gallup Surveys 521, 524, 532, 543, 582, 588, 649, 673, 675, and 701; data represent white respondents only.

Source: Norval D. Glenn, "Aging, Disengagement, and Opinionation," Public Opinion Quarterly, 33 (Spring, 1969), 27, Table 5.

popular myths in American political folklore is that aging brings about a conversion to conservatism.²³ In terms of the dominant issues of domestic American politics in the past forty years, the conservative-liberal dimension has generally been linked to the question of federal vs. local responsibility in political matters, or governmental vs. private responsibility for the welfare of the individual. If in fact aging produces political conservatism, then perhaps there is no need to be concerned with the impact of older people on politics, since the conservative may not press demands upon the national political system for increased dollars and services.

There is, of course, some basis in social psychological studies to argue that the processes of maturation yield a less open view of the world, that psychological rigidity and conservatism in personal values are characteristic of the orientations of the aged. Experiments have demonstrated that older persons may be less open to attitude change, and more set in their ways of problem solving.²⁴ Interestingly, studies of political attitudes and values are often cited by psychologists as additional evidence of the general age-related conservatism.

At least three factors associated with the onset of old age can provide the frustrations and threats to the individual which may be causes of rigid and conservative behavior patterns. First, age brings significant losses in personal status; while new statuses may be obtained, they cannot really replace those which had been the basis of a lifetime of social gratification. Second, although biological losses do occur with age, the actual biological losses are not as important as are the individual's perception of such losses. Third, with old age comes stability of life style, occupation, career, residential location, and family choices. The

latitude for new areas of choice may quite realistically be limited, and the older person may thus feel a loss of personal freedom.

These social and psychological manifestations of old age create a situation in which the aging individual is quite susceptible to real and imagined threats. Since the individual is facing losses in status, in perceived biological performance, and in personal freedom, new situations may be seen as threatening. Consequently, older persons, like persons of all ages, may respond to threatening situations through the application of their own personally-validated stereotypes. 27

Thus, there is evidence of a tendency toward conservatism which may come with advanced age, and there is at least some social psychological theory on which to base a general claim for such conservatism as a response to changing and threatening situations. Yet even the gerontological evidence is not perfectly clear; studies typically use different conceptual and operational measures of conservatism, and the age differences are not sharp: "...seldom is the trend strong enough to put the majority of the young on one side of an issue and the majority of the old on the other side." 28

An examination of three areas of attitude data pertaining to the conservatism issue in American domestic politics yields little support for the idea that older people are, or become, substantially more conservative than the rest of the population. As a first example, Table 7 presents some of the earliest public opinion data available on this topic - Hinshaw's presentation of Gallup poll data from the late 1930s.²⁹ A number of issues germane to the liberal-conservative dimension as defined in terms of the scope of federal activity are included here. In some cases, for example on the matters of federal ownership of railroads, electric companies, and radio stations, there are no patterns of age differentiation. Where there is some evidence of a more conservative position taken by the older group these differences are rather small, as in the questions pertaining to federal regulation of business, and they certainly do not put the old on the conservative

²³A recent formulation of this view is included in: Robert R. Hudson and Robert H. Binstock, "Political Systems and Aging," in *Handbook of Aging and The Social Sciences*, ed. Robert H. Binstock and Ethel Shanas (New York: Van Nostrand, 1976), pp. 369—400.

²⁴Sidney L. Pressey and Raymond G. Kuhlen, Psychological Development Through the Life Span (New York: Harper & Row, 1957), pp. 436-505;
Leonard Z. Breen, "The Aging Individual," in Handbook of Social Gerontology, ed. Clark Tibbitts (Chicago: University of Chicago Press, 1960), pp. 145-162;
H. J. Heglin, "Problem Solving Set in Different Age Groups," Journal of Gerontology, 11 (July, 1956), 310-316.

²⁵Raymond G. Kuhlen, "Changing Personal Adjustment During Adult Years," in *Psychological Aspects of Aging*, ed. John Anderson (New York: American Psychological Association, 1956), pp. 21–35.

²⁶Kuypers and Bengtson, "Social Breakdown and Competence."

²⁷Breen, "The Aging Individual," p. 154.

²⁸Raymond G. Kuhlen, "Aging and Life Adjustment," in *Handbook of Aging and the Individual*, ed. James E. Birren (Chicago: University of Chicago Press, 1959), p. 882.

²⁹Robert Hinshaw, *The Relationship of Attitudes and Opinions to Age*, Ph.D. dissertation, Department of Psychology, Princeton University, 1944.

side of the issue and the young on the liberal side. In fact, on the issue concerning the limitation of private fortunes, the older respondents took a more liberal position than the younger respondents.

A second example concerns the relationship between aging and identification with the Republican party in the United States, since Republican identification has been used as one measure of political conservatism. At least in terms of general public perceptions there is an association between old people, conservatism, and the Republican party: "The fact that the older generation of the electorate is most heavily identified with the Republican Party and most unswerving in its devotion to it

conforms to the general perception of older people as the most conservative element of the body politic."30

Use of the phrase older generation in referring to older people in the contemporary electorate points to what has been an interesting controversy in recent political research. It has been observed that in the 1950s and 1960s, in any given election or any given year most older persons were Republicans while most younger people were Democrats. Do we conclude from such data that the aging process has

30 Angus Campbell, "Politics Through the Life Cycle," The Gerontologist, 11 (Summer, 1971), 114, emphasis added.

Table 7. Early Gallup Data on the Scope of Federal Power

	Yes	No	No Opinion	N
Young ^a	27%	59%	14%	925
Medium	28	58	14	1160
Old	26	59	15	970
Do you think the Gover	nment should ow Yes	n the electric co No	ompanies? 7–31 -40 No Opinion	N
Young	30%	47%	23%	450
		45	23	541
Medium	32	40	43	341

3. Would you favor Federal Government ownership and control of all radio broadcasting stations? 5-20-38

	Yes	No	No Opinion	N
Young	15%	70%	15%	990
Medium	17	64	19	1202
Old	16	64	20	960

4. During recent years the trend has been to give the Federal Government greater power to regulate business, industry, and agriculture. Are you in favor of this trend towards centralizing the power in Washington? 8-9-37

	Yes	No	No Opinion	N
Young	45%	39%	16%	992
Medium	40	44	16	1035
Old	36	48	16	915

5. During the next four years, do you think there should be more regulation or less regulation of business by the Federal Government than at present? 10-24-40

	More	Same	Less	No Opinion	N
Young	25%	21%	33%	21%	781
Medium	22	18	42	17	1136
Old	20	19	42	19	1235

6. Do you think the size of private fortunes should be limited? 6-7-37

	Yes	No	No Opinion	N
Young	40%	51%	9%	1021
Medium	43	50	7	988
Old	48	42	9	812

 $^{^{2}}$ Young = 18-30; medium = 31-47; old = 48+.

Source: Robert Hinshaw, The Relationship of Attitudes and Opinions to Age (Ph.D. dissertation, Department of Psychology, Princeton University, 1944), pp. 21-22, Table III.

brought about a change in the conservative direction, or that the conservatism or Republicanism of contemporary older voters is simply a generational cohort phenomenon?

A number of studies have investigated these alternatives. The authors of The American Voter suggested on the basis of the accumulated cross-sectional evidence of the 1952 through 1958 election surveys that the New Deal period had created a generational hold on young people who were voting for the first time during that period.31 One study which argued that aging produces a conversion to political conservatism focused on an analysis of the relationship between age and Republican party identification, analyzed in a sequence of Gallup polls taken at four-year intervals from 1946 through 1958. While it was argued that the data demonstrated a linear change to the conservative alternative, a re-analysis of some of the data argued that the conclusion connecting increasing Republicanism to aging was not supportable when the data were analyzed using the technique of cohort analysis.32

In a follow-up study Glenn and Hefner employed a data base including surveys taken every four years between 1945 and 1959 in which they employed formal cohort analysis procedures, included controls for sex and education, and statistically corrected the data for the underrepresentation of lower-class respondents in the earlier Gallup surveys. 33 Thus, the study brought to the analysis of the relationship between aging and conservatism an expanded data base and much methodological experience and sophistication. The authors concluded that "this study should rather conclusively lay to rest the once prevalent belief that the aging process has been an important influence for Republicanism in the United States."34

³¹Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), pp. 153–156.

32John A. Crittenden, "Aging and Party Affiliation," Public Opinion Quarterly, 26 (Winter, 1962), 648-657; Neal E. Cutler, "Generation, Maturation, and Party Affiliation: A Cohort Analysis," Public Opinion Quarterly, 33 (Winter, 1970), 583-588. This exchange is reprinted in Quantitative Analysis of Political Data, ed. Samuel A. Kirkpatrick (Columbus, Ohio: Charles E. Merrill, 1974), Chapter 6, pp. 465-482.

33 Norval D. Glenn and Ted Hefner, "Further Evidence on Aging and Party Identification," Public Opinion Quarterly, 36 (Spring, 1972), 31-47.

34Ibid., p. 47. Despite the "conclusiveness" of this statement, the issue is alive and well in political science. A sharp exchange can be followed in: Paul R. Abramson, Generational Change in American Politics (Lexington, Mass.: D.C. Heath, 1975); Philip E.

As a final example of the lack of any inherent social policy conservatism on the part of older people, we may look to analyses of the attitude measures of federal involvement in society included in the election studies conducted by the University of Michigan Center for Political Studies. As reported by Campbell, an acceptable Guttman scale was formed using items on federal involvement in employment guarantees, school construction, the delivery of medical services, fair treatment of blacks in jobs and housing, and the question of whether electricity and housing should be left totally to private enterprise.³⁵ The scale behaved as expected, with Republicans exhibiting more conservative orientations than Democrats, high income people being more conservative than middle- or low-income people, and the highly educated being more conservative than the less educated. Campbell reports the surprise of the analysts, however, when it turned out that the young were more conservative than the old.

Decomposition of the scale into its component attitudes quickly indicated the source of this reversal. While for most people in the electorate the five issues were part of an overall attitudinal orientation, the older people were more likely to endorse certain items but not others. Specifically, on questions concerning governmental support of medical programs and guarantees of full employment — issues of particular relevance to the welfare of the aged — the older respondents were substantially less conservative than the younger members of the electorate, a conclusion supported also by more recent analyses of the Michigan data. 36

That older members of the electorate are not conservative on certain issues and are likely to make demands upon government for the expansion of programs aimed at their own well-being

Converse, The Dynamics of Party Support: Cohort Analyzing Party Identification (Beverly Hills: Sage Publications, 1976); Paul R. Abramson, "Developing Party Identification: A Further Examination of Life-Cycle, Generational, and Period Effects," paper prepared for presentation at the Annual Meeting of the American Political Science Association, Washington, D.C., September, 1977.

³⁵ Angus Campbell, "Social and Psychological Determinants of Voting Behavior," in *Politics of Age*, ed. Wilma Donahue and Clark Tibbitts (Ann Arbor: University of Michigan, 1962), pp. 87–100.

³⁶Campbell, "Politics Through the Life Cycle"; E. M. Schreiber and Lorna R. Marsden, "Age and Opinions on a Government Program of Medical Aid," Journal of Gerontology, 27 (January, 1972), 95-101; Jerry L. Weaver, "The Elderly as a Political Community: The Case of National Health Policy," Western Political Quarterly, 29 (December, 1976), pp. 610-649.

is predictable on the basis of previous analyses of American political orientations. In The American Voter the Michigan analysts classified all voters as to the level of issue content expressed in their attitudes. The four general categories employed were: ideology or nearideology, group benefits, nature of the times, and no issue content at all. The modal category for the electorate was the "group benefits" orientation, a category into which 45 per cent of the electorate was classified.³⁷ While the analysis did not present any information on age patterns in these classifications, there is no reason to believe that older voters are different in this regard from the rest of the electorate. On the contrary, as older persons increasingly recognize the particular needs which accompany aging, especially in the context of low levels of retirement and pension income, they may be more likely to take a group-benefits orientation toward political issues.38

In summary, then, this discussion demonstrates that despite the popular image of old people as being the most conservative age group politically, there is little evidence of an age-related political conservatism on the basic domestic political issue concerning the role of the federal government in the areas of economics and social welfare. As a result of such factors as the group-benefits orientation and differences between generational cohorts, we should not uncritically expect older members of the electorate in the future to be conservative on the issue of making increasing demands upon the federal government for the allocation of scarce resources.

Summary and Conclusion: Research Questions for "Political Gerontology"

The focus of this analysis has been to emphasize the basic factors relevant to the future role of age and the aged in American politics, factors which provide a foundation for more specific research in "political gerontology." First, population trends are producing larger numbers and proportions of older persons. But more directly germane to political conflicts, dependency ratios demonstrate that the baby boom cohorts of the 1940s will become a "gerontology boom" at a time when the lowered birth rates of the 1970s will have produced a relatively smaller number of income-producing workers. At the same time, tomorrow's older population will reflect the education, political experience, and activist life-styles which characterize the younger adults of the 'sixties and 'seventies.³⁹

Second, recent gerontological evidence indicates that older people do not necessarily "disengage" from social activity. Indeed, the greater the opportunity for participation, such as that provided by the political arena, the more likely that continuity will characterize the behavior patterns of older persons.

The third factor focuses on a basic myth of contemporary political folklore: that people become politically conservative as an inevitable consequence of the processes of aging. At least two factors strongly suggest that future groups of older persons will not necessarily be basically conservative. First, several analyses conclude that the conservatism observed in recent years among older persons is generational in nature, and not related to the aging process. Second, a group-benefits orientation in American politics typically motivates individuals to support programs which will yield benefits for their group. Given the increased needs on the part of the aged for medical care, the rising costs of medical care, and widespread poverty among the aged, it is no surprise that older people have been found to be even more favorable toward federal medical care programs than are the traditionally less conservative younger voters.

The thrust of these three factors — demographic, social-psychological, and political — is that the political system will be increasingly affected by the politics of age in future years, and substantially so by the beginning of the next century. Of course, we do not mean to suggest that these three factors by themselves

39In a recent paper Ragan suggests that the beginnings of a "backlash" against the increasing benefits now being allocated for the elderly can already be detected in both the popular and the academic press. While most of the reviewed articles and statements did not "view with alarm" the current state of affairs, many comments pointed to the conflict-prone situation summarized by the dependency ratios. Pauline K. Ragan, "Another Look at the Politicizing of Old Age: Can We Expect a Backlash Effect?", paper prepared for presentation at the Annual Meeting of Society for the Study of Social Problems, New York, August, 1976.

³⁷ Campbell et al., The American Voter, p. 249.

³⁸A generational cohort analysis of attitudes toward federal responsibility in the provision of medical care, based upon the Michigan presidential election surveys, quite clearly revealed that conorts observed aging from 60-64 to 65-68 years of age were substantially more supportive of the liberal (federal) position than were either the youngest cohorts in the electorate or the electorate as a whole. Furthermore, this pattern was found for each pair of successive elections over the 1952-1972 period. See: Vern L. Bengtson and Neal E. Cutler, "Generations and Intergenerational Relations: Perspectives on Age Groups and Social Change," in Binstock and Shanas, pp. 130-159.

will determine the future influence of older people upon American politics. Indeed, some might argue that the politics of the year 2000 cannot be foreseen, and that events of the next twenty-five years may completely reorient American politics. In the absence of such a reorientation, however, the factors discussed here do predict the increasing salience of the age factor in politics. To the degree that this prediction is considered plausible, then a number of additional questions for political researchers requires investigation. We shall conclude this overview by suggesting three broad areas of research in "political gerontology."

The first is the issue of generational differences in political orientations. Much of our knowledge about the political behavior of the mass public is based on cross-sectional inquiries. Yet as our analysis of the issue of conservatism has illustrated, generalizations which may be descriptively valid for the polity at midcentury are not necessarily valid for the year 2000. The concept of "generations" has been part of social analysis for dozens of years, 40 and has recently become popular in the study of youth and political socialization. 41

Although the concept of generations has been useful in the analysis of successive cohorts of adolescents, a substantial and growing literature in the area of developmental psychology suggests that the generational concept is appropriate for the study of adults as well. Psychologists, perhaps even more so than political scientists, have traditionally been willing to accept cross-sectional age-correlations as evidence of "inherent" maturational or ontogenetic human processes. Yet, as described in the second section of this article, an increasing number of psychological studies demonstrates that a variety of "basic" personality attributes are distributed within the population as a function of generational differences. And since

⁴⁰Karl Mannheim, "The Problem of Generations," in Essay on the Sociology of Knowledge, ed. and transl. Paul Kecskemeti (London: Routledge and Kegan Paul [1928], 1952), pp. 276-322; Julian Marids, "Generations — The Concept," in International Encyclopedia of the Social Sciences, Volume VI (New York: The Free Press, 1968), pp. 88-92.

41 Vern L. Bengtson, "The Generation Gap: A Review and Typology of Social-Psychological Perspectives," Youth and Society, 2 (September, 1970), 7–31; Neal E. Cutler, "Toward a Political Generations Conception of Political Socialization," in New Directions in Political Socialization, ed. David C. Schwartz and Sandra Kenyon Schwartz (New York: The Free Press, 1975), pp. 363–409; M. Kent Jennings and Richard Nieml, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations," American Political Science Review, 69 (December, 1975), 1316–1335.

the chronological age of a respondent in an interview study or subject in an experiment is an index of both developmental age and generational location, the generational dynamic may rival the traditional developmental explanation for a range of human attributes.

Political scientists and sociologists have also begun to consider the generational explanation of age-related phenomena. Indeed, one recent sociological analysis argues that generational trends in retirement programs will alter society's very definition of "old age," so that by the year 2000 the "young old" will be a large group of people aged 55-75, who will be healthier, more educated, and more politically involved than generations of older persons in the past.⁴² And within political science, several studies have analyzed such political phenomena as partisanship, voter turnout, alienation and tolerance, attitudes toward domestic and foreign policy attitudes, and political ideology, in the context of generational-versus-maturational explanations.⁴³

In studying the response of political systems to historical patterns of public demands, therefore, political scientists will increasingly have to recognize that evidence of generational trends may invalidate many of our most strongly held generalizations. And since data bases and methodologies appropriate for trend and generational analysis are becoming increasingly available, 44 the generational analysis of poli-

 $^{42}\mbox{Neugarten},$ "Age Groups in American Society and the Rise of the Young-Old," and Neugarten, "The Future and the Young-Old."

43Summaries and reviews of much of this literature may be found in: Neal E. Cutler, "Generational Analysis and Political Socialization," in Handbook of Political Socialization: Theory and Research, ed. Stanley A. Renshon (New York: Free Press, 1977); Neal E. Cutler, "Generational Approaches to Political Socialization," Youth and Society, 8 (December, 1976), 175–207.

44 For a general discussion see Herbert H. Hyman, Secondary Analysis of Sample Surveys: Principles, Procedures, and Potentialities (New York: Wiley, 1972), Chapter 7. See also: William M. Evan, "Cohort Analysis of Survey Data: A Procedure for Studying Longterm Opinion Change," Public Opinion Quarterly, 23 (Spring, 1959), 63–72; Neal E. Cutler, The Alternative Effects of Generations and Aging Upon Political Behavior: A Cohort Analysis of Attitudes Toward Foreign Policy, 1946–1966 (Oak Ridge: Oak Ridge National Laboratory, 1968); Karen Mason, William Mason, H. H. Winsborough, and W. Kenneth Poole, "Some Methodological Issues in Cohort Analysis of Archival Data," American Sociological Review, "Aging and Cohort Succession: Interpretations and Misinterpretations," Public Opinion Quarterly, 37 (Spring, 1973), 35–49; Norval D. Glenn, "Cohort Analysts' Futile Quest: Statistical Attempts to Separate Age, Period, and Cohort Effects," American

tical variables should receive continuing and substantial attention.

A second area of needed research concerns the connection between age consciousness and political attitudes. Social gerontologists have long studied the social-psychological correlates of age consciousness. In the view of one sociologist, age consciousness is similar to class consciousness: while everybody objectively has an age and a class, all people do not identify with that age or class. 45 Yet to the degree to which old people become aware of themselves as old, they may take on the politically relevant characteristics of a minority group or subculture. 46 Several studies have shown age consciousness to be related to such phenomena as activity, morale, adjustment, and happiness. 47

In one recent study the chronologically old were separated into those who identified with old age and those who did not, and a number of interesting political differences were observed.⁴⁸ The subjectively old were found to be more liberal on issues of the desired scope of federal activity; more conservative with respect to such contemporary social controversies as legalization of marijuana, abortion, school bussing; and more pessimistic about both their own and the nation's economic future. The important point for political researchers is the recognition that age identification, like class consciousness, partisan identification, ethnic identification, and sex identification, is potentially a potent force in the organization of citizens and the mobilization of political activity. And when combined with the demographic

Sociological Review, 41 (October, 1976), 900-908; Norval D. Glenn, Cohort Analysis (Beverly Hills: Sage Publications, 1977). changes described previously, age consciousness represents a particularly important topic for political research.

A third area of needed political research concerns the connection between social organization and the impact of age issues and the aged upon politics. Unlike age-based organizations of the past, modern organizations of the elderly are likely to be relatively permanent and to have a greater potential impact on politics. Such organizations as the American Association of Retired Persons and the National Council of Senior Citizens have funding sources outside of their own memberships, are bureaucratically rationalized and professionally staffed, have geographically and politically heterogeneous memberships, and are interested in a wide range of political issues.⁴⁹

The study of the political role of age-based organizations should focus on three kinds of behavior. First, many of these organizations have large offices in Washington as well as in selected state capitals, and thus formally participate in the political process. They employ professional lobbyists, testify at hearings — especially those of the Senate Special Committee on Aging and the House Select Committee on Aging — and often draft legislation. Indeed, such national programs as HEW's network of county-based Area Agencies on Aging specifically require the local participation of organizations of the elderly in decision making. 50

Second, many age-based organizations establish formal coalition ties with other organizations. Thus the potential political impact of the Gray Panthers and the National Caucus of the Black Aged, for example, is enhanced by their collaborative political efforts with organizations (and resources) of the non-aged.⁵¹

Third, a major product of age-based organizations is their work in raising the age con-

⁴⁵Matilda White Riley, "Social Gerontology and the Age Stratification of Society," *The Gerontologist*, 11 (Spring, 1971), Part I, 79–87.

⁴⁶ Arnold M. Rose, "The Subculture of the Aging: A Framework for Research in Social Gerontology," in Older People and Their Social World, ed. Arnold Rose, and Warren Peterson (Philadelphia: F. A. Davis, 1965), pp. 3–16; Gordon F. Streib, "Are the Aged a Minority Group?", in Applied Sociology, ed. Alvin W. Gouldner and S. M. Miller (New York: The Free Press, 1965).

⁴⁷George R. Peters, "Self-Conceptions of the Aged, Age Identification, and Aging," The Gerontologist, 11 (Winter, 1971), Part II, 69–73; Warren A. Peterson, "Research Priorities on Perceptions and Orientations Toward Aging and Toward Older People," The Gerontologist, 11 (Winter, 1971), Part II, 60–63; Donald G. McTavish, "Perceptions of Old People: A Review of Research Methodologies and Findings," The Gerontologist, 11 (Winter, 1971), Part II, 90–101.

48Neal F. Coulon "The Asian Revolutions and

⁴⁸ Neal E. Cutler, "The Aging Population and Social Policy," in Aging: Prospects and Issues, ed. Richard H. Davis (Los Angeles: University of Southern California Press, 1976), pp. 102-126.

⁴⁹Henry J. Pratt, "Old Age Associations in National Politics," Annals of the American Academy of Political and Social Science, 415 (September, 1974), 106-119; Henry J. Pratt, The Gray Lobby (Chicago: University of Chicago Press, 1976).

⁵⁰ Robert B. Hudson, "Rational Planning and Organizational Imperatives: Prospects for Area Planning in Aging," Annals of the American Academy of Political and Social Science, 415 (September, 1974), pp. 41–54; Raymond M. Steinberg, A Longitudinal Analysis of 97 Area Agencies on Aging, Research Reports of the Social Policy Laboratory, Andrus Gerontology Center, University of Southern California, May, 1975.

⁵¹ Jacquelyne Johnson Jackson, "NCBA, Black Aged and Politics," Annals of the American Academy of Political and Social Science, 415 (September, 1974), 138–159.

sciousness of old people. The advertisements and publications of several of these organizations specifically aim at educating their constituency about the older person's special position in society, and the potential political advantage in pursuing "senior advocacy" through organizational strategies. ⁵² Indeed, in recent years one need only note the civil rights movement and the women's movement as examples of the role which organizations play in translating category membership into group consciousness and group consciousness into political action.

Some observers have suggested that the aged historically have not been successfully involved in politics on their own behalf; for example, in the passage of Social Security in 1935 and Medicare thirty years later, the organized elderly were less involved and influential than were several other sectors of society. ⁵³ Consequently, such analysis concludes, the aged are unlikely to become a substantial political force in

future politics.54 Yet evidence presented in this article suggests that past may not be prologue to the role of older people in American politics. We are beginning to see evidence that the current cohort of older persons is setting the stage for age consciousness and age-based political action. Political scientists should begin now to formulate research questions in order to see if and how over the next twenty-five years the baby boom cohort of the 1940s - socialized in the intense political atmosphere of the 1960s and 1970s and aging in a period of increasing age consciousness - will become an activist gerontology boom of the year 2000, press demands of the aged upon the political system, and thus establish new patterns for the politics of old age. As this article has endeavored to demonstrate, it is the multidisciplinary study of these emerging political patterns that provides a research guide for "political gerontology."

54Binstock in particular takes the position that the elderly are most unlikely to wield substantial political influence on their own behalf either through electoral politics or interest group politics. "If government responds positively to the challenge of expanding services and facilities [for the elderly], it will be because of pervasive ideological considerations and not because it is pushed to do so by senior power." Robert H. Binstock, "Aging and the Future of American Politics," The Annals of the American Academy of Political and Social Science, 415 (September, 1974), 199-212.

⁵²See the several articles in *Advocacy and Age*, ed. Paul A. Kerschner (Los Angeles: University of Southern California Press, 1976).

⁵³Robert H. Binstock, "Interest-Group Liberalism and the Politics of Aging," The Gerontologist, 12 (Autumn, 1972), 265-280; Dale Vinyard, "Rediscovery of the Aged: 'Senior Power' and Public Policy," paper prepared for delivery at the Annual Meeting of the American Political Science Association, Chicago, September, 1976.

Economic Power and Political Influence: The Impact of Industry Structure on Public Policy

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Fev: questions are as important to an understanding of American democracy as the relationship between economic power and political influence. 1 Yet few questions have received so many disparate answers from scholars and pundits alike. While few students of the subject would deny that corporations have unusual resources for political action, there is considerable disagreement over the extent of their success in bringing these resources to bear in the policy process. Depending on which examples one chooses, in fact, it is possible to conclude that American corporations are either politically impotent or politically omnipotent, with infinite variations in between. Some writers see in the extensive interchanges between corporate and governmental elites, the frequent success of corporate lobbying in both legislative and administrative realms, and the widespread pattern of private expropriation of public authority sufficient evidence to support Woodrow Wilson's charge that "the masters of

¹This point is argued convincingly in: Henry Kariel, The Decline of American Pluralism (Stanford: Stanford University Press, 1961); Grant McConnell, Private Power and American Democracy (New York: Alfred A. Knopf, 1966); Theodore Lowi, The End of Liberalism (New York: W. W. Norton and Company, Inc., 1969); Robert A. Dahl, "Business and Politics: A Critical Appraisal of Political Science," in Robert A. Dahl, Mason Haire, and Paul F. Lazarsfeld, Social (New York: Columbia University Press, 1959), pp. 3, 44; Robert A. Dahl, After the Revolution (New Haven, Conn.: Yale University Press, 1970); Edwin M. Epstein, The Corporation in American Politics (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969).

the government in the United States are the combined capitalists and manufacturers of the United States." But others stress the alternative sources of power in the political process and the nonpolitical activities that can absorb the energies and attention of the economic elite. 3

While the literature on the subject contains a view of the relationship between economic power and political influence to suit almost every taste, however, there are surprisingly few

²Woodrow Wilson, The New Freedom (New York: Doubleday, 1913), p. 57. See, for example: C. Wright Mills, The Power Elite (New York: Oxford University Press, 1956); G. William Domhoff, Who Rules America? (Englewood Cliffs, N.J.: Prentice-Hall, 1967); Floyd Hunter, Community Power Structure (Chapel Hill: University of North Carolina Press, 1953); Robert S. Lynd and Helen M. Lynd, Middletown in Transition (New York: Harcourt, Brace, Jovanovich, 1937); Morton Mintz and Jerry Cohen, America, Inc. (New York: The Dial Press, 1971); Philip M. Stern, The Great Treasury Raid (New York: Random House, 1967); Mark Green, The Closed Enterprise System, Ralph Nader's Study Report on Antitrust Enforcement (New York: Grossman Publishers, 1972); Fred R. Harris, "The Politics of Corporate Power," in Corporate Power in America, ed. Ralph Nader and Mark J. Green (New York: Grossman Publishers, 1973), pp. 25–44; Theodore Lowi, The End of Liberalism.

³See, for example: Robert A. Dahl, Who Governs? (New Haven: Yale University Press, 1961); Raymond Wolfinger, The Politics of Progress (Englewood Cliffs, N.J.: Prentice-Hall, 1974); David B. Truman, The Governmental Process: Political Interests and Public Opinion, Second Edition (New York: Alfred A. Knopf, 1971). Raymond Bauer, Ithiel de Sola Poole, and Lewis Dexter, American Business and Public Policy (New York: Atherton Press, 1963).

systematic, empirical analyses capable of confirming or denying general propositions. In addition, there is a marked tendency to treat the entire corporate sector as a uniform entity and thus to ignore the potential variations in corporate political influence flowing from variations in the structure of particular industries. Is an industry with a few large firms more powerful politically than an industry of the same size with many smaller firms? Is it true, as former U.S. Senator Fred Harris recently wrote, that "reducing economic concentration by breaking up shared monopolies would also help reduce the political power of big business"?4 Or would this merely increase the number of access points different industries have to government? Regrettably, the existing literature provides few signposts by which to judge the answers to these and other similar questions.

One obvious reason for this lack of research findings is the difficulty of measuring the actual extent of corporate political activity and then evaluating its success. Confronted with this difficulty, students of the subject have been forced back to second-best and third-best methodological approaches. The most common of these is reliance on illustrative examples and juicy case studies, with little effort to be comprehensive or systematic. This has led, as Andrew Scott and Margaret Hunt noted in 1960, to an image of business's political influence studded with success stories.⁵ In more recent years, several more rigorous approaches have been developed, but the three best known - the reputational approach, the positional approach, and the decision-making approach have themselves been subjected to telling methodological criticisms.⁶ A fourth approach, em-

⁴Fred Harris, "The Politics of Corporate Power," in Nader and Green, Corporate Power in America, p. 39.

⁵Andrew M. Scott and Margaret A. Hunt, Congress and Lobbies (Chapel Hill, N.C.: University of North Carolina Press, 1960), p. 6. Similar criticisms can be found in: Earl Latham, "The Body Politic of the Corporation," in The Corporation in Modern Society, ed. E. S. Mason (New York: Atheneum, 1966); Bernard Cohen, The Influence of Non-Governmental Groups on Foreign Policy Making (Boston: World Peace Foundation, 1959), pp. 218–236; Theodore J. Lowi, "American Business, Public Policy, Case Studies and Political Theory," World Politics, 16 (July, 1964), 677–715; and Samuel J. Eldersveld, "American Interest Groups: A Survey of Research and Some Implications for Theory and Method," in Interest Groups on Four Continents, ed. Henry Ehrmann (Pittsburgh: University of Pittsburgh Press, 1960).

6Each of these three approaches seeks to assess the relationship between economic power and political influence. The "reputational approach," which proceeds by collecting votes on community influentials from a panel of experts, has been criticized for

bodied in the research seeking to account for variations in public policies among American states in terms of state political and economic characteristics, has escaped some of the methodological problems of the earlier approaches, but has so far utilized indicators of economic life that focus on general *levels* of economic activity (per capita income, degree of industrialization, etc.) rather than on the politically more relevant phenomenon of economic structure.⁷

focusing on presumed rather than actual power, and for beginning with a self-fulfilling prophecy in positing the existence of a power structure at the outset. The "positional approach," which focuses on the backgrounds and characteristics of the persons who hold the positions purported to be the most powerful in the society, has been faulted for confusing formal authority with, the actual exercise of power. And the "decision-making approach," which concentrates on the actors involved in the making of important decisions, has been called into question for its case specific nature and its neglect of those dimensions of power involved in keeping subjects off the agenda of political decision making altogether.

For classic examples of these three approaches, see, respectively: Floyd Hunter, Community Power Structure: A Study of Decision Makers (Chapel Hill, N.C.: University of North Carolina Press, 1953); C. Wright Mills, The Power Elite (New York: Oxford University Press, 1956); G. William Domhoff, Who Rules America? (Englewood Cliffs, N.J.: Prentice-Hall, 1967); and Robert Dahl, Who Governs? (New Haven: Yale Uni-

versity Press, 1961).

For a discussion of these approaches, see: Robert Dahl, "A Critique of the Ruling Elite Model," The American Political Science Review, 52 (June, 1958), 463–469; Raymond Wolfinger, "Reputation and Reality in the Study of Community Power," American Sociological Review, 25 (October, 1960), 636–644; Nelson Polsby, Community Power and Political Theory (New Haven: Yale University Press, 1963); Peter Bachrach and Morton Baratz, "The Two Faces of Power," American Political Science Review, 56 (December, 1962), 947–952; Raymond Wolfinger, "Nondecisions and the Study of Local Politics," American Political Science Review, 65 (December, 1971), 1063–1080; Richard Merelman, "On the Neo-Elitist Critique of Community Power," American Political Science Review, 62 (June, 1968), 451–460.

7Thomas R. Dye, Politics, Economics, and the Public: Policy Outcomes in the American States (Chicago: Rand McNally, 1966); Richard Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables, and Welfare Policies in the American States," Journal of Politics, 25 (May, 1963), 265-289; Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," American Political Science Review, 63 (September, 1969), 959-966; Ira Sharkansky and Richard Hofferbert, "Dimensions of State Policy," in Politics in the American States: A Comparative Analysis, Second Edition, ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1971). Sharkansky and Hofferbert have been most comprehensive in their treatment of economic variables, examining no fewer than 15 separate variables. Though they include items as diverse as value added

What is needed, clearly, is an approach to the study of corporate political power that avoids the shortcomings of the anecdotal and case study approaches through reliance on systematic empirical techniques, but without sacrificing attention to the politically meaningful dimensions of economic structure. At a minimum, this requires work at two levels: first, at the theoretical level, to analyze the concept of economic structure and identify those aspects most likely to affect the exercise of political influence; and second, at the empirical level, to formulate ways to test hypotheses about the relationship between various aspects of economic structure and the successful exercise of political power.

This article seeks to take at least a modest step toward this goal by formulating a set of hypotheses about the likely impact of various aspects of economic structure on corporate political power, and then testing these hypotheses by assessing how well they are able to account for variations in effective federal corporate income tax rates and state excise tax rates. Central to the approach elaborated here is the notion that whatever relationship exists between economic structure and political influence in general should be evident in variations among industries in securing public policies favorable to them. Two basic questions thus define the main thrust of our inquiry: first, are variations among industries in securing favorable public policies systematically related to the economic structure of the industries? And second, what proportion of the variation in policy can be so explained?

To answer these questions, the analysis is divided into two parts. In the first section, we examine the features of the political system that seem to create opportunities to translate economic power into political influence, and then identify five basic aspects of economic structure that we hypothesize will affect the success an industry has in taking advantage of these opportunities. In the second section, we undertake two empirical tests of these hypotheses, one focusing on federal corporate tax rates and the other on variations in state excise tax rates, especially motor fuel excise rates.

In undertaking a systematic, empirical test

by manufacturing and the number of telephones per 1,000 population, however, they do not include variables measuring the structure of different industries that would make it possible to test hypotheses about the impact industry structure (e.g., firm size, market concentration) has on political influence. Yet these are the hypotheses most often generated by students of the economic aspects of political power.

of the economic structure/political influence hypothesis using cross-sectional data and regression techniques, we do not intend to dismiss the existing case study material or deny its importance. To the contrary, as will become readily apparent, this material has provided the crucial insights upon which our hypotheses are based. The contention here, therefore, is not that the case study literature is wrong or irrelevant, but rather that this approach has taken us about as far as it can, and that the field is now ripe for more rigorous hypothesis testing. While we make no claim that the tests we have devised are in any sense comprehensive or conclusive, we believe that they throw some interesting, new empirical light on a question that has been treated far less systematically in the past; and that, in the process, they may suggest some fruitful new ways to explore the important question of the political implications of economic structure more generally.

Industry Structure and Political Influence: The Theory

In general terms, two broad sets of factors shape the impact that an economic sector has on public policy: first, the nature of the political system; and second, the structure of the economic sector itself. The first of these helps define the political system's susceptibility to economic influences; the second helps determine the ability of a particular industry to take advantage of that susceptibility. While other factors — like skill, luck, timing, and leadership — also play important roles, these two set the broad parameters of the relationship between economic and political power, and therefore offer the most fruitful subjects for systematic analysis. Let us examine each in turn.

The Political Process. Perhaps the defining characteristic of the American political system is its permeability to outside pressures. Through voting, congressional testimony, campaign finance, and a host of additional means, the political system offers relatively easy access by citizens to the central policy-making process on a regular basis. While this relative permeability raises the possibility of broad public control of governmental policy à la pure democratic theory, however, it also raises the paradoxical possibility of translating disproportionate economic power into disproportionate political influence in a way that can frustrate broad public control. Three aspects of the political system in particular facilitate this translation.

The first of these concerns the maldistribution of incentives for political action, what might be termed the "free rider problem." As numerous democratic theorists have noted, political involvement is, like any other investment, contingent upon the expectation of a reasonable rate of return on invested resources. But since each individual consumer-taxpayer (or small firm) typically bears only a small portion of the costs and enjoys only a small share of the benefits from public programs, he rarely has the incentive to spend the energy, time, and resources needed to influence the policy. For other political actors, like large corporations, however, the potential benefits and costs of government action are sizeable enough to make political involvement a rational investment. The consequence is a gross disparity in the incentives for political involvement that works to induce the citizen-taxpayer toward passivity while stimulating the large corporation toward political activism. In his essay on The Logic of Collective Action, Mancur Olson provides a general theory of political activism in just these terms, arguing that groups with larger numbers can be expected to be less effective in securing collective goods than those with fewer members because

a collective good is, by definition, such that other individuals in the group cannot be kept from consuming it once any individual in the group has provided it for himself. Since an individual member thus gets only part of the benefit of any expenditure he makes to obtain more of the collective good, he will discontinue his purchase of the collective good before the optimal amount for the group as a whole has been obtained. In addition, the amounts of the collective good that a member of the group receives free from other members will further reduce his incentive to provide more of that good at his own expense. Accordingly, the larger the group, the farther it will fall short of providing an optimal amount of a collective good [emphasis in original].8

Not only does Olson's theory suggest a difference in incentives for political activism between consumer-taxpayers and the corporate sector generally, however, it also suggests a basis for expecting variations in incentives within the corporate sector itself because of differences in the economic structure of different industries. It is this aspect of the free rider concept we will test below.

Not only are the *incentives* for political involvement grossly disparate, but so is the distribution of politically relevant *resources*. Large-scale corporate enterprises have impor

⁸Mancur Olson, The Logic of Collective Action, Public Goods and the Theory of Groups (Cambridge Mass.: Harvard University Press, 1965), p. 35. tant political advantages by virtue of their control over sizeable quantities of several crucial political resources: money, expertise, and access to government officials. These resources are frequently direct outgrowths of the enterprises' regular economic activities, which, in the age of the "new industrial state," frequently put the enterprise in intimate contact with scores of government officials on a day-to-day basis. By contrast, the typical consumer-taxpayer has only one resource regularly at his command: the vote. While the electoral process theoretically gives the mass of unorganized citizens a mechanism to remedy whatever inequities arise from the struggle of organized groups without incurring the costs associated with more direct political action, things rarely work out so smoothly in practice. Indeed, as Dye and Zeigler point out, the tendency to obscure policy issues in campaigns, the difficulty of reading majority policy preferences from elections, and the problem voters have holding elected officials accountable rob the vote of its utility as a determiner of policy.9 As in his role of consumer in the corporate-controlled marketplace, so in his role as voter, the individual citizen must generally take what the prevailing structure of political interests serves up for him. although even the most powerful oligopolist will take care not to push his luck too far.

What makes this maldistribution of incentives and resources for political action so important is the highly fragmented character of the policy process in American government, which plays directly into the hands of those with organized resources focused on particular policy issues. Part of this fragmentation is dictated by constitutional provisions establishing a government of "separated institutions sharing powers." 10 But part of it grows out of the more informal arrangements these institutions have developed to process their workload. The classic example of this fragmentation is the pervasive decentralization of power in the U.S. Congress. Long ago legislators discovered that they could hold their own in the interinstitutional donnybrook that is the American political system only by channeling legislative business through specialized committees run by politically independent legislators with long tenure in the Congress. In pursuit of institutional viability, therefore, decision-making au-

⁹Thomas R. Dye and Harmon Zeigler, *The Irony of Democracy* (Belmont, Calif.: Wadsworth Publishing Co., Inc., 1970), p. 149.

¹⁰Richard Neustadt, *Presidential Power*, Signet Edition (New York: The New American Library, 1964), p. 42.

thority was fractionated into numerous relatively autonomous chunks.

What is not so clearly understood, however, is that the same fragmentation of authority that frustrates effective overall policy direction in Congress operates as well in the executive branch. Despite the deceptive symbolism of a single Chief Executive giving direction to the executive establishment, the federal bureaucracy is really a bewildering smorgasbord of institutional types with varying degrees of autonomy. Rather than unified administrative structures. Cabinet-level departments frequently resemble loose collections of warring fiefdoms, only nominally subservient to a common sovereign. Add the numerous government corporations, the quasi-independent regulatory agencies, the special commissions, the advisory committees, the singleheaded and multiheaded agencies, the institutions and institutes, and the interagency committees, and the impression of unity and consistency disappears like fog under the morning sun.11

What produces this administrative fragmentation is the bureaucrat's need for succor in a political system that looks somewhat askance at his very existence. Since Congress is the ultimate source of agency funds and authority, it is only natural that agencies should turn first to Congress for support. But since power is fragmented in Congress, it quickly becomes so in the agencies because committee chairmen typically arrange agency structure to facilitate their own control. ¹³

Agency reliance on congressional committees and their chairmen is supplemented, however, by agency dependence on client groups, particularly those with influence on the congressional committees. As one long-time government official recently noted: "Private bureaucracies in Washington now almost completely parallel the public bureaucracies in those program areas where the federal government contracts for services, regulates private enterprise, or provides some form of financial assistance." 14

The consequences of this pattern of governmental decision making are profound. Instead

of a single integrated policy process, what emerges is a series of individual, and largely independent, "policy subsystems" linking portions of the bureaucracy, their related congressional committees, and organized clientele groups in a symbiotic state of equilibrium. 15 The key to policy-making power, therefore, is access not to the political system generally but to these well-insulated and highly structured subsystems. Voter-taxpayers can occasionally gain access to the overall political system. Effective access to the subsystems, however, is typically confined to those with the expertise, resources, and influence to provide stable sources of support to key subsystem actors over extended periods of time.

Further complicating the job of the activist citizen is the predominance of administrative over legislative decision making in modern government. The theory of separation of powers notwithstanding, the bureaucracy performs crucial legislative and judicial as well as administrative functions in American government. The vast bulk of legislation considered by Congress originates in the bureaucracy, and it is the bureaucracy that makes the most potent input into the legislative process thanks to its control of information. In addition, statute law (law passed by Congress) is overwhelmed by the far greater volume of administrative law produced by the bureaucracy in the course of interpreting congressional intent. As a former commissioner of the Federal Communications Commission has put it:

While the Courts handle thousands of cases each year and Congress produces hundreds of laws each year, the administrative agencies handle hundreds of thousands of matters annually. The administrative agencies are engaged in the mass production of law, in contrast to the Courts [and Congress], which are engaged in the handicraft production of law. 16

This predominance of administrative rule-making puts a premium on technical expertise and on the ability to follow the complex evolution of administrative decision making. Far from expanding presidential power per se, the expansion of administrative rule making really contributes to the influence of the private subsystem actors who alone have the resources to retain Washington legal counsels and research staffs to analyze relevant agency decisions and respond to them effectively. The very structure of

¹¹ For an excellent analysis of the diversity and political consequences of federal administrative fragmentation see Harold Seidman, *Politics, Position and Power* (New York: Oxford University Press, 1970).

¹² Lester M. Salamon and Gary L. Wamsley, "The Federal Bureaucracy: Responsive to Whom?" in People vs. Government: The Responsiveness of American Institutions, ed. Leroy Riselbach (Bloomington: Indiana University Press, 1975), pp. 151–188.

¹³ Seidman, Politics, Position, Power, pp. 37, 40.

¹⁴Ibid., p. 18.

¹⁵On the concept of "policy subsystems," see J. Leiper Freeman, *The Political Process* (New York: Random House, 1965); Lee J. Fritschler, *Smoking and Politics* (New York: Appleton-Century Crofts, 1969).

¹⁶Ouoted in Lee J. Fritschler, Smoking, p. 94.

governmental decision making, like the maldistribution of incentives and resources, thus facilitates the translation of economic power. into political influence.

Economic Structure and the Political Process. If the maldistribution of incentives and resources for political action, and the structure of the policy process facilitate the translation of economic power into political influence, the economic structure of particular industries helps determine which ones are in the best position to take advantage of this opportunity. But which aspects of economic structure are most germane? What types of differences among industries are most relevant in explaining variations in their levels of political influence?

To answer these questions, it is necessary to turn to the literature on industrial organization and to evaluate this literature in the light of political science findings about the effectiveness of various corporate pressure group tactics.¹⁷ What emerges most clearly from such an evaluation is the hypothesis that firm size, the average size of firms in an industry, is the most important aspect of economic structure so far as the implications for political influence are concerned. Much of the concern about conglomerates and rising aggregate concentration in the American economy, in fact, grows out of the fear that large firms wield not just disproportionate economic power, but also disproportionate political influence, 18 for a variety of reasons. In the first place, to the extent that public policies deliver benefits proportional to the size of the firm (which is the case, for example, with the income tax system), then the larger the firm the greater the incentive to participate in politics, for reasons suggested by

17We are indebted here to Edwin Epstein, *The Corporation in American Politics* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969), particularly pp. 67-111 for an excellent review of patterns of corporate political involvement.

18See, for example, Carl Kaysen, "The Corporation: How Much Power? What Scope?" in The Corporation in Modern Society, ed. E. S. Mason (Cambridge, Mass.: Harvard University Press, 1959), p. 99; Kaysen asserts: "The market power which large absolute and relative size gives to the giant corporation is the basis not only of economic power but also of considerable political and social power of a broader sort." According to Senator Fred Harris: "In any area, the size of a big corporation can be translated into political strength." Harris, "Politics of Corporate Power," p. 39. Ronald H. Coase writes that "if policy is to be based on 'fear of size,' it is surely desirable to discover what is really feared, whether it results from size, and whether this comes about in all circumstances or only in some" [Congressional Record (June 16, 1969)].

Olson. But larger firms not only have greater incentives to participate in politics, they also have larger resources with which to do so. Kaysen¹⁹ and Edwards²⁰ have both suggested that economic power is an important source of political power, either directly, through campaign finance, or indirectly, through the purchase of expertise to generate information instrumental in the policy process and liaison agents to transmit this information to the relevant policy makers. A National Industrial Conference Board study found that large firms tended to represent their legislative views more frequently than did smaller firms.21 By the same token, Bauer, Poole, and Dexter found that firm size is an important determinant of the political activity of executives, since the executives of large firms could afford the luxury of hiring staffs and taking the time to inform themselves about policy issues.22

What makes the absolute size of available resources, and hence firm size, so important politically is the fact that political involvement has certain fixed costs attached to it (we will call this the "threshold problem"). Like a single pain-reliever commercial, corporate political activity is likely to produce little payoff unless sustained over a period of time and spread over a variety of policy-making arenas. Since small firms can rarely sustain these high fixed costs, they are generally constrained to channel their political involvement more extensively through trade associations, with all the intra-organizational differences, lack of control, and consequent weakening of influence that carries with it.²³

Access to economic resources is not the only political asset arising from larger firm size. Also important is the prestige typically accorded

¹⁹Kaysen, "The Corporation."

²⁰Corwin D. Edwards, "Conglomerate Bigness as a Source of Power," Business Concentration and Price Policy (Princeton, N.J.: Princeton University Press, 1955), pp. 331-352.

National Industrial Conference Board, The Role of Business in Public Affairs, Studies in Public Affairs, No. 2 (New York: National Industrial Conference Board, Inc., 1968), p. 8.

²²Bauer, Poole, and Dexter, American Business and Public Policy, p. 227.

²³Commenting on the relative advantages of having a full-time firm representative in Washington and working through a trade association, Corwin Edwards notes: "While some smaller business interests make a comparable showing through associations set up for the purpose, the experience of a Washington official is that small companies generally find out what is happening too late and prepare their case too scantily to be fully effective where their interests conflict with those of large companies." Edwards, "Conglomerate Bigness," p. 347.

individuals who reach the command posts of the larger corporate enterprises, a prestige that is transferable into political influence through the numerous advisory committees and personal contacts that bind the worlds of business and government together. Equally important is the larger pool of expertise potentially available to the larger firms, and, on some occasions, the larger number of employees and stockholders who can be mobilized (or threatened to be mobilized) for political action. Other things being equal, therefore, we hypothesize that an industry containing large firms will have greater political influence than an industry of the same size but composed of more numerous small firms. At the same time, we expect this influence to be mitigated somewhat by the visibility of large firms and the consequent dangers they face of attracting adverse public attention.

To assess the impact of firm size on political power, however, it is necessary to control for the effects of industry size, for the obvious reason that the size of the firm may really be a function of the overall scope of the industry. But industry size also probably affects political power in its own right as well, independently of the size of firms within the industry. If the average firm size is held constant and the overall industry size increases, the number of firms in an industry must increase. Under Olson's thesis, this should be associated with a reduction in the political effectiveness of the industry, since Olson argues that the incentives for any particular actor to work to secure collective benefits decline as the number of members in the group increases. This would lead us to hypothesize an inverse relationship between industry size and political influence, other things being equal.

A competing thesis can also be found in the literature, however. This thesis holds that large industries, whatever the average size of firms comprising them, will have greater political influence because they will have larger poels of money, talent, client support, contacts, and related resources than smaller industries. Nevertheless, on balance we subscribe to the Olson "free rider" interpretation as a working hypothesis and therefore posit a negative relationship between industry size and political influence.

If firm size and industry size are related to each other, both in turn are likely to be related to a third aspect of economic structure: market concentration, usually defined as the proportion of an industry's sales or assets accounted for by its four (or eight) largest firms. For a given average firm size, an increase in industry

size would most likely be associated with a decline in market concentration. By the same token, for a given industry size, an increase in typical firm size will likely be accompanied by an increase in market concentration. Our firm size and industry size variables could thus inadvertently be measuring market concentration. This is important because there is reason to expect that market concentration also affects political influence independently of the effects of firm size and industry size. In the first place, market concentration typically yields higher profits²⁴ that can be channeled into political activity. A more competitive industry of roughly the same size would be likely to earn fewer profits and hence lack some politically relevant resources that might be available to it if it were more highly concentrated. More concentrated industries also have the advantage of avoiding the debilitating and time-consuming process of negotiating industry positions on political matters among numerous competing firms, a process that has frequently been cited as a major obstacle to trade association political effectiveness.²⁵ In eliminating competitors in the economic marketplace, highly concentrated industries also eliminate competition in framing industry positions in the political marketplace. In addition, the more concentrated the industry, the less severe the "free rider problem" it encounters, since firms that dominate an industry receive a larger share of whatever benefits investment in political activity brings. Hence they have a greater incentive to make this investment.

Partially counter-balancing these political advantages of economic concentration, however, are some liabilities. For example, concentrated power, particularly when combined with large firm size, is likely to enhance corporate visibility. As Bauer, Poole, and Dexter put it: "A business can be too big to be politically effective along some lines ... nowadays many

²⁴For an excellent survey of the evidence relating market concentration to profit rates see Leonard W. Weiss, "Quantitative Studies of Industrial Organization," in Frontiers of Quantitative Economics, ed. M. D. Intrilligator (Amsterdam: North-Holland Publishing Company, 1971), pp. 362-379. Weiss concludes that "practically all observers are now convinced that there is something to the traditional hypothesis... Almost all of the 32 concentration-profits studies except Stigler's have yielded significant relationships for years of prosperity or recessions, though they have depended on a wide variety of data and methods" (p. 371).

²⁵See, for example, Bauer, Poole, and Dexter, American Business and Public Policy, pp. 332-340; and David Truman, The Governmental Process, pp. 156-187.

really big corporations are not eager to dance among the chickens..."²⁶ Closely related to the visibility problem is the "wear out your welcome" problem. When a few firms must carry the entire burden of representing an industry in the public policy-making arena, they run the risk of overdrawing on their account of goodwill with key policymakers, particularly since most policymakers can scant afford the appearance of subservience to a small group of firms. Nevertheless, on balance, we believe the presumptive evidence supporting a direct relationship between market concentration and political power is the more persuasive for hypothesis-framing purposes.

To test the independent effect of market concentration on political influence, however, it is also important to control for the effects of profit rate differentials given the evidence we have just cited about the relationship between profit rates and concentration. But profit rate is also likely to affect political influence independently of market concentration because of the link between profitability (whether caused by concentration or not) and the availability of resources necessary for political effectiveness. What is more, highly profitable firms have more to gain from efforts to alter policies such as the federal income tax which place a larger absolute burden on high-profit firms.

At the same time, however, the political efforts to achieve these favorable policies may produce adverse consequences that make them not worth the price. For example, firms earning higher than average profits may shy away from political action if they fear that such action would attract public attention to the monopoly position that yields them these large profits. By the same token, extensive political pressure tactics could hinder pursuit of the kinds of corporate goals stressed by the "new theories of the firm" — goals that go beyond profit maximization to focus on managerial prestige, sales revenue maximization, or "satisficing" behavior.²⁷ On balance, we favor the first of

these two sets of explanations and thus expect a positive relationship between profit rate and political influence.²⁸

A fifth aspect of economic structure that seems likely to influence the level of political success of an industry is its degree of geographical dispersion. With respect to this aspect of economic structure too, however, opinions differ markedly about the consequences for political impact. If we assume that each member of Congress has many constituent groups in his district and that he devotes attention to their desires in proportion to their likely impact on his re-election, then it follows that an industry must be sufficiently large in order to attract the attention of its legislative representatives. For a given firm size and industry size, industries will be more successful in attracting this attention to the extent that they are geographically concentrated. As one participant in a Brookings Institution round table put it: "I can't think of anyone who has more of an impact on a congressman than a representative of a corporation having a big plant in his area."29 Presumably, if the same industry's plants were more dispersed, and hence smaller. its ability to secure the support and attention of its local representatives would be reduced.

A competing hypothesis with respect to geographical concentration is that highly concentrated industries lack the broad-based political support necessary to pass legislation and have it implemented. As Senator Hart has noted, "when a major corporation from a state wants to discuss something with its political representatives, you can be sure he [sic] will be heard. When that same company operates in thirty states, it will be heard by thirty times as many representatives." Given this reasoning,

²⁶Bauer, Poole, and Dexter, American Business and Public Policy, p. 266.

²⁷It has been proposed that firms sometimes do not maximize the attainment of some well-defined objective function, but rather "satisfice." That is, they seek choices which satisfy at least minimum levels of aspiration with respect to a variety of objectives. For example, management may set a target rate of return on invested capital as its profit objective, and if that target is achieved, it turns to the satisfaction of other nonprofit objectives. See Herbert Simon, "Theories of Decision Making in Economics and Behavioral Science," American Economic Review, 49 (June, 1959), 253–283; Robert F. Lanzillotti, "Pricing Objectives in Large Companies," American Economic Review, 48

⁽December, 1958), 921-940; William Baumol, Business Behavior, Value and Growth, rev. ed. (New York: Harcourt, Brace and World, Inc., 1967); Oliver Williamson, The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964).

²⁸The introduction of a control for profit rate will also probably change the expected direction of the relationship between market concentration and political influence. This is so because the positive political impact attributed to concentration was based largely on the expected higher profits of concentrated industries. With this factor controlled for, the visibility and "wear out your welcome" problems may dominate the relationship. Under these circumstances, we should expect an inverse relationship between market concentration and political influence.

²⁹Paul W. Cherington and Ralph L. Gillen, *The Business Representative in Washington* (Washington, D.C.: The Brookings Institution, 1962), p. 55.

³⁰Senator Phillip Hart (D-Michigan), Speech to the Lawyers Club in Ann Arbor, Michigan, April 8, 1968,

dispersed industries would be more effective than concentrated ones. On balance, however, we find the first argument more convincing, and posit an inverse relationship between geographical dispersion and political influence once we have controlled for firm size and industry size.

Altogether, therefore, we have identified five dimensions of economic structure that appear, from the available literature and a priori reasoning, to be likely to affect an industry's level of political influence. Other dimensions of economic structure - such as level of unionization of firms, capital-output ratio, etc. - could also be considered, but we believe these five are the most promising to explore. In the case of at least four of them, moreover, they are significantly interrelated. Table 1 below depicts this fact, showing the correlations among these five dimensions of economic structure for the one hundred and ten industries identified in the Internal Revenue Service's "minor industry" classification of mining and manufacturing firms.31

cited in Mark J. Green, *The Closed Enterprise System*, p. 19. Senator Fred Harris concurs, noting in a recent article that: "A company with plants in ten congressional districts can expect more congressional champions than a company with plants in only five districts." Fred Harris, "The Politics of Corporate Power," p. 39.

31The IRS classifies all economic activity into "major industry" groups. Within each group are further subdivisions, called "minor industries." Firms are classified into "minor industries" on the basis of their predominant product. Of the 129 "minor industries" in mining and manufacturing identified by the IRS, fifteen are "not allocable" categories in the major industry groups. The firms in these categories usually do not provide sufficient information to permit the IRS to classify them into a substantive minor industry classification. In general, these industries are small relative to the other "minor industries" in the sample, and the firms in these industries are not likely to have homogeneous political interests. In addition, four other industries were omitted for various reasons that prohibited calculation of their effective average corporation income tax rates, one of the major dependent variables used in the tests

The crucial question to consider, however, is not whether these dimensions of economic structure are related to each other, but whether they are related to the successful exercise of political influence, both independently and as a group. To answer this question, we must formulate suitable indices of the successful exercise of political influence, translate our economic structure variables into measurable form, and test empirically for the impact of the latter on the former. It is to these tasks that we now turn.

Empirical Tests

To date, the few efforts to analyze the relationship between economics and public policy have either neglected to consider economic structure as a variable ³² or have treated this variable through essentially case specific decision-making approaches. ³³ While we have built on these approaches to develop our hypotheses, we have found them inadequate for hypothesis testing, the first because we believe that economic structure is an important variable and that there are ways to measure its politically relevant dimensions; and the second

below. Accordingly, the correlations reported here relate to 110 of the 129 minor industries identified.

³³In his study of New Haven, for example, Robert Dahl evaluates the influence of economic notables by studying a collection of decisions in four key policy arenas in New Haven over a ten-year span. Since these notables participated infrequently in decisions in almost all of the four policy areas, Dahl concludes that economic power is not translated effectively into political power in New Haven. Similarly, Bauer, Poole, and Dexter, in assessing business influence on U.S. trade policy, scrutinized the degree of business participation in the actual policy-making arenas during consideration of trade legislation. In addition, they carefully analyzed the flow of information within the business community and its trade associations concerning foreign trade matters. On both counts, they concluded that business influence was marginal, despite what many would take to be high economic stakes in the issue. Businessmen generally had little knowledge about trade matters, and their lobby

Table 1. Correlations Among Five Dimensions of Economic Structure for 110 IRS "Minor Industries" in Mining and Manufacturing

Variable ^a	F	I	С	R	D
Firm Size (F)	1.000				
Industry Size (I)	.276	1.000			
Concentration (C)	.448	280	1.000		
Profit Rate (R)	.419	.107	.310	1.000	
Geographical Disperson (D)	.120	172	.493	.069	1.000

^aUnits of measurement for each variable are discussed in the text below and summarized in the "measurements" column of Table 2.

³²See footnote 7 above.

because we believe, with Bachrach and Baratz, that the decision-making approach fails to take account of the numerous items that economic notables keep from becoming issues in the first place, and because we feel it is not suited to broad-gauged empirical testing.

The approach we have adopted, therefore, is to formulate a measure of the successful exercise of political power that can be used to generate an index of political effectiveness for all industries, and that can be related to indices of economic structure. Accordingly, we measure power, in Bertrand Russell's terms, as "the production of intended consequences." In other words, what we propose to do is to measure the success with which an industry exerts political influence in terms of the extent to which policies emerge that benefit it.

This approach is similar to the one used in economic analyses of firm performance, where relationships between firm structure and performance are measured without examining the intervening variable of firm conduct. This approach is used by economists because of the near-impossibility of quantifying the conduct variable.35 Clearly, the same problem obtains in dealing with the intermediate variables in the political influence process. Measuring the actual exercise of political influence - lobbying, campaign contributions, informal contacts, and so forth – across numerous industries is highly sensitive to the gross imperfections in the information reporting requirements and the resulting glaring gaps in data. Under the circumstances, systematic empirical testing of the economic structure/political influence hypothesis can only occur through reliance on the structure/performance paradigm of economic analysis. At the very least, it should be fruitful to explore what contributions this paradigm can make to political analysis as well.

In choosing which policy outputs to employ as our measures of the successful exercise of power, we searched for those that were not only substantively important but also susceptible to empirical treatment and broad in their coverage. Tax policies thus emerged as the leading contenders. In particular, we focused on federal income tax policy and state excise tax policy. Both of these are universal in their

application yet varied in their impact, thus offering an opportunity for systematic analysis. What is more, both are cases of what Theodore Lowi has termed "redistributive" policy arenas, which typically involve substantial class conflict and opposition to economic elite dominance.36 To the extent that we identify a link between economic structure and the successful exercise of power in these arenas, therefore, we will have established an even more dramatic confirmation of the economic structure/political influence hypothesis than would be the case if we were looking only at a "regulatory" arena. Even so, readers should be cautioned that our findings in these two arenas may not be generalizeable to other policies and that we make no claims about the representativeness of our dependent variables. With these caveats in mind, we can now turn to the empirical tests themselves.

Federal Corporate Income Tax Rates: Empirical Test I. Under the federal income tax laws in effect at the time for which we collected our data, all firms were required to pay 52 per cent of their taxable income in taxes. However, the Internal Revenue Code includes numerous "special" tax provisions (such as the investment credit, the special capital gains rate, the surtax exemption for income under \$25,000, the depletion allowance, etc.) that reduce the effective tax rate below this 52 per cent level. For each of the 110 "minor industries" for which tax data were available, we computed the effective average tax rate produced by these special tax provisions.37 Our dependent variable, then, is the deviation of this effective tax rate from the 52 per cent rate. This "tax avoidance rate" measures the success with

36Actually, Lowi is inconsistent in characterizing the appropriate category for tax policy, referring to disputes over taxes at one point as classic examples of "distributive politics," and at another point as classic examples of "redistributive politics." Our dat may provide an empirical basis for deciding which of these views is most accurate, for to the extent that we can explain variations in tax impact in terms of industry economic structure, we will have discredited the view that tax advantages are distributed among industries on something resembling the first-come-first-served basis of a distributive arena. See: Lowi, "American Business, Public Policy," pp. 705–707.

³⁷The effective average tax rate is the ratio of actual tax liabilities to estimated "true" accounting net income, which is the industry's reported income plus the amount it was able to exclude from its reported income because of the special provisions in the Internal Revenue Code. For a detailed description of the effective average tax rate see John J. Siegfried, "Effective Average U.S. Corporation Income Tax Rates," National Tax Journal, 27 (June, 1974), 245–259.

organizations proved incapable of seriously influencing consideration of the policy.

³⁴Bertrand Russell, *Power: A New Social Analysis* (London: Allen & Unwin, 1962), p. 25.

³⁵For a clear statement of this approach, and of the argument for it, see Joe S. Bain, *Industrial* Organization (New York: John Wiley and Sons, Inc., 1968), p. 329.

which an industry has reduced its tax burden below the standard 52 per cent rate thanks to the operation of the "special provisions" in the code. As shown in Appendix Table 1, the tax avoidance rate varied from a high of 35.8 per cent for the Miscellaneous Nonmetallic Minerals industry to a low of 3.9 per cent for the Newspaper industry.

Based on our earlier discussion, we hypothesize that the size of the Tax Avoidance Rate is a function of an industry's economic structure, as reflected in its firm size, industry size, level of market concentration, profit rate, and degree of geographical concentration. The equation below expresses this model succinctly:

$$TAR = 0.52 - t = f(F,I,C,R,D)$$

where

TAR = tax avoidance rate

t = the effective average corporation income tax rate

F = firm size (millions of dollars)

I = industry size (number of employees)

C = market seller concentration (percentage points)

R = the rate of "true" accounting profit plus interest on total assets (percentage points)

D = geographical concentration (index number based on employment)

In positing a relationship between economic structure and the Tax Avoidance Rate, of course, we are not claiming that this is the only explanation of variations in tax avoidance rates among industries. It is conceivable that many of the "special" tax provisions were instituted on the basis of sound public policy, independent of the influence of interested parties. Alternatively, the effect of some of the laws and rules on particular industries may be "flukes," instituted either in error or as unnoticed sideeffects of other well-designed policy decisions. Neither of these explanations is ruled out by our model. What our model will tell us, however, is whether any portion of the variation in Tax Avoidance Rates can be attributed to variations in political influence flowing from variations in economic structure, and, if so, which aspects of economic structure are most important.38

38These opposing arguments about the causes of variations in tax policy are nicely summarized by Louis Eisenstein, who characterized the tax policy process as "a continuing struggle among contending interests for the privilege of paying the least." According to Eisenstein, to view the tax policy process in any other way is just a "pleasing illusion." Louis Eisenstein, The Ideologies of Taxation (New York: The Ronald Press Company, 1961), pp. 3-4. See also

To test the relationship between economic structure and Tax Avoidance Rates, we employed multiple regression techniques.³⁹ Throughout the analysis, we rely on the Internal Revenue Service's Statistics of Income and the Bureau of the Census's Census of Manufactures for 1963 to measure both our independent and dependent variables. The year 1963 was used because of the unavailability of data to measure some of the independent variables in later years. For example, the 1967 Census of Manufactures failed to provide sufficient detail on the location of manufacturing plants to compute the geographical dispersion variable, and the necessary 1972 data were not yet published at the time we conducted our research. In addition, certain of the computations that went into the derivation of the effective average corporate tax rate for each industry relied on Treasury studies completed in the 1950s, and few of these have been updated. For all these reasons, 1963 data were most appropriate. Since we are more interested the existence of the relationships hypothesized than in their size at some particular point in time, moreover, this use of 1963 data is less serious than it might otherwise be, especially given our expectation that these relationships are likely to be fairly durable.

To test our hypotheses about the political impact of variations in economic structure, we converted each of our dimensions of economic structure into measurable terms and tested its impact on the Tax Avoidance Rate, using multiple regression techniques to control for the impact of the other factors in the model. Table 2 below summarizes the results of this analysis, along with the measurement used for each variable, and the relationship hypothesized on the basis of our theory. (Appendix Table 2 records the simple correlation between each of our independent variables and the Tax Avoidance Rate.)

Firm Size: The first thing that emerges from

Stanley S. Surrey, "The Congress and the Tax Lobbyist: How Special Tax Provisions Get Enacted," Harvard Law Review, 70 (May, 1957), 1145-1182.

³⁹To determine the proper functional form of the hypothesized relationship, we employed the Box-Cox maximum likelihood method, which suggested that a multiplicative (nonlinear) form of our equation was most appropriate. For further discussion of this technique, see Paul G. Hoel, Introduction to Mathematical Statistics (London: John Wiley and Sons, Inc., 1962), pp. 220–225. In a multiplicative model, the reported coefficients are interpreted as the percentage change in the dependent variable produced by a one per cent increase in the value of the independent variable.

this table is that the hypothesized relationship between firm size and political influence, which formed the heart of our inquiry, is supported by the data. As noted in the table, firm size was measured by taking the asset size of that firm which accounts for the median asset dollar in an industry. When this factor was tested for its effect on the corporate Tax Avoidance Rate, while controlling for the effects of our four other measures of economic structure, the results were statistically significant and in the expected direction. A one per cent increase in firm size was found to be associated with a .08 per cent increase in the Tax Avoidance Rate. Even though it is difficult to evaluate the practical importance of this degree of change, it is nevertheless significant to have validated this hypothesis empirically across so large a number of observations.

Industry Size: Unlike the firm size variable, the industry size variable seems to have little significant impact on the Tax Avoidance Rate. Industry size was measured in terms of total employment, which allowed us to tap the political influence represented by employee dependence on the industries that employ them. As noted, we had theoretical reasons to expect a positive relationship between industry size and tax avoidance, on grounds that larger industries had more resources with which to overcome the threshold problem and greater numbers of potential supporters among voters. But we also had reason to expect a negative relationship, on the grounds that larger industries would encounter the "free rider" problem more severely. Therefore, we subjected this hypothesis to a more rigorous, two-tailed significance test. The results suggest that the "free rider" hypothesis is the more accurate one -afinding that makes sense in light of the many competing interests that vie for the voter patronage of industrial workers. On tax matters in particular, it seems more likely that employees will support a position contrary to that of management, especially if they are unionized, since unions recognize that taxes not paid by business will result either in a reduction of public services or an increase in other taxes. But the observed negative relationship, though consistent with our earlier hunch, is too weak to give us more than 90 per cent confidence that it did not occur merely by chance.

Market Concentration: In the case of market concentration, the results show a significant impact on the Tax Avoidance Rate, but in a direction opposite to what we originally expected. Market concentration was calculated on the basis of the proportion of all industry shipments accounted for by the four largest firms in the industry.⁴⁰ We hypothesized that

40To compute this variable for each "minor industry," we employed the concentration ratios provided for the Census Bureau's more narrowly defined Standard Industrial Classification of firms. These concentration ratios for the SIC industries were then aggregated, using a weighted average based on employ-

Table 2. Relationship Between Tax Avoidance Rate and Five Aspects of Economic Structure,
110 IRS "Minor Industries"

Variable	Measurement	Hypothesized Sign ^a	Regression Coefficient	t-ratio
Firm Size	Asset size of firm which accounts for the median asset dollar in an industry	+	.084**	2.42
Industry Size	Employees	+/	095	-1.72
Market Concentration	Per cent of sales accounted for by four largest firms	+/	512**	-2.94
Profit Rate	Net income plus interest as % of assets	+/	301*	-2.46
Geographical Concentration	Index based on distribution of employees across states; higher values indicate more concentration	+/-	017	-0.15

^aA positive sign indicates that we hypothesize a direct relationship between the independent and dependent variables. A positive and negative sign signifies that we have found theoretical reasons to expect either a direct or indirect relationship. In such cases, we subject our results to a two-tailed significance test.

^{**}Statistically significant at the .01 level.

^{*}Statistically significant at the .05 level.

 R^2 (coefficient of determination) = .158

Fisher's F statistic = 3.892*

Standard error of the estimate = .465

Number of observations = 110

market concentration would be associated with a high level of political influence, since concentrated industries could expect to avoid the "free rider" problem more easily than nonconcentrated industries. At the same time, we noted that highly concentrated industries would face some disabilities in exercising political influence as a result of their fears about arousing public attention and what we called the "wear-out-your-welcome" problem. As is apparent from Table 2, this latter argument is the one that finds support in the evidence. A one per cent increase in market concentration is associated with a .512 per cent decrease in Tax Avoidance Rates. One reason for this finding could be that our measure of market concentration does not capture the "free rider" problem as accurately as a simple measure of the number of firms in an industry, which is already partially accounted for in the industry size variable. Indeed, with the "free rider" and higher profit rate aspects of market concentration factored out (which is what our controls for firm size, industry size, and profit rate essentially do), it is understandable that the independent effect of market concentration on the successful exercise of political power is negative.

Profit Rate: The profit rate variable also turns out to be negatively related to success at tax avoidance. Profit rate is defined here as the ratio of net income plus interest to total industry assets, where net income is calculated by taking reported taxable income and adding to it the items excluded from reported taxable income thanks to the special tax provisions. On the basis of the available literature, we suggested that industries with higher profits would be most effective politically because of their access to more substantial resources and the greater incentive they would have to reduce taxes on their profits. Yet, we also noted that such industries had a strong incentive to abstain from efforts to lower their effective average tax rates for fear that extensive political involvement might draw attention to the favorable market environment responsible for their high returns and thus invite government regulation. In addition, we suggested that such firms, having achieved their profit objectives, might be pursuing other objectives that are inconsistent with the pursuit of political influence.

The significant negative relationship between profit rate and Tax Avoidance Rate recorded in Table 2 suggests that these latter arguments are

more accurate. Profit rate does have a significant impact on the successful exercise of political influence as measured here, but for reasons associated with the favorable-market-environment-more-to-lose hypothesis, not the higher-profits-more-resources-more-to-gain hypothesis.

Geographical Concentration: Of all the measures of economic structure used here the geographical concentration factor turns out to have the least discernible impact. Geographical concentration was measured by an index based on the distribution across states of employment in each of the 110 Census-defined industries.⁴¹ The more concentrated the employment of the industry in a few states, the larger the concentration index.

We hypothesized that since a highly concentrated industry would have the greatest success in attracting the attention of at least one set of legislators, it could be assured of having its interests represented in the political process. Here, too, however, an alternative hypothesis also made sense, namely that the more dispersed an industry the more access it could expect to have in the political process. The data reported in Table 2 do not provide sufficient evidence to favor either argument. However, it could be that both factors are at work, but that their approximately equivalent impacts cancel out in our empirical test.

Taken together, the five dimensions of economic structure incorporated in our model explain 16 per cent of the variation in corporate tax avoidance rates, and do so at a confidence level exceeding 95 per cent. For a model seeking to explain so complex a phenomenon over so large a cross-section of observations, this is an encouraging result. What it suggests is that variations in economic structure do affect the success of various industries in securing public benefits (or avoiding public costs), even though such variations explain only a portion of this success. Beyond this, our findings suggest the feasibility of testing the widely hypothesized link between economic structure and political influence in a more systematic, empirical fashion than has been done in the past. In this regard, our discovery

ment, to derive the concentration ratios for the "minor industries" which the SIC firms comprise.

⁴¹This index consists of the sum (across states) of the squared share of total industry employment located in each state in 1963. An index of this type is sensitive to both the number of states in which an industry has employees and the distribution of employees among these states. Fewer states or a more unequal distribution will cause the index to rise. Squaring the shares weights the large employment states (and hence the more important states) more heavily in the index.

of a positive relationship between firm size and Tax Avoidance Rates is particularly significant, since it suggests an empirical base for the long-standing argument that antitrust policy is necessary to avoid not just undue concentrations of economic power but also threatening concentrations of political power.

This latter conclusion is further substantiated, moreover, when we take the analysis one step further and examine variations in tax avoidance within industries, not just variations among them. For this purpose, we examined the petroleum refining industry in particular, calculating the tax avoidance rates for categories of firms within this industry grouped by asset size.42 Table 3 below records the results of this analysis. What it shows is that the very largest oil refiners, in particular the integrated "majors," have been best able to take advantage of the special tax provisions that affect business in general and the petroleum industry in particular.43 This finding supports the results of our cross-industry analysis reported above. Not only do industries with larger firm sizes have larger tax avoidance rates, but also the larger

42Those firms having less than five million dollars of assets were excluded because many of them showed deficits, and we were not able to remove these effects from the data. Furthermore, firms with less than five million dollars in assets are not representative firms in the petroleum refining industry.

43 For a more detailed discussion of the petroleum industry within the framework developed here, see: Lester M. Salamon and John J. Siegfried, "The Relationship Between Economic Structure and Political Power: The Energy Industry," in Competition in the Energy Industry, ed. Thomas D. Duchesneau (Cambridge, Mass.: Ballinger, 1975).

Table 3. Tax Avoidance Rates (TAR) for Petroleum Refining Industry, 1967

Asset Size Class (\$1,000,000)	TAR ₁	TAR ₂
5–10	14.3	14.8
10-25	0.2	8.2
25-50	14.5	14.5
50-100	13.0	13.2
100-250	34.8	35.4
Over 250	27.9	42.3

TAR₁ = 0.52 - (\frac{\tax liabilities}{\tautrue" accounting profits}),

treating foreign taxes entirely as a substitute for U.S. taxes; foreign tax credit not a "special" provision.

TAR₂ = 0.52 - (tax liabilities - foreign tax credit "true" accounting profits),

treating the foreign tax credit as a "special" tax provision.

firms within those industries do better than the smaller ones.

Before we can have confidence in any of these tentative conclusions, however, it is necessary to determine whether they are peculiar to the federal income tax arena. Accordingly, we turn our attention to an aspect of state policy to determine whether the economic structure/political influence hypothesis finds empirical support there as well.

State Gasoline Excise Tax Rates: Empirical Test II. In assessing the impact of economic structure on state politics and policy, it is necessary to alter the mode of analysis somewhat. Instead of focusing on industry-by-industry variations in policy impact, the focus here must be on state-by-state variations. What we have done, therefore, is to identify a range of policy that exists in all the states, and then examine state variations in that policy in terms of state-by-state variations in the strength and structure of the economic sectors most affected by it.

The motor fuel excise tax is an excellent candidate for this kind of scrutiny. Every state has such a tax, and the tax rate varies significantly from state to state. In twenty-eight of the states, motor fuel excise tax receipts are "dedicated" to highway construction by constitutional amendment. Not surprisingly, major oil firms have not opposed increases in motor fuel excise tax rates in these states, since these taxes construct highways that ultimately help to increase the demand for gasoline. However, in twenty-two of the states, where motor fuel tax receipts are not dedicated to highway construction, the oil companies have generally lobbied against such increases.⁴⁴

Since state policy making has traditionally been considered even more vulnerable than federal policy making to private interest pressure, 45 we expect significant relationships be-

44Information provided by Mr. William Haga, representative of major oil company interests in the State of Tennessee. Since 1971 some major oil companies have taken a position of not opposing gasoline taxes which are used for nonhighway purposes if they are dedicated instead to mass transit purposes. Not all companies currently take this position. Since our empirical test is for 1967, however, this change in policy will not affect our hypothesis. Furthermore, we are seeking to discover evidence of general relationships between economic structure and political decision making, and consequently the relevance of our analysis is not diminished by a change in the policy of some oil companies.

45On the permeability of state legislatures to special interest pleading, see: Terry Sanford, Storm over the States (New York: McGraw-Hill Book Company, 1967).

tween state-by-state variations in petroleum industry economic structure and state motor fuel excise tax policies, both the policies on "dedication" and the policies on tax rates. Of particular concern are the impacts of two dimensions of state petroleum industry structure: first, the overall size of this industry in the state, measured as a share of state personal income; and second, the extent to which large firms dominate this industry within the state. Based on earlier hypotheses about the impact of economic structure on political influence, we hypothesize, first, that states in which the petroleum industry is large will be more likely to dedicate their motor fuel excise taxes to highway construction than is the case for states without large petroleum industries; and second, that among nondedication states - those that do not dedicate motor fuel excise taxes to highways - the motor fuel excise tax rate will vary indirectly with both the overall size of the petroleum industry in the state and the dominance of large firms within the industry. This latter hypothesis, which suggests that motor fuel excise tax rates will be lower where the petroleum industry is both large and dominated by large firms, reflects the industry's opposition to special taxes on its products when the resulting revenue is not tied to highway construction. In the case of the dedication states, where the petroleum industry has taken a generally neutral stance towards proposed increases in motor fuel excise tax rates (because these taxes are earmarked for further highway expenditures that can be expected to boost gasoline sales), we hypothesize little relationship between our two measures of industry structure and motor fuel excise tax rates.

To test these relationships, we developed measures of the overall size of the petrcleum industry in each state and the degree of dominance of large firms within this industry in each state. The first of these measures was computed as the ratio of the value added by the petroleum industry in each state to the personal income of the state. The second was computed as the ratio of the number of firms greater than a fixed employment size to the total number of firms in the industry in each state.⁴⁶ In both cases, we computed our indices separately for the crude oil production and petroleum refining segments of the petroleum industry. This was

46The index was computed for crude petroleum using 100 employees as the cutoff size and for petroleum refining using 250 employees as the cutoff size. These sizes were chosen on the basis of data availability and to provide sufficient variation in the explanatory variables constructed from them. 1967 Census of Manufactures data were used.

done because there is reason to believe refiners are relatively more interested in excise taxes than crude oil producers, both because a relatively greater proportion of refinery products go into motor fuel production and because a much larger proportion of refinery products are consumed at home, in the states where the refineries are located.

To assess the validity of the first hypothesis, i.e., that states with large petroleum industries are more likely to dedicate their motor fuel taxes to highway construction because of the political clout of this industry in state politics, we grouped the states in terms of two factors: whether they contain large or small petroleum industries, and whether or not they dedicate their motor fuel excise taxes to highway construction. The results, reported in Table 4, generally support our hypothesis. Almost fourfifths of the states where petroleum refining accounts for a substantial share of state personal income dedicate their motor fuel taxes to highway construction, compared to only onehalf of the states where the overall size of the petroleum refining industry relative to the overall state economy is small. A similar relationship holds with respect to the crude production industry, but there the disparity is much narrower (60 versus 54 per cent), which is what our hypothesis suggested.

To test the impact of firm size and relative industry size on the level of state motor fuel excise tax rates, a somewhat more complex analysis was necessary. Like other state revenues, gasoline taxes too may respond more to the availability of income or the general effective demand for public services in a state than to oil industry lobbying. In fact, several political scientists have demonstrated a substantial relationship between state income levels and state governmental expenditures.47 Since there was reason to expect that the relative size of the petroleum industry in a state would vary with the size of the state's economy, it was necessary to introduce controls to verify that our economic structure variables are not really proxies for these overall income variables. In particular, we utilized two control variables: first, the ratio of total state governmental revenues to total state personal income (which measures the state's preference for public vs. private consumption and thus the effective demand for revenues); and second, the state's per capita income (which measures the supply of wealth available). Multiple regression techniques were then applied to determine the impact of these economic structure and control

⁴⁷See footnote 7 above.

Table 4. Distribution of States Cross-Classified by Motor Fuel Excise Tax Dedication and Size of Industry in State

Size of Petroleum Industry		r Fuel Taxes to onstruction	Do Not Dedicate Motor Fuel Taxes to Highway Construction		
	Number of States	Per Cent of Row	Number of States	Per Cent of Row	
Petroleum refining					
Large ^a	7	(78%)	2	(22%)	
Small ^b	21	(51%)	20	(49%)	
Crude oil production					
Large ^a	9	(60%)	6	(40%)	
Smallb	19	(54%)	16	(46%)	

^aGreater than one per cent of state personal income.

variables on the level of state excise tax rates for all states, and for dedication and nondedication states separately.

The results of these tests, recorded in Table 5, substantially confirm our hypotheses about the relationship between industry economic structure and the successful exercise of political influence. For all fifty states, the structure of the petroleum refining industry is significantly related to the level of motor fuel excise tax rates in precisely the direction hypothesized: tax rates are lower where the petroleum refining industry is large and where it is dominated by large firms. By contrast, no significant relationships were found between tax rates and crude oil industry structure, which is consistent with our argument that crude oil producers are less interested in motor fuel tax rates than are

refiners.⁴⁸ By the same token, the controls for state tax effort and per capita income showed no significant relationship to motor fuel tax rates, suggesting that the economic structure of

48Several alternative explanations for this lack of relationship might also apply. In the first place, the result might reflect relatively less success at influencing policy on motor fuel tax rates because of lower concentration and the associated higher organizational costs of lobbying even though crude producers have a similar desire for lower rates. Furthermore, the data used to measure the share of activity of this sector in each state also included natural gas production. Although natural gas production and crude production are highly correlated across states, the additional variation produced by the inclusion of natural gas production (which does not go toward motor fuels) may be the source of the absence of an effect of this variable on the gasoline excise tax rate.

Table 5. Relationship Between State Motor Fuel Excise Tax Rates and Petroleum Industry Economic Structure

<u> </u>		Refinin	g Industry	Crude	Oil Industry	Co	ntrols	R ² (Proportion
Regression Model for:	Constant	Overall Size ^a I	Large Firm Dominance ^b	Overall Size ^a	Large Firm Dominance	State Tax Effort ^d	State Per Capita Income	of Variance Explained)
All 50 States	8.537	-0.423* (-2.78)	-1.295* (-2.54)	+0.046 (2.02)		-0.065 (-1.54)	-0.010 (-0.03)	.291
22 Nondedication States	8.290	-0.414 (-0.88)	-1.963* (-2.77)	+0.070 (1.15)		-0.093 (-1.75)	0.251 (0.65)	.454
28 Dedication States	7.543	-0.370 (-0.43)	518 (-0.93)	0.024 (0.17)	3.024* (2.31)	0.044 (0.31)	-0.183 (-0.73)	.283

(Figures shown are partial regression coefficients [B] with t-ratios in parentheses.)

bLess than or equal to one per cent of state personal income.

^{*}Statistically significant at the .05 level.

^aValue added by refining or crude industry in state as a percentage of state personal income.

bNumber of refineries with greater than 250 employees as a percentage of total number of refineries in state.

cNumber of crude oil and natural gas establishments with greater than 100 employees as a percentage of total number of crude and natural gas establishments in state.

d_{Total} state government revenues as a percentage of state personal income.

a state may be more important in explaining significant policy differences than aggregate levels of state economic activity. Altogether, our model accounted for close to 30 per cent of the variation in state motor fuel tax rates among the fifty states, most of it as a result of the impact of the refining industry structure variables.

A comparison of the results for the twentytwo nondedication states to those for the twenty-eight dedication states lends further support to these conclusions. As expected, our economic structure/political influence model is far more successful in explaining tax rate variations in the nondedication states, where the industry lobbied against tax rate increases, than in the dedication states, where it did not. Particularly noteworthy is the operation of our refining industry firm size variable, which is significantly negatively related to the tax rate in the twenty-two nondedication states but not in the dedication states. Evidently, where the large refiners flex their political muscles (the twentytwo nondedication states), they have considerable success. Here is important additional support, therefore, for the firm size/political power hypothesis outlined at the outset of this article.

This finding is even more noteworthy in view of the absence of any statistically significant relationship between motor fuel tax rates and the state per-capita income variable that other researchers have identified as an important determinant of state policy. But these researchers did not control for the economic structure variables we have argued are most relevant politically. Interestingly, the per-capita income variable is not even consistently related to the tax rate in the expected direction. The same is true, moreover, of the state tax effort variable, which, far from having a significant impact on motor fuel excise tax rates, is also not even related to the tax rate in the expected direction in two out of the three tests.

In summary, it appears that states with substantial oil refining industries are more likely to dedicate motor fuel tax revenues to highway construction. Similarly, in those states where no such dedication occurs and where the petroleum industry has consequently made political efforts to keep tax rates low, industry success has been a function of the size of the firms in the industry in the state: the larger the firms, the greater the success in keeping motor fuel tax rates low. The results of this test of the relationship between economic structure and political influence in the area of state tax policy thus lend support to the results of our earlier test in the area of federal income tax policy, especially with regard to the political influence

flowing from large firm size.

Summary and Conclusions

Despite its tentative and partial character, the evidence presented in this study lends empirical credence to the notion that the structure of the American corporate economy has important implications for the operations of the American political system at both state and federal levels. Especially striking are the positive relationships discovered between firm size and industry success in avoiding both federal corporate income taxes and state excise taxes. To the extent that political power is reflected in such actual policy outcomes, we can therefore say that larger firm size does indeed seem to yield greater political power. Also interesting are the findings suggesting that larger industries (as opposed to larger firms) are less successful politically, which supports the "free rider" hypothesis advanced by Mancur Olson, since larger industries in general have more firms than smaller industries. In addition, we found evidence that industries which are most visible and most fearful of government intervention because of their attractive (i.e., concentrated) market structure or profitability are more inclined to avoid (or are less successul at mounting) political influence efforts aimed at reducing their tax burdens.

Beyond these immediate substantive conclusions, however, the work summarized here also has broader methodological and theoretical implications because of what it suggests about the feasibility of a comprehensive empirical approach to analyzing the relationship between economic structure and political power, a relationship that is vital to the future of American democracy. For too long now, political scientists have contented themselves with imperfect case study approaches to this question, ignoring the insights available in the economics literature that make it possible to identify measurable aspects of economic structure and thus to undertake more systematic testing of hypotheses. 49 While we are fully aware of the special strengths of the case study approach and the limitations of our own, we believe that the relationship between economic and political power will not be fully understood until the findings of particular case studies can be com-

⁴⁹ Two recent exceptions to this point, focusing on the trade policy area are: C. P. MacPherson, "Tariff Structures and Political Exchange," Ph.D. Dissertation, Department of Economics, University of Chicago (December, 1972); and J. J. Pincus, "Pressure Groups and the Pattern of Tariffs," Journal of Political Economy, 83 (August, 1975), 757-778.

pared with the results of more comprehensive and systematic empirical research of the sort reported here. Given the importance that an understanding of this relationship holds for the structure of the American political system, and

the implications it has for the shaping of public policies, it is our hope that the analysis offered here will help open the way for the progress on the systematic, empirical side of this research strategy that is so clearly needed.

Appendix Table 1.

1963 Tax Avoidance Rates for Industries with Rates More than
One Standard Deviation from the All-Industry Mean
(Mean = 12.6, Standard Deviation = 6.82)

IRS "Minor Industry" Code	Industry Description	Tax Rate Avo <u>idance</u> (Percentage)	
1498	Miscellaneous Nonmetallic Minerals	35.8	
1100	Coal Mining	33.8	
2912	Petroleum Refining with Extraction	32.2	
3330	Nonferrous Primary Metals	30.5	
3662	Electronic Components	28.2	
1310	Crude Petroleum Natural Gas and Liquids	27.9	
1410	Stone and Gravel	27.6	
2870	Agricultural Chemicals	27.2	
2911 ⁻	Petroleum Refining without Extraction	27.0	
2091	Vegetable and Animal Oils	25.5	
3098	Miscellaneous Plastic Products	23.7	
2410	Logging, Lumber, and Basic Wood Products	21.0	
1020	Copper, Lead, Zinc, Gold, Silver Ores	20.9	
2010	Meat Products	20.2	
3198	Leather Tanning and Finishing	19.5	
2070	Confectionary Products	5.8	
3530	Construction, Mining, Material Handling Machinery	5.6	
3611	Electrical Transmission and Distribution Equipment	5.6	
3260	Pottery	5.4	
3698	Electrical Machinery, N.E.C.	5.4	
2212	Broad Woven Fabric Mills, Man-Made Fibers	5.0	
3560	General Industry Machinery	4.9	
264 0	Converted Paper and Paperboard Products	4.6	
2100	Tobacco Manufactures	4.1	
2611	Pulp Mills	4.1	
2711	Newspapers	3.9	

Appendix Table 2.

Mean and Standard Deviation of Independent Variables and Correlation with Tax Avoidance Rate

Independent Variables	Mean	Standard Deviation	Simple Correlation with TAR
Firm Size (F)	\$166 million	\$368 million	.060
Industry Size (I)	223,500 employees	225,900 employees	050
Market Concentration (C)	39.62%	14.34%	176
Profit Rate (R)	11.15%	5.46%	189
Geographical Disperson (D)	.1401	.0742	.007

COMMUNICATIONS

Standing to Sue Updated

TO THE EDITOR:

I think it worthwhile to bring my article "Standing to Sue: Interest Group Conflict in the Federal Courts" (APSR, September, 1976) up to date, especially in light of two recent cases decided by the Supreme Court, Warth v. Seldin and Simon v. Eastern Kentucky Welfare Rights Organization. 1

In so far as the essay pertained to conflict in the judicial forum between private groups, and in particular to cases in which groups seek to forestall actions by corporate business, the argument remains intact. Courts have continued to enlarge on the substantive standing test in force from Data Processing to enable environmentalists, consumers, and others to press their suits against a wide range of projects. Since Warth, Courts have recognized some new varieties of injuries-in-fact (e.g., "the risk that serious environmental impacts will be overlooked"); and they have further extended requisite zones of interest (e.g., socioeconomic impacts such as increased problems of fire and police protection and higher tax rates are protected by the National Environmental Policy Act). Organizations continue their post-Sierra Club representations: remarks one judge, after members of an unincorporated group without by-laws are deposed by defendants and shown to disagree about the purposes of the suit: "I am troubled by the lack of unity and substance of this organization. However, some members of Concerned Citizens live, work, and spend leisure time in Umtillo County. They meet the Warth standing test."2

In Warth v. Seldin, a divided Court denied an array of organizational plaintiffs standing to challenge local zoning ordinances that the Court acknowledges had the effect of excluding low-income and minority residents. The opinion is complex, shifting from one set of criteria and facts to eliminate one plaintiff, to

¹422 US 490 (1975); 426 US 26 (1976).

²City of Davis ν. Coleman, 5 ERL 20635 (1975); McDowell ν. Schlesinger, 6 ERL 20225 (1976); Port of Astoria ν. Hodel, 5 ERL 20658 (1975). These cases are picked from literally scores of recent examples. another set to eliminate the next; it concerns Article III standing rather than standing under the Administrative Procedures Act; and zoning is a peculiarly local matter. Nevertheless, the opinion does indicate an insistence on an injury which (1) is demonstrably an effect of the alleged infraction of law, and (2) may be afforded relief by the Court's action. The economics of the housing market, and not merely the zoning policy, is said to be responsible for plaintiffs' problems.

These requirements for injury-in-fact are reiterated in Simon v. Eastern Kentucky Welfare Rights Organization, where the Court dismissed as "speculative at best" the claim that indigents denied care by nonprofit hospitals have been harmed by an IRS ruling removing tax incentives to provide such care, and therefore have no standing under the Administrative Procedures Act. The Court in its opinion explicitly upholds S.C.R.A.P., claiming the plaintiffs there would have met the tests of causation and efficacy-of-relief. Resisting the temptation to agree with Justice Brennan's dissent in Warth, that the decisions of these cases can be explained only by a hostility to the claim on the merits, I might venture a distinction which squares with these decisions and leaves private defendants still vulnerable to challenges through the previously liberalized law of standing. For in these recent cases the Court rejects the argument of some social might-have-been in the absence of official behavior: minority residents would still have been excluded without the zoning; hospitals might not have given indigents care without the tax ruling. But anticorporation groups usually seek to stop or modify some specific, concrete project. Thus, the lower courts, based on Warth, denied standing for minority and other groups who claimed to be injured by H.U.D.'s granting funds for sewage and recreational improvements without due regard to the recipient town's exclusionary zoning ordinances; and, based on Simon, found standing for an organization of farm workers and farmers seeking to force corporate landlords to sell off certain excess lands, in accordance with reclamation laws.3 There is no question, however,

³NAACP v. FPC, 425 US 662 (1976).

that the majority on the Court is groping toward a more restrictive role for the judiciary in social conflict, and until their approach moves beyond the invocations of prudence in Warth and is anchored in clearer doctrine, any such prognosis must be, as Justice Powell might say, speculative at best.

One final point. Federal judges generally will, without raising questions of standing, allow parties who have participated in agency proceedings to challenge administrative rulings in Court. For example, the Supreme Court recently reviewed a ruling by the FPC that the Commission was not required by its legislative mandate ("in the public interest") to bring about equal employment in utility companies, contrary to the petition of the NAACP.4 While upholding the ruling, a "loss" for the NAACP, the Court went on to say that the FPC must make certain that consumers are not required to absorb any costs of strikes, boycotts, loss of contracts, or other effects of compliance or noncompliance with the antidiscrimination statutes - thereby putting a new subject matter on the regulatory agenda and paving the way for future challenges to utility companies. No issue of standing was raised. In the last couple of years the agencies themselves; in part because of prodding by the judiciary, have become relatively relaxed about what parties and organizations they will hear; indeed some are already in the business of financing witnesses to come before them. It is possible that even though the law of standing is tightened, a variety of groups, some with broad-gauge social claims like the NAACP, will reach the courtroom by the administrative route. Moreover, since this Court seems to want to restrain itself from overturning judgments made in the other branches, it would appear also bound to defer to the agencies' determination of who is a proper party at interest. While in such circumstances judges' discretion will in this limited sense have been trimmed, all of the dilemmas of interest group litigation discussed in "Standing to Sue" persist.

KAREN ORREN

University of California, Los Angeles

John Locke: Problems and Perspectives

TO THE EDITOR:

In his fine review of John Locke: Problems and Perspectives (APSR, September 1975), Robert H. Horwitz says the following of Hans Aarsleff's contribution to this collection of Locke studies: "But what is the status of Eternal Salvation [which is required for divine justice to be effectual] in Locke's teaching? Aarsleff's inspiring interpretation depends decisively on whether Locke has demonstrated the immortality of the soul. Unfortunately, according to Locke, its demonstration is beyond the capacity of human reason." (The classic argument in this vein is to be found in Leo Strauss's Natural Right and History, pp. 203 ff.: "[Locke] says, on the one hand, that, in order to be a law, the law of nature must not only have been given by God, but it must in addition have as its sanctions divine 'rewards and punishments,' of infinite weight and duration, in another life. On the other hand, however, he says that reason cannot demonstrate that there is another life. Only through revelation do we know of the sanctions for the law of nature or of 'the only true touchstone of moral rectitude.") Horwitz, agreeing with if not necessarily following Strauss, goes on to suggest that "Aarsleff appears to help Locke overcome the critical question of the immortality of the soul through nothing less than an act of faith: 'Absolute proof of the nonexistence of a future state is not to be had, consequently the knowledge of the mere probability of a future state with rewards and punishments is that which must direct our search for happiness."

Now Aarsleff has not "helped" Locke in any way, for it is Locke himself who argues (in the Essay Concerning Human Understanding) that "the bare possibility, which nobody can make any doubt of," of another life governed by divine rewards and punishments, makes it a good bargain to conform one's voluntary actions to the divine law: "If the good man be in the right, he is eternally happy; if he mistakes, he is not miserable, he feels nothing." Whatever one may think of this argument - it has the look of debased Pascalianism - it is Locke's own, and not one provided by Aarsleff with a view to "helping" Locke to overcome the incoherence of his theory through an "act of faith." (Ironically enough, one of the clearest things in Aarsleff's interpretation of Locke is his careful distinction between "demonstration" and mere "probability"; since, however,

⁴Evans ν. Lynn 537 F.2d 571 (1975); National Land For People, Inc. ν. Bureau of Reclamation, 417 F. Supp. 449 (1976).

this cannot be treated at length here, I will evade that responsibility by pointing out that I have already treated Aarsleff and Strauss on Lockean "demonstration" and "probability" in my article, "On Finding an Equilibrium between Consent and Natural Law in Locke's Political Philosophy" [Political Studies (Vol. 23, No. 4, 432–452), Oxford (December, 1974)] — an article which at least indicates the passages in Locke, Aarsleff and Strauss which should be consulted, whatever one might think about the proper interpretation of those passages.)

PATRICK RILEY

University of Wisconsin-Madison

TO THE EDITOR:

A brief response is called for — even at this late date — lest I appear ungrateful for Professor Riley's thoughtful critique of my review and for his generosity in drawing to our attention his interesting Locke article in *Political Studies*. Through his suggestion that I have wrongly accused Aarsleff of trying to "help" Locke, Riley has at least continued to focus our attention on such basic and difficult issues as Locke's position on natural law and the relationship, if any, in his thought between natural law and divine law. Riley stresses in his criticism that "it is Locke himself" who provides Aarsleff with the basic elements of his interpretation.

Now. Riley and I have no argument regarding the Lockean sources of Aarsleff's interpretation. Indeed, I went to considerable pains in my review to emphasize that "Locke's writings are replete with quotations from scripture and his professions of Christian piety can hardly fail to capture the attention of even the most superficial readers" (p. 1016). Unfortunately, as Riley observes repeatedly in his article, Locke qualifies and even at times flatly contradicts many of these quotations in other parts of his work. This leaves us with the difficult task of discovering his ultimate position on these questions and whether the elements in his position are consistent. Riley himself makes clear in his discussion of the secondary literature how few are the interpreters of Locke who have demonstrated the tenacity and other qualities of mind and heart required to see these tasks through to completion. It is easier by far to "help" Locke achieve a seeming consistency by accepting as conclusive certain limited elements of his thought while overlooking, failing to take seriously, or even repressing those aspects that don't fit. That, I had implicitly suggested in my review, is the crux of Aarsleff's problem.

Although Riley struggles manfully and openly to avoid this all too common problem, I daresay that he, too, succumbs to the temptation of trying to "help" Locke. This is suggested by the very title of his article, as well as by its substance, intent as he is "On Finding an Equilibrium Between Consent and Natural Law in Locke's Political Philosophy." It seems to me that it is Riley, not Locke, who provides the "equilibrium." To his credit, Riley identifies elements of Locke's thought that don't fit the "equilibrium" hypothesis. Nevertheless, he concludes rather condescendingly that Locke's "theory of natural (or rather divine) law ... taken as a whole, is intelligible, even if one might not want to derive the validity of such law from divine will, and even if one might wish that he had not (occasionally) argued as if such a law could be derived from reason alone" (Political Studies, p. 452). "But at least," concludes Riley, "the defects of Locke's system can be remedied with his own concepts, and in a Lockean spirit." Whether it is Locke's work or Riley's analysis that stands in need of improvement, the interested reader must judge for himself.

ROBERT H. HORWITZ

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More on Response Instability

TO THE EDITOR:

The recent exchange between Professor Achen and his critics in the American Political Science Review [December, 1976] centered on the problem of whether to attribute the random variability of attitude responses reported by Converse in American panel data to respondents who were vague about their attitudes or to vague questions. I suggest a third model: both respondents and questions were clear but on different wavelengths so that respondents attempting to map clear but complex or ambivalent attitudes onto a clear but simple Likert scale were forced into an unstable translation process that appears to be highly random.

For example, suppose I believe in affirmative federal action to end racial desegregation. But suppose I also oppose coercive government action, especially where it produces strong vocal opposition and civil unrest. An SRC researcher hands me a questionnaire, a seven-point Likert scale asking about my attitude toward court-ordered school busing with options ranging from "agree strongly" to "disagree strongly." I submit that I might reasonably translate my clear attitudes onto this scale by selecting almost any of the responses. I

might signal my agreement in principle and say "agree strongly." Or, expressing my ambivalence, I might signal my deep discomfort in practice by checking any option through "disagree moderately."

Or take foreign aid. I believe in helping needy people. But suppose I also dislike waste, corruption, using my dollars to line pockets of foreign elites, and making people dependent on handouts. Translating this attitude onto a simple seven-point scale about my attitude toward foreign aid might again be done by agreeing strongly or by checking any of the mixed, intermediate positions through "disagree moderately." Over time my true attitude may remain the same, but my recorded attitude could be highly variable as I faced anew the problem of translating a fairly complex set of relevant attitudes into one point on a scale.

In general a "translation difficulty" model of response instability agrees with Professor Achen in the conclusion that better questions probably would reveal American adults to have more stable orientations than Converse concluded. However, I am skeptical that vagueness is the relevant issue; both the attitudes in the examples above and Converse's questions seem quite clear to me. Measured attitude stability could be predicted to rise if researchers either use more complex attitude items that align themselves comfortably with naturally occurring ambivalences or perceived complexities among the general public or if researchers use questions that decompose derived attitudes toward policy questions into their constituent parts.

LLOYD S. ETHEREDGE

Massachusetts Institute of Technology

Alliance Behavior in Balance-of-Power Systems

TO THE EDITOR:

Patrick J. McGowan and Robert M. Rood in their article, "Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe" (APSR, 69, 3, pp. 859-870) have inferred three specific hypotheses from Morton Kaplan's "theory" of the balance-of-power system, based on the assumption that alliance behavior in such a system is a stochastic process, specifically a Poisson process. The hypotheses are that in a balance-of-power international system: (1) the occurrence of alliances will be stochastically distributed; specifically, the number of alliances formed per unit of time is a Poisson random variable; (2) the time intervals between alliances are random-

ly distributed; specifically, the distribution of interalliance intervals is a negative exponential random variable; and (3) a decline in the systematic rate of alliance formation precedes system changing events, such as general war. Testing these hypotheses against data on the frequency of formal and informal alliances between 1815 and 1914, they conclude that Kaplan's theory "has greater credibility than heretofore" (p. 870).

There are, in the first place, certain problems regarding the applicability of the Poisson distribution to the data on alliance formations over time. The Poisson distribution is applicable where a large number of objects (events) is distributed over a large area (time span). The data used by McGowan and Rood, however, consist of 55 cases of alliance formation (formal and informal) over 20 five-year periods. The small number of events makes the apparent fit with the Poisson distribution questionable.

Moreover, the Poisson process requires that each event be independent of all others, i.e., for any two events A and B, p(B|A) = p(B). Now, while it is commonly asserted of the European classical balance-of-power system that alliances were made irrespective of the ideological characteristics of alliance partners, that a particular alignment did not "predispose the same nations to align themselves with each other at the next opportunity" (p. 861), this is not quite the same thing as the independence of events as required in statistical theory. For a change in the composition of one coalition following a realignment in the opposing coalition is not really independent of the latter event. Think of a balance-of-power system consisting of six nations a_1, \ldots, a_6 . Let the opposing coalitions S₁ and S₂ at time t₀ be

$$S_1 = (a_1, a_2),$$

 $S_2 = (a_3, a_4),$

with a₅ and a₆ neutral actors. Now, suppose at t_1 coalition S_1 expands to (a_1, a_2, a_5) . Then, if at t2, coalition S2 is reorganized to consist of (a₃, a₄, a₆), we cannot maintain that what happens at t2 is independent of what happened at t₁. Not only that, even the time interval between the two events is dependent upon the fact that one is a response to the other. Instances of such readjustments of alliance structures, one in response to another, abound in the history of the classical balance-of-power system. The tenability of the assumption becomes particularly problematic when one is trying to locate clusters among as small a number as 55 cases of alliance formation over 20 time intervals in order to fit the data to a Poisson distribution.

This is revealed in the results of the test of the third hypothesis, which is the one of major substantive political interest. This hypothesis predicts that "system-changing events" such as a general war will be preceded by a decline in the rate of alliance formation. The prediction is borne out for the five-year period 1910-1914 preceding World War I in which no alliances were formed. However, McGowan and Rood's data reveal another period in which no alliances occur - the period 1820-1824 - following which no "system-changing events" happen: this the authors dismiss rather casually, and from a theoretical standpoint quite unsatisfactorily, as "the quiet period after the burst of diplomatic activity that concluded the Napoleonic Wars" (p. 869). Further extending the scope of the third hypothesis (although they concede that they do not want "to push it too far"), they also suggest "a serendipitous finding of some theoretical interest," viz., that in four out of five instances of change from one diplomatic period to another as described by Rosecrance [Action and Reaction in World Politics (Boston: Little, Brown, 1963), pp. 232-266], there was a decline in the rate of alliance formation (pp. 869-870). However, there are three other instances of a similar decline in the alliance formation rate (the periods 1830-1834, 1850-1854 and 1855-1859) in which no such change "in the structure of the system" occurs. The relationship between "system-changing events" and a declining rate of alliance formation, therefore, is not, it would appear, as firmly established as McGowan and Rood maintain.

Besides these problems of a statistical nature relating to the empirical testing of the hypotheses, there are certain more fundamental problems involving the theoretical status of the hypotheses themselves. Since McGowan and Rood's hypotheses are derived from Kaplan's "essential rules" for the balance-of-power system [System and Process in International Politics (New York: John Wiley, 1957], it would not be out of place here to discuss these briefly. Of the six rules, Rules (1) and (2) are the rationality assumptions for the behavior of individual actors. Rule (4) is the method of operation of the system, the means by which actors maintain the essential features of the system within a dynamic process. Rules (3), (5)

(5) and (6) the existence of an equilibrium which ought to be deducible from the behavior of actors as specified in Rules (1), (2) and (4). This point has been made by Arthur Lee Burns [Of Powers and their Politics (Prentice-Hall, 1968), pp. 113-116]. In other words, instead of analyzing the dynamics of the balance-ofpower system in terms of the behavioral assumptions about the actors (Rules (1) and (2)) and the adjustment mechanism of the system (Rule 4) and proving the existence of a stable equilibrium in the system, Kaplan merely states that the actors will behave so as to maintain the equilibrium (Rules (3), (5) and (6)). Thus, we do not have a model in which stability exists as a necessary logical consequence of the primary assumptions regarding the behavior of the actors and the dynamic adjustment process of the system; we have stability simply because we assume that actors act so as to maintain the equilibrium.

The result is that there is no way of determining whether a deduction made from this model is a necessary (essential) property of a balance of power system. Take Hypothesis 3 deduced by McGowan and Rood. This purports to relate the transformation of the system to a change in its activity rate. Since we do not know, however, the essential conditions which guarantee stability in the system, we have no way of knowing whether this is either a necessary or a sufficient condition for the transformation of a balance-of-power system. Kaplan himself, for instance, lists an elaborate, though by no means exhaustive, catalogue of the conditions which might cause a balance-ofpower system to be transformed into some other arrangement. These conditions are as diverse as the existence of actors who refuse to play according to the rules, capability changes inducing positive feedback, incompatibility among the essential rules of the system, existence of actors who prefer supranational organizations, failures of informational outputs, lack of flexibility of the adjustment mechanism (Kaplan, p. 27). True, all of these factors may contribute to the instability of a balance-ofpower system, and one could probably list several others as well. The point, however, is that this enumeration of the various possible factors which might transform the system remains entirely at the descriptive-empirical hypothesis that there was a freezing of alliance structures before the outbreak of World War I, we would still not necessarily have a tested theoretical answer to the question "Why did a general European war break out in 1914?" (McGowan and Rood, p. 889). The prerequisite for a satisfactory answer to this question is a theory of the classical balance-of-power system specifying the necessary conditions for stability of the system in terms of its structure, the behavior of the actors, the adjustment mechanism and the resources of the system, all in the historical context of the existence and collapse of the classical balance-of-power system.

PARTHA CHATTERJEE

Centre for Studies in Social Sciences, Calcutta

TO THE EDITOR:

Because "the classical literature on the balance of power consists of vague, often ambiguous, descriptions of the way the system is supposed to operate," contemporary studies of balance of power processes are open to at least two types of criticism: (1) the propositions investigated are drawn from a theoretically inadequate model of the balance of power, and (2) the techniques used to evaluate the empirical fit of the propositions are inappropriate to the task at hand. This is the essence of Dr. Chatterjee's communication regarding our article. More specifically, we are charged with making three errors of a statistical nature,

- (1) our application of the Poisson is incorrect because of our small N;
- (2) our use of the Poisson is incorrect because we confuse statistical independence with causal independence; and
- (3) we have misread the results of the test of our third hypothesis, leading to an ambiguous conclusion.

Finally, a "more fundamental" theoretical problem of our research, Dr. Chatterjee argues, is that

(4) our hypotheses, even if they are consistent with the data, do not tell us anything of theoretical interest because Kaplan's model fails to deduce the necessary conditions for a stable balance-of-power system.

In response to the first criticism, Dr. Chatterjee clearly misreads our article, and he apparently misunderstands the Poisson model. Our test of hypothesis 1 is not based on an N of 20 five-year periods as he says, but rather on an N of 101 years. This should be clear to any reader from the discussion on p. 867 and from Table 1 on p. 868. Dr. Chatterjee's confusion on this point probably stems from the fact that in testing hypothesis 3 we reaggregated our data on the basis of five-year intervals and retested for the presence of the Poisson. Moreover, this demonstration that the goodness of fit of the Poisson is not affected by how time is aggregated strengthens our findings regarding our first hypothesis.

Furthermore, the applicability of the Poisson model is not limited by the size of N as Dr. Chatterjee states. In any application in which the intervals are of equal length, say days or years, the sole parameter governing the generation of the Poisson distribution is m, the mean. Our estimated m produced by dividing 55 alliances by 101 years is .54, very similar to the m estimated by Richardson in his path-breaking studies of deadly quarrels when he divided 59 wars by 110 years.² Moreover, the Poisson is an exact distribution, that is, for any given value of the mean there is a distinct Poisson distribution. And since the mean has the well-known statistical property of being an unbiased estimate of m regardless of sample size, the size of N does not affect the applicability of the Poisson. Its effect is on good-of-fit tests such as Chi-square, rather than on the Poisson itself. With our N of 101 this is not a problem.

In his second point, Dr. Chatterjee suggests that we have misapplied the Poisson because of his contention that balance-of-power alliances are time-dependent processes. However, his criticism is based on assertion rather than on the presentation of empirical evidence that this is the case. By devising a hypothetical case in which two alliances lack independence by definition, he in effect sets up a straw man which he can then attack. Of course, our demonstration that the pattern of alliance formation over time fits the Poisson does not prove that alliance formations are, in fact, independent. But, our findings do render the assumption of independence more plausible.³

¹Partha Chatterjee, "The Classical Balance of Power Theory," *Journal of Peace Research*, 9 (1972), 51. 51.

²Lewis F. Richardson, Statistics of Deadly Quarrels (Pittsburgh: Boxwood Press, 1960), p. 129.

³On this point see Dina A. Zinnes, Contemporary Research in International Relations (New York: Free Press, 1976), pp. 252-253.

Our findings upon testing Hypothesis 3 have been misrepresented. The period from 1820 to 1824 was quiescent only in that no alliances were formed. It was a period identified by Denton (discussed by us on p. 869) as one in which war intensity was high. While we would agree that we have not provided rigorous statistical evidence to support the "serendipitous" findings in our article, Dr. Chatterjee chooses to ignore the citations in footnote 83 on page 870. In other research reported in a paper cited in that footnote, we provide statistical evidence rendering our serendipitous findings more plausible and we cite two studies by Singer and Small which reinforce our findings.

Dr. Chatterjee's fourth point is obviously more a criticism of Kaplan than of our article. We think that he has misunderstood Arthur Lee Burns, whose work he cites. Burns suggests that Kaplan's rules, especially Rules 1, 2, and 4 "...retain their heuristic capacity to suggest explanations of the process which, together, they help to describe." Moreover, Burns suggests that the appropriate way to evaluate Kaplan is to develop and test hypotheses about the general effects of these rules which in turn would yield a set of quantitative indicators of the process (e.g., the alliance formation rate). This is exactly what we did in our study.

Dr. Chatterjee ends his communication with an eloquent plea for an axiomatic theory of the classical balance-of-power system that will permit deductions concerning the necessary conditions for system stability and that at the same time achieves historical specificity. How can we disagree with yet another call for deductive theory in international relations? Several comments, however, are in order.

An axiomatic theory "in the historical context of the existence and collapse of the classical balance of power system" may be a contradiction in terms, and certainly very difficult to achieve. Where are social scientists with an empirical orientation to find such a theory? Professor Chatterjee's own attempt to provide the elements of such a theoretical model is strong on logical rigor but weak in historical context. Kaplan's "theory," which in our article we put within quotes specifically be-

cause of its logical weaknesses, still represents the best known attempt to formalize the balance of power and, we think, it possesses considerable heuristic power as suggested by Burns.

In our view international relations research is based on deduction and induction. Our empirical generalization that in the case of nineteenth-century Europe, decreases in the alliance formation rate were associated with war, including World War I, should be of interest to proponents of axiomatic theory construction since they cannot include all possible system stability conditions in their theories and achieve at the same time their valued goals of parsimony and elegance. Axiomatic theory building must start somewhere, and what we know about the operation of balance-of-power systems as shown by previous historical and statistical research certainly must be accepted as a fundamental point of departure. Likewise, when the deductive theory of balance of power called for by Dr. Chatterjee is produced by him or another theorist, we shall welcome its appearance and rush to test its predictions. That we did not produce such an axiomatic theory in our article is certainly true, but not, we think, relevant to what we did do.

Thus, while we thank Dr. Chatterjee for his interest in our work we cannot accept his criticisms for the reasons we have given.

PATRICK J. McGowan

University of Southern California
ROBERT M. ROOD

University of South Carolina

Primates and Political Authority

TO THE EDITOR:

I turned to Professor Willhoite's "Primates and Political Authority" (APSR, 70 [December, 1976], 1110-1126) with some excitement, expecting to find in it a new dimension of the theme of "the primacy of politics." The essay does indeed pull together some fascinating information about the social lives of primates. Upon reflection, however, it is not clear that the essay has any implications whatever in regard to political authority. The central thesis of the essay is that there are "...good reasons to believe that man's biological nature incorporates strong propensities to establish and sustain dominance-deference hierarchies within his social groupings; that is, the stratification of political authority, power and influence may be by nature intrinsic to human social

⁴Arthur Lee Burns, Of Powers and their Politics (Englewood Cliffs: Prentice-Hall, 1968), p. 115.
⁵Ibid.

⁶Chatterjee, "The Classical Balance of Power Theory," op. cit. This historical context may be present in his recent book, Arms, Alliances and Stability (Delhi: Macmillan, 1975), but we have not yet been able to secure a copy.

existence" (p. 1110). This is followed in turn by the suggestion that liberalism and radicalism may be in need of critical reconsideration and perhaps drastic revision.

Willhoite's thesis is vulnerable to two serious criticisms. In the first place, it is a mistake to equate political authority with power, stratification, dominance-deference relationships, and so on. Power and stratification may truly appear in all human groupings. That is an empirical question. But nothing in man's nature can entail political authority or reduce it to the level of a natural or "intrinsic" phenomenon. For political authority, unlike power and other such concepts, is a relationship that includes reciprocal moral commitments, as well as power. The search for the conditions in which mere dominance may be made legitimate - which is to say, transformed into authority - is, as Rousseau suggested at the beginning of the Social Contract, the major task of political theory.

Furthermore, it must be insisted that the question of the meaning of "authority" is not to be dismissed as "merely semantical." The English language, though often vague, is not an amorphous pulp.

In spite of the attempts of some political scientists to operationalize "authority" by equating it with power or dominance, it continues to mean legitimate power, or legitimate right of decision or command (according to the context) both to laymen and to political scientists. A pair of hypothetical examples will suffice to illustrate this: (1) Were the Attorney General to advise the president that "you do not have the authority to dismiss the Chairman of the Federal Reserve Board on the sole basis of policy differences," no one would interpret this to mean that the president lacked the physical means to effect the dismissal. On the other hand, the phrase, "you don't have the power to dismiss him" is ambiguous. It could imply either the lack of the legal right or the lack of the practical capacity to dismiss. (2) Were the director of the F.B.I. to inform the president that "it is not within our power to prevent all terrorist acts at domestic air terminals," no one would suppose that he would be disclaiming the legal right to prevent such acts. But if he did want to claim a lack of jurisdiction, then he would have to say, if he wished to be understood, that "it is not within our authority...." Authority, clearly, is not at all the same thing as power, stratification, and dominance, and nothing but confusion can result from ignoring or glossing over the dif-

The second difficulty appears to be a misuse

of the claim that the supposed "propensity" of humans ("to establish dominance-deference hierarchies," etc.) is natural. What can such a claim mean? From the behavioral perspective, everything that man does is natural. "It is the fundamental premise of evolutionary biology,' Professor Willhoite tells us, "that man's social behavior is totally a part of the 'natural world.'" Well, since used car salesmen, for example, have lately shown a propensity to wear white shoes and belts, this must, then, be natural. As must the propensity of steelworkers to vote Democratic. Is Willhoite's "propensity" of the same order as these? If it is, then he has told us nothing new - nothing that we need to observe primates in order to discover. Men do indeed establish and sustain structures of dominance and deference, and it must be natural. But others, even if only a small minority, aspire to live on a footing of equality and reject even egalitarian forms of authority. Is that therefore natural also?

Clearly, Willhoite wants to claim that man's propensity toward dominance-deference relationships is natural in some stronger sense. But how can one thing be more natural than another? This is possible, logically, only if a nonnatural category is admitted. The obvious candidate is the artificial, that is, the man-made, as opposed to the natural. But Willhoite, as we have seen above, rejects this dualism. This would seem to leave the propensity of men to establish dominance-deference relationships on the same basis as the propensity to abolish them. It will be instructive to attempt to save this argument by introducing the category of the artificial anyway: man, we shall say, has a natural propensity to establish dominancedeference relationships. But the propensity doesn't dictate the concrete details of the relationships. Given man's symbolic capacity. he has an indefinite number of ways in which to fulfill this propensity. Here, human artifice steps in. That there will be dominance and stratification is naturally determined. But their exact form is variable and subject to historical influences. Some human arrangements will fulfill the propensity more completely than others and thus, presumably, be more stable. Some will fall so far short that they will be destroyed immediately. Will this modification of the argument do the trick? It will, as long as we continue to find men, without exception, living in dominance-deference relationships. But what if we were to find even one case of men living in full equality, and with nothing more than transitory and ad hoc relationships of dominance-deference? Then we would have to say that there is no propensity that precludes such

arrangements - since they actually exist.

The upshot of this is that the validity of Willhoite's thesis depends upon evidence about what humans do — not upon evidence about what primates do. And that is what we knew (or should have known) in the first place.

PHILLIP C. CHAPMAN

University of Arizona

TO THE EDITOR:

Professor Chapman's first criticism is that I illegitimately equated political authority with power, stratification, and dominance-deference relationships. I'm sorry if I conveyed that impression to any reader; I certainly did not mean to use "authority" in any eccentric way but simply as power or hierarchical relationships considered to be legitimate or rightful. Through my discussion of "attention structure" and "charisma," I was suggesting that hierarchyforming is phylogenetically rooted in the human species and long preceded the emergence of hominids' capacity to distinguish symbolically between legitimate and illegitimate power. I would further suggest that we often tend to accord authority, in the strict sense, to those who are in fact our hierarchical superiors. It is also possible, of course, to believe that one is obliged to defer only to those to whom one has initially conceded authority. That I have a strong normative preference for this latter ordering of priorities should be evident from. the concluding paragraphs of my article. My principal concern, however, was to outline a phylogenetic perspective on human hierarchies, the behavioral substructure of all authority relationships.

Professor Chapman's second critical point made me painfully aware that I had used the terms "nature" and "natural" equivocally and had thus unintentionally contributed to misleading readers about my meaning. In my central thesis statement (p. 1110), I used "by nature" in the traditional political philosophic sense. But on p. 1124, I used "natural" in Robert Bigelow's evolutionary-biological sense (as typified in the quote by him on pp. 1125-1126). I wish now that I had said "phylogenetically" instead of "by nature" in my thesis statement, because, as the remainder of the article makes clear, that is precisely what I meant. From an evolutionary-biological perspective I would continue to reject a dichotomy between "natural" and "artificial" in human behavior. Rather, the significant question is the degree to which specific types of behavior are constrained or channeled phylogenetically. From this standpoint, then, used car salesmen's apparel choices are perfectly "natural" but very likely free of any significant degree of genetic control. I continue to believe that there are persuasive reasons for giving serious consideration to my basic hypothesis: the propensity to form hierarchies is not only, in the descriptive sense, "natural" to humans, but has also been strongly selected for in the phylogenesis of our primate species.

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Politicians in Uniform

TO THE EDITOR:

The recent article by Robert Jackman ("Politicians in Uniform: Military Governments and Social Change in the Third World," APSR, 70 [December, 1976] 1078-1097) attempts to resolve the question of the relationship between military governance and social change. The core of Jackman's argument is a reconstruction of Nordlinger's "Soldiers in Mufti," (APSR, 64 [December, 1970] 1131-1148). Jackman identifies important limitations in Nordlinger's tests of various hypotheses concerning the effects of military rule and attempts to improve Nordlinger's work by using covariance analysis. We think, however, that Nordlinger made a number of errors which Jackman repeats, albeit in a slightly different form. More importantly, we believe that Jackman's article illustrates a major pitfall in the empirical literature on policy outputs, i.e., the danger of analyzing political economy problems without giving careful thought to their nonpolitical dimensions.

Jackman correctly identifies major errors in Nordlinger's article, including the misspecification inherent in zero-order correlations and the indeterminateness of the direction of causality which results when the dependent variable (social change) occurs before the independent variable (military involvement)! Jackman is right to correct these errors, but they do not exhaust the difficulties in Nordlinger's piece.

One of the primary problems of Nordlinger's analysis is the high probability of measurement error. Nordlinger measured the effects of military involvement on seven different indicators (most coming from Adelman and Morris's Society, Politics and Economic Development), including such items as "leadership commitment to economic development," "rate of improvement of human resources," "change in the effectiveness of the tax system," "change in the rate of gross investment," and "rate of growth of GNP per capita." "Control" variables included the size of the middle class and the

degree of "modernization of its outlook." Whether such a concept as "leadership commitment to economic development" makes any sense we're not sure, but in a situation where lending agencies like the International Monetary Fund and the World Bank require a plan as a precondition for aid, it is hopeless to think that the existence of a plan indicates the extent of such a commitment. Moreover, even were we to accept to validity of this measure, it still has problems. As defined it does not appear to represent any kind of scale, and even if it did, it would have but three levels, thereby engendering problems of limited variation.

Similar problems arise with Nordlinger's second index of economic development. Can one talk about the modern outlook of social classes? Adelman and Morris's category of the most "modernized" middle class included such countries as Argentina, Brazil, Chile, Greece, Rhodesia, South Africa, Taiwan and Uruguay. Perhaps the middle class is just fickle; perhaps the concept makes little sense in the first place. Moreover, even a cursory reading of Nordlinger's index reveals that it measures much the same thing as the dependent variables of several of the equations. This may account for the more frequently significant t-coefficients and the higher R2's in Table 1 and 2 than elsewhere in the article. By adopting Nordlinger's choice of variables, even while changing his specification, Jackman is thus vulnerable to major errors.

Let us now consider Jackman's own model. He specifies the problem to be explained as one of "social change" and utilizes four indicators of that concept: change in per capita energy use (a proxy for economic growth), change in the ratio of school enrollment to population, change in the number of doctors per capita, and change in the number of radios per capita. These four indicators are largely unrelated (see Table 5). As separate concepts, this would be fine, but since they are supposed to be measures of a single concept, their near-orthogonality raises major questions. How are we to know that the indicators are measuring the same thing? Perhaps there is no concept here at all. We agree with Professor Jackman that these are interesting variables in themselves, but we disagree with his apparent belief that he has got around the problem of multidimensionality by applying the same explanatory model to each variable. That this approach is unsuccessful is most evident in the results: R2's that average .196 with one control variable and .211 with the other. In the presence of evidence of misspecification of this magnitude, nothing can be concluded about the slopes of the included

variables. In other words, the effect of the military's involvement in politics cannot be determined in these models.

While Jackman may have corrected some of Nordlinger's problems, he thus did not correct all of them. Problems of measurement, contamination, and specification clearly remain. These problems are solvable in principle, but it is our conviction that Nordlinger's problems lay deeper than Jackman recognized and that this is his ultimate undoing. More importantly, it is at this point that the fundamental problems that bedevil this particular article can be seen to underlie much of the broader literature on policy outputs. Nordlinger's dependent variables include some that are directly controllable by the government (like the tax system), some that are partially controllable by the government (like the rate of gross investment), and some which the government influences much less, even if it adopts "appropriate" policies. The presence of these different kinds of dependent variables has important implications for this kind of work.

One consequence of recognizing these distinctions is that we should shy away from any analysis that uses the same set of variables and the same mathematical form to account for variations in different kinds of dependent variables. Not only is the government differently related to each kind of indicator, but some indicators may logically precede others and may, in fact, be parameters in a model determining others. Gross investment rates, for example, may depend on the tax system, while per capita GNP growth depends, at least in part, on the gross investment rate. Minimally one might conclude that identical models of these phenomena are inappropriate.

Suppose we want to examine the effect of government policy on agricultural productivity. We might consider such factors as the incentives to invest in new agricultural technologies, the domestic terms of trade between agriculture and industry, the international terms of trade and the relative price advantage thereby given to domestic agricultural producers, the structure of land law, and the efficiency of the transportation network. All these clearly have a considerable bearing on the rate of growth of agricultural productivity, and they might well be included in any regression model.

A very different model would be appropriate to explain change in school enrollment ratios. Such a model might include lagged measures of public expenditures on education, the previous level of enrollments (including the colonial heritage), population density and the number of languages spoken (which affect costs). Worth

noting, however, is that governments seeking to speed social change might choose not to spend money to accelerate enrollments, either because they already have a school-leavers problem, or because it might be cheaper to educate students abroad, or because the private return to education is sufficiently high relative to the social return that subsidies are economically unproductive.

Modeling these kinds of outcomes seems relatively easy compared to modelling changes in per capita product, partly because the underlying structure appears to be more obvious, and partly because some of these indicators are themselves logically precedent to other kinds of change. (There is, of course, the complication that changes in school enrollment ratios, for example, may depend on economic growth, but in such a model economic resources can be taken as a parameter.)

Perhaps this argument sounds so involved that crunching ahead is the only alternative to giving up the whole affair. We think not, and it seems to us that our earlier distinction indeed offers a simplifying key to the problem. First, one should separate results of (1) direct government activities like changing the tax structure, allocating public funds, or repressing and torturing; from (2) partially controllable results like increasing school enrollments and raising gross investment; and from (3) ultimate outcomes like changing GNP, reducing the Gini coefficient, or increasing the population's caloric intake. Immediately we see that in order to affect ultimate outcomes, directly controllable activities must vary. So we can start with these direct and more easily modelable policies: public spending, taxation, repression. If they do not vary according to the degree of military involvement, the problem is resolved right there. If they do, then we must move to the next (less controllable) activity, always modeling with a plausible specification.

Such an approach raises the question of whether we can expect government policy to have much of an effect on ultimate economic outcomes. Put another way, is it credible that policy instruments represent real control variables in determining the behavior of the dependent variables? A negative answer to this question may, in fact, be plausible. The 1973 rise in the U.S. agricultural prices had more to do with Russia's decision to enter the world market than it did with U.S. agricultural policy, whatever our feelings about Butz's response to this fact. And much of the third world's development in the early 1950s resulted not from the domestic policies of their governments but rather from the rapid expansion of demand

for primary products resulting from the Korean War. The importance of these arguments can be seen in Jackman's null finding. Methodological caveats aside, is this result to be taken as showing that military governments in fact do not differ from civilian regimes in their policy outcomes? Or does it simply mean that government policy in general does not have much influence over the phenomena of concern?

In sum, we wish to emphasize that the technical features of a complex analysis are significant and pose difficult problems, but the deeper problem is the danger of modeling complex political-economic relationships without giving primary attention to their distinctive structures.

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TO THE EDITOR:

Ames and Bates voice a series of muddled complaints with my paper. None of these complaints is relevant to my analysis, most are unoriginal, and some are quite silly.

The major objection appears to be that I did not give careful thought to the "nonpolitical dimensions" of "political economy problems." (I assume that this is intended to be my "principal undoing" rather than Nordlinger's.) This complaint is not without its logical and semantic difficulties: if non-political issues are the key to a problem, in what sense can that problem meaningfully be labeled one of political economy? More puzzling, however, is the very restrictive definition of "political" implied in their assertion (nowhere do Ames and Bates provide an explicit definition). Toward the end of their remarks, political is taken to refer to "controllable activities." Thus, "direct government activities" like allocating public funds are "political"; "partially controllable results [sic]" such as increasing school enrollment ratios are (presumably) semipolitical; while "ultimate outcomes" like altering the size distribution of income are "nonpolitical." This implied definition is too restrictive to be useful, and fits poorly with the thrust of most political economy. For example, it is naive to assert that the size distribution of income is nonpolitical, but Ames and Bates cannot be prevented from making that assertion for this reason alone.

Ames and Bates have had some difficulty representing what my analysis was all about. Their remarks serve only to cloud the issues that I addressed. I concluded that government structure (in the form of the "simple civilianmilitary government distinction" [p. 1097]) has no effects on a variety of aspects of social change (when I wrote the paper in 1974, the dominant view1 was that such effects were quite strong). My conclusion does not imply that government policies have no effect on these dependent variables, although it does imply that if such policy effects exist, then variations in government policies themselves are not dependent on the simple civilian-military government distinction. A large body of literature does indicate that governments can effectively pursue policies designed to stimulate the economy, to increase school enrollment ratios, and so on. My results simply suggest that the probability that a government will successfully pursue such policies is not dependent on whether or not it is a military junta.

Ames and Bates should familiarize themselves with the elementary analytic distinction between political structure and government policy. Unfortunately, they seem not to realize that this distinction makes their remarks irrelevant to the issues addressed in my paper. I did argue that "military governments ... do not differ from civilian regimes in their policy outcomes." Whether or not "government policy in general does not have much influence over the phenomenon of concern" (the question that exercises Ames and Bates) is a wholly separate issue. Because answers to the second question do not depend on answers to the first, their remarks are quite beside the point, however much Ames and Bates may wish to speculate on this topic.

A second area of confusion in their remarks centers on the distinction between model building and hypothesis testing. My paper was concerned with testing a series of hypotheses that link military regimes to social change. In contrast, Ames and Bates would like to specify causal models that explain change (see, for example, their paragraph 9), which leads them to take my low R²'s as "evidence of misspecification." This misrepresents the purpose of my paper. Low R²'s might have provided some (but hardly the most interesting) evidence of poor specification had I attempted to specify a general model of social change. However, given

¹In fact, Huntington's argument and Nordlinger's results seem to be widely accepted. See, for example, S. E. Finer's analysis in the new edition of his well-known *The Man on Horseback: The Role of the Military in Politics* (Baltimore, Md.: Penguin Books, 1976), pp. 260–267.

(a) that this was not my goal, and (b) that I was testing the validity of a series of well-known arguments, the low R²'s are more usefully viewed as evidence inconsistent with those hypotheses and consistent with my original conclusion. Actually, other evidence (for example, the small t-ratios and the mixed pattern in the signs of the parameter estimates) are more important in this context. Rather than belabor the point, I invite Ames and Bates to consider more carefully the ambiguities inherent in using R² as a criterion for model specification (the less obvious of these ambiguities are discussed in most econometrics texts).

Ames's and Bates's confusion carries over into their claim that my model does not allow me to determine "the effects of the military's involvement in politics." This is nonsense. If my model is misspecified owing to an omittedvariables problem, and if this results in biased estimates (it need not), the typical result would be an overestimate of the size of the militaryregime parameters.2 I fail to see how this could lead me to revise my original conclusions. If my model is misspecified in the sense that I do not consider intervening variables (say, government policies), and if military regimes do affect social change indirectly, my estimates would still show military-government effects. This point is so elementary that I will not pursue it here.³ Ames and Bates do not offer an alternative specification that they think better addresses the issues with which I was concerned. That they do not is, in my view, indicative of their confusion and uncertainty over what they are trying to say.

The remarks about the multidimensionality of "social change" are gratuitous. I argued (p. 1092) that social change is not a unidimensional concept, and further, that there are no good substantive reasons for treating it as such. In fact, there are strong grounds for treating it as multidimensional, as I noted. Ames and Bates apparently agree (they offer no arguments to the contrary). However, they seem to think that military regimes might have different effects on each of my four dependent variables (if they do not, their objection is not in the least bit relevant). Had they considered the issue seriously, they would have noticed that my analysis was designed to allow for this possibility. Had they stopped a little longer to

²On this general subject, see for example Potluri Rao and Roger L. Miller, *Applied Econometrics* (Belmont, Calif.: Wadsworth, 1971), pp. 60-67.

³For a recent discussion, see Duane F. Alwin and Robert M. Hauser, "The Decomposition of Effects in Path Analysis," *American Sociological Review*, 40 (February, 1975), 37–47.

digest my results, they might have also found that there is no evidence to support the argument that the effects of the civilian-military regime distinction on social change vary by type of social change.

Finally, Ames and Bates assert that the "core" of my analysis is based on Nordlinger's data, and that I am "thus vulnerable to major errors." This is a strange criticism, because nowhere do I recall defending the validity of those data. I invite Ames and Bates to reread my paper, where I noted instead that Nordlinger's data "have serious limitations, which means that they cannot be used to generate an overwhelming challenge to Huntington's original hypothesis" (p. 1090). My reason for reanalyzing Nordlinger's data before proceeding to my own (which I consider the core of the analysis) was to show that differences between my conclusions and his are not an "artificial" function of data differences. Ames's and Bates's remarks on this subject are quite unintelligent.4

Where does all of this leave my analysis? Ames and Bates have failed to raise any issues that would lead me to modify any of my original conclusions (pp. 1096–1097). Instead, they have employed a naive definition of the term "political," and then muddled through a set of "issues" that (a) have no bearing on my paper, and (b) have dubious implications for anything. Had Ames and Bates spent a little more time formulating their argument, we might have been spared their platitudinous closing prescription. Even better, had they remembered that the efficacy of any prescription depends on the validity of the initial diagnosis, we might have been spared their remarks altogether.

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The Minimum Winning Coalition

TO THE EDITOR:

In his paper "Hollow Victory: The Minimum Winning Coalition" (APSR, 70 [December, 1976], 1202-1214), Mr. Russell Hardin offers an excellent lemma which significantly im-

⁴For example, they claim that a variable that has only "three levels" ipso facto suffers from "problems of limited variation." The absurdity of this remark is clear when we think of binary variables (e.g., the black-white distinction in the U.S. and the Catholic-Protestant distinction in Northern Ireland): presumably Ames and Bates do not think that such variables can have only trivial (i.e., substantively

proves the proof of the size principle.

He then goes on, however, to spoil his argument by trying to show that the proof is insufficient by reason of what turns out to be an erroneous counterexample. This counterexample is supposed to show that on some occasions people prefer larger than minimal winning coalitions, whereas what it actually shows is that they prefer these larger coalitions only if they can change the rules of the game.

This counterexample derives from a five-person game (Game 1) which he says (p. 1203) has the characteristic function:

"The value of each 3-player coalition is 60.
"The value of each 4-player coalition is 50.
"And the value of the 5-player grand coalition, of course, is zero."

Mr. Hardin's use of the word "each" catches the spirit of Von Neumann and Morgenstern's remark just before their definition of the characteristic function that the features of the definition hold "for all subsets of S of I" where I is the total set of players. Since Mr. Hardin's game is said to be zero-sum and symmetric, we can infer that the rest of the characteristic function of his game is:

The value of the empty coalition is zero.

The value of each one-person coalition is -50.

The value of each two-person coalition is -60.

Quite properly Mr. Hardin defines the characteristic function as "the total payoff which the coalition can guarantee its members no matter what the players outside the coalition do" (p. 1203). Of course, since in zero-sum games not all sets of players can achieve their values, what this means is that, for every set of a given size, that set can guarantee at least the value of a coalition of that size, if the set forms a coalition.

When Mr. Hardin comes to offer his supposed counterexample, however, he violates this definition and his given characteristic function. The payoff schedule in his counterexample is:

and is obtained by (ABC) bribing D or vice

meaningless) variation. Surely they know that to address the question of limited variation, one compares the observed dispersion of a variable with its theoretically possible range.

¹ John Von Neumann and Oskar Morgenstern, The Theory of Games and Economic Behavior (Princeton, N.J.: Princeton University Press, 1953, 3rd edition), p. 241.

versa. Quite obviously the subset composed of D and E gets less than what it could guarantee itself against all the other players and furthermore could unilaterally enforce. By the characteristic function, D and E as a two-player set can guarantee itself no less than -60; yet in the counterexample it gets -78. Clearly what Mr. Hardin has done is change his game in midargument, allowing D and E to content themselves with what they could easily avoid. Let us call his new game Game 2, then the characteristic function of the two games can be depicted as in Figure 1. As can be seen from the figure, three-person sets would much prefer Game 2 to Game 1. Of course a counterexample that relies on changing the characteristic function of the game under consideration does not prove much of anything. The fact that the members of the set of D and E in the supposed counterexample are receiving less than they could guarantee and enforce for themselves in a coalition should alert us that something funny is going on. Specifically, it should alert us to the fact that the four-person coalition and payoff is simply not realizable among rational players who have plenty of time to bargain. If this payoff schedule were proposed (as it well might be) or even temporarily adopted (as also might happen), then E could offer a counterproposal in which D and E as a coalition both improve their positions individually, for example

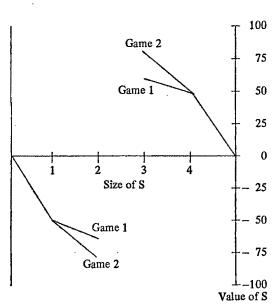


Figure 1. Characteristic Function

I do not suggest that such a payoff schedule is itself stable. I do point out, however, that

- (1) it is better for both D and E than the distribution proposed by Mr. Hardin, and
- (2) D and E by themselves can enforce it. That is, regardless of what A, B, and C do, D and E can refuse to pay out more than -60 in Game 1. Since they have a positive incentive to so refuse, they have the motive as well as the ability to prohibit this supposed counterexample.

Furthermore, in general for this game, if any coalition of four persons contains a subset of three persons who together get more than 60, there is some division of -60 among the remaining two members which is individually better for both of them and which the two person coalition can enforce by reason of the rules of the game in the characteristic function. If they do enforce it - and among rational players enforcement of the two-person coalition is to be expected - then they can also force the replacement of a four-person coalition with a three-person one. Consequently, any four-person coalition containing a three-person subset that receives more than 60 is unstable, unless, of course, the game is changed, which is what I suppose Mr. Hardin means to do by allowing it to stop at his proposed payoff distribution.

If there is a four-person coalition in which some three-person subset receives exactly 60, then, while the four-person coalition can be formed, there is no need for it, and it is difficult to see what role the fourth person plays in it. Surely the three-person set has no motive to expand to a four-person coalition just to contain the unnecessary player. In this case the four-person coalition is trivial.

If there is a four-person coalition in which no three-person subset receives as much as 60, then some such subset can, with an advantage for each of its three persons, reduce the coalition to a three-person one and enforce it to obtain 60. Consequently, with both the motive and the power they can be expected to do so. Of course, such a three-person coalition may itself be unstable. Thus, when

is proposed or exists, players C, D and E can upset it with the payoff schedule

and enforce it. Just what three-person coalition will form and be enforced cannot be predicted, although *some* three-person coalition can be enforced if the members together receive 60.

To summarize the results for four-person coalitions, in Game 1 it is the case that either

- (1) they are pointless, or
- (2) there is within them some subset of three persons all of whom individually prefer an enforceable three-person coalition, or
- (3) there is some immediately available set of two players (one inside and one outside the four-person coalition), both of whom prefer some enforceable payoff arrangement in a two-person coalition to permitting the four-person coalition to come into existence or to continue to exist.

Consequently, we may be sure that so long as Game 1 is being played, there will be no four-person coalitions because no four-person coalition can enforce itself against the sets of players that necessarily have some preferable payoff in two- or three-person coalitions. That is, a four-person coalition can be forced, either from inside or outside, to reduce to three persons.

But we do not know which three-person coalition will exist, for in zero-sum games there are no undominated payoff schedules. Hence, for every division among three persons some superior alternative of distribution and coalition can be found. Is there any way out of the cycle? Intuition tells us that, since three-person coalitions valued at least 60 can enforce themselves, some one of them will in fact do so. But which one? Solution theory attempts to offer an answer. To sketch one such simple theory for this game – and to show that it absolutely excludes four-person coalitions - I introduce the notion of the bargaining set.² In this theory there are payoff configurations in which each actually formed winning coalition receives at least its value and no individual player does worse than the value of a coalition of a single member. The problem is to determine whether

²R. J. Aumann and Michael Maschler, "The Bargaining Set for Cooperative Games" in *Advances in Game Theory* (No. 52 in Annals of Mathematics), ed. M. Dresher, L. S. Shapley, and A. W. Tucker (Princeton University Press, 1964), pp. 443–476. I am indebted to Richard Smith for pointing out the relevance of the bargaining set.

or not a payoff configuration, x, is stable over n players:

 $x = (x_1, x_2, \ldots, x_n; B_1, B_2, \ldots, B_h),$ where B_1, \ldots, B_h are existing coalitions and x_1, \ldots, x_n are the individual payoffs. The set of all stable payoff configurations is called the bargaining set. In theory, all actually occurring outcomes will be chosen from this set.

To determine stability, consider some winning coalition B_j in x with disjoint subsets K and L. We say K has an objection or threat against L if K can propose a configuration

$$y = (y_1, y_2, ..., y_n; C_1, C_2, ..., C_r),$$

in which L is not a member of K's new coalition and in which

$$y_i > x_i$$
 for all i in K

 $y_i \ge x_i$ for all the players, i, whom K has brought into the winning coalition to replace L.

We say that L has a counterobjection or counterthreat against K if L can propose a configuration

$$z = (z_1, z_2, \ldots, z_n; D_1, D_2, \ldots, D_k),$$

where L has formed a winning coalition with some of K's partners in y (but not including K itself) such that

 $z_i \ge x_i$ for all i in L and for all i who are L's new partners,

 $z_i \geqslant y_i$ for all i who are in L or who are in L's new coalition and who were partners of K in y.

We say that x is stable if for every objection y, there is a counterobjection z. The fundamental notion is that, if K and L threaten to split up, they will do so if one can get an advantage that cannot be countered; but if every threat can be countered, there is no point to splitting up, and hence x is in that limited sense stable.

In Mr. Hardin's game, the four-person coalition with the configuration

$$x = (26, 26, 26, -28, -50; (ABCD), (E))$$

is clearly unstable. Let K be person D and let him propose against L (here (AB)), the objection

$$y = (-30, -30, 28, 16, 16; (AB), (CDE)),$$

which is a valid objection because

$$y_D > x_D (16 > -28)$$

$$y_C \ge x_C (28 > 26)$$

$$y_E \ge x_E (16 > -50)$$
.

Can L (here (AB)) offer a valid counter objection? To do so A and B must each get as much as they got in x, that is, 26 each. Then they may ally with either C or E or both. If with C, then C must get at least $z_C = 28$ and the total for (ABC) is 80; but D and E do not have to pay out more than -60 so (ABC) is an impossible dream. If (AB) ally with E, then E must get at least $z_E = 16$, and the total for (ABE) is 68, which is also an impossible dream, again because C and D do not have to pay out more than -60. If (AB) ally with both C and E, they (C and E) must get respectively 28 and 16 so that the total for (ABCE) is 96 and D need not pay out more than -50. Hence L (here (AB)) has no possible counterobjection, and therefore K's (here D) objection can stand uncountered and x must be regarded as unstable.

In general in Game 1, all four-person coalitions involve unstable payoff configurations either because one member is getting a negative payoff or because no set of three players is achieving its full potential. But there are some stable payoff configurations with three-person winning coalitions, namely all the permutations of (20, 20, 20, -30, -30). Consider

$$x = (20, 20, 20, -30, -30; (ABC), (DE)).$$

Let K be C and L be (AB). Then, if C threatens $y = (-30, -30, 20 + \epsilon, 20 - \epsilon, 20; (AB), (CDE)),$

which is valid because $y_C > x_C$ and $y_D \ge x_D$ and $y_E \ge x_E$, L (here (AB)) has a counterobjection:

$$z = (20, 20, -30, 20, -30; (ABD), (CE)),$$

which is also valid because $z_A \geqslant x_A$, $z_B \geqslant x_B$, $z_D \geqslant x_D$ and $z_A \geqslant y_A$, $z_B \geqslant y_B$, $z_D \geqslant y_D$. By appropriate rearrangements of $+\epsilon$ and $-\epsilon$, it is possible to show that *all* objections of A, B, and C against each other with alternative three-person coalitions can be responded to with similar counterobjections. Hence x is stable and in the bargaining set.

I have thus shown, albeit informally, that Game 1 admits of no stable four-person coalitions because they can be broken up from either inside or outside by players who have both the motive and power to do so. But Game 1 does admit some three-person coalitions that have a kind of limited stability even though there is no absolute guarantee against cycling among possible coalitions and payoff schedules. We can conclude, therefore, that while all possible coalitional arrangements in this (and all other) zero-sum games can be dominated and hence need not last, still there is some possible stability in three-person coalitions, and, for certain, there is no stability at all in four-person

coalitions, which is precisely what the size principle asserts.

In a footnote on page 1206 Mr. Hardin remarks that his example is based on a "more general demonstration" in Norman Frohlich "The Instability of Minimal Winning Coalitions," APSR, 69 (September, 1975), 943-946. This essay contains exactly the same error as Mr. Hardin's example, except where Mr. Frohlich applies it to characteristic functions with positive slope.³

In Mr. Frohlich's argument, for a game with $n \ge 5$ players, there is a winning set S, with s members, where $s \leq n-2$, and a complement, -S, which is divided into sets R and Q. The essential feature of the situation is that the members of R can "bribe" their way into (SUR) by paying S an amount a, where a means less of a loss for the members of R than their current loss in R. Now before the bribe the set S was paid v(S), and the set (RUQ) was paid -v(S), since the game is zero sum. After the bribe (SUR) is paid (v(S)+a), while the sum of the payments to the subset composed of R and Q is (-v(S)-a). Now, by itself $(R \cup Q)$ can guarantee that it receive at least -v(S). But after the bribe its members actually receive (-v(S)-a), which is less than they could jointly guarantee themselves. Hence R and O have both the motive and the power to form a coalition that reduces S back to s members in exactly the way I have pointed out in discussing Mr. Hardin's example. The only way in which the bargaining among players could possibly stop at the larger coalition (SUR) rather than the smaller coalition, S, at least for zero-sum games with negative slope, is that there be some heretofore unmentioned rule that allows the larger coalition to change the game into one in which v(S) is greater and -v(S) smaller than originally stipulated. In the absence of such a rule, by the generalization of the arguments I offered about Mr. Hardin's game and the bargaining set, it can be shown that the set of outcomes that actually come into existence consists entirely of configurations with minimal winning coalitions. Indeed, it has been pointed out to me that the size principle is a straightforward (and trivial) deduction from the notion of the bargaining set.

WILLIAM H. RIKER

University of Rochester

³In William H. Riker and Peter C. Ordeshook, An Introduction to Positive Political Theory (Englewood Cliffe, N.J.: Prentice-Hall, 1973), p. 185, a similar argument is used about characteristic functions with positive slope, but it is described on p. 186 as "organizational" as distinct from mathematical.

TO THE EDITOR:

Professor Riker's letter falls into two parts: a criticism of my "Hollow Victory," and a new set of assumptions on which to base an alternative proof of the size principle. The first part, the criticism of "Hollow Victory," is based on the assertion that "what Mr. Hardin has done is change his game in midargument." This assertion and the accompanying Figure 1 are wrong. The payoff schedule (26, 26, 26, -28, -50) is demonstrably an imputation for my Game 1, because it is derivable by inspection from the characteristic function which Riker spells out (Luce and Raiffa, Games and Decisions (New York: Wiley, 1957), p. 193, give the defining conditions on imputations, which are merely the permissible outcomes in a game). The four-person coalition ABCD receives 50 as it should. It requires none of the disingenuousness of changing the rules of Game 1 for the players to achieve these payoffs, and Figure 1 is without game-theoretic meaning. Therefore the first half of Riker's letter as well as his concluding remarks on Norman Frohlich's paper are wrong.

If, on the contrary, Frohlich's and my papers were genuinely wrong, then Occam's razor (what can be done with fewer assumptions is done in vain with more) should have excised the second part of Riker's letter, because it is based on a restricted version of the Aumann-Maschler bargaining set (see the reference in Riker's fn. 2). The latter is built by piling assumption on assumption until, in a limited form which does not entirely please Aumann and Maschler, the restriction to minimum winning coalitions is virtually assumed. Bargaining set theory is too involved for adequate treatment in this space, but I will briefly summarize several aspects of it which are relevant to the size principle.*

A simple version of bargaining set theory defines "individually rational payoff configurations" to be stable (or in the bargaining set). Such configurations are those in which all extant coalitions achieve their value and in which every permissible objection can be met by a valid counterobjection. Riker writes that the outcome (26, 26, 26, -28, -50; ABCD, E) enforced by a four-person winning coalition is not in the bargaining set of Game 1. He is right. In the simple bargaining set theory, however, this is of no consequence for the size principle, because the outcome (12½, 12½, 12½, 12½, 12½,

*Much of my understanding of bargaining set theory I owe to discussions, under the duress of the printer's deadline, with Joe Oppenheimer. -50; ABCD, E), all its permutations, and the outcome (0, 0, 0, 0, 0; ABCDE) are in the bargaining set. Hence, the bargaining set places no restrictions on the size of winning coalitions.

Such outcomes as (0, 0, 0, 0, 0; ABCDE) are annoying in a solution theory. To prevent them, Aumann and Maschler (p. 445) propose the additional assumption that a coalition will not form if some of its members can obtain more by themselves forming a permissible coalition." Noting that this is "a strong assumption," they call it "coalition rationality." If we now add this assumption to the definition of a revised bargaining set, outcomes associated with four- and five-person coalitions are no longer in the bargaining set for Game 1. If a coalition has incentive to expand under Riker's earlier assumption set (in Riker and Ordeshook, Introduction to Positive Political Theory, p. 177), it cannot now do so if the resultant coalition would include a subset with incentive to evict one or more members. Definitio ex machina, larger than minimum winning coalitions will not be permitted. In this sense, the size principle is, as Riker here writes, a trivial deduction from the revised Aumann-Maschler bargaining set, because the principle is proved by a question-begging assumption.

The objection and counterobjection argument from bargaining set theory renders the word "bargaining" peculiarly inapt - the actual nature of the theory is more nearly antibargaining. The objection-counterobjection definition of rationality prohibits all bargaining in which one player gives up something in order to bribe another into a mutually beneficial coalition. A player is permitted to bargain not in order to protect most of what he might receive but only to increase it (in an objection) or to maintain it exactly (in a counterobjection). This implies a peculiar preference ordering. If Aumann (or Maschler) were wealthy and disdainful of the larger society, he naturally would prefer keeping his wealth untaxed; but faced with a hard choice of losing it entirely or merely paying a small tax on it, he evidently would prefer losing it entirely (the more "stable" outcome). We have become inured to the fact that assorted game theorists can devise situations in which counterintuitive outcomes do not seem perverse, but we may still hesitate to put great store in behavioral theories based on counterintuitive (even counterfactual) behavior assumptions.

Unfortunately, this is not the only, nor even perhaps the most important, prohibition on bargaining in the Aumann-Maschler theory. Bargains initiated by losers are also prohibited, because objections can be posed only from

within extant coalitions. Why must a loser shut his political mouth until someone among the winners approaches him? Perhaps because otherwise the bargaining set would often be as empty as the core. For example, in Aumann and Maschler's (p. 447) game one of whose two "stable" outcomes is given in the payoff configuration (75, 25, 0; AB, C), suppose the loser were permitted to object with (0, 26, 24; A, BC). Player A could not counterobject, and no payoff configuration in the game would be in the bargaining set. Is it rational of player C to be reticent as required by Aumann and Maschler when he might bargain his way to a better life while the worst he risks by speaking up is to remain in his dismal state?

Kenneth A. Shepsle ("On the Size of Winning Coalitions," APSR, 68 [June, 1974], 505-518) used bargaining set theory to argue for the size principle in five-person symmetric games. Unlike Aumann and Maschler, Shepsle permitted losers to bargain. Rather than prove the principle, he provided a four-person payoff configuration with an objection to which he said there was no counterobjection. The bete noire of the size principle, Robert L. Butterworth, in a comment (ibid., pp. 519-521), provided a counterobjection. With a display of courage that a contributor to a mathematics journal would not dare, Shepsle nevertheless let his paper go to print. Perhaps the only constructive result in that twenty-page use of the Review was Shepsle's final footnote in his acknowledgement of error, or rejoinder (ibid., p. 524n), noting that "the size principle is an immediate consequence" of imposing coalition rationality. In his main paper, he cited a paper by Robert Wilson (with which I am not familiar), saying Wilson assumes coalition rationality "by restricting consideration" to minimum winning coalitions (ibid., p. 513n). For properly defined games, assuming coalition rationality and permitting only minimum winning coalitions to form are equivalent. Aumann and Maschler (pp. 445-446) express unease with the assumption of coalition rationality because it is subject to the usual objections to the core as a solution theory. Among these is that the core is essentially counterfactual in that players of zero-sum games commonly achieve outcomes, whereas if the assumptions underlying the core are the relevant standard of rationality, such games are not resolvable.

The strongest objections to bargaining set theory, especially in its restricted Aumann-Maschler variant, are that it rules out of order much behavior which seems more compelling than the theory. I will not elaborate the type of such behavior any further here other than to note that not lowest on the list is the behavior of players in Game 1 in forming a larger than minimum winning coalition. Suppose A, B, and C contract to coalesce and to cooperate in trying to increase their payoffs above those in (20, 20, 20, -30, -30; ABC, DE), which belongs to the revised bargaining set. ABC may now offer separately to both D and E entry into a larger coalition with a payoff of -28. D and E now face a partial Prisoner's Dilemma. If they cooperate in coalition with each other, they can achieve a joint payoff of -60, although at least one of them will then receive less than -28. If one of them defects, they receive a joint payoff of -78, as in the outcome (26, 26, 26, -28, -50). Of course, it is impossible for both to defect, but that is all the more reason for each to hasten to defect first. Much as in usual Prisoner's Dilemma, at least one of D and E will be better off to defect no matter what the other plans to do. Indeed, because the Prisoner's Dilemma is only partial in this respect, it is of little advantage to D and E that they might communicate to each other their offers from ABC.

All of this behavior is defined out of the realm of rationality in the revised Aumann-Maschler solution theory. If one nevertheless likes the fundamental emphasis of bargaining set theory, he must either assume the size principle or accept such outcomes as (0, 0, 0, 0, 0; ABCDE). I and no doubt many others, including Ransom Gillet, would not find it uncongenial to assume the size principle in many contexts, although that leaves us very far from proving it in the elegant fashion Riker originally attempted from an unobjectionable set of assumptions. Indeed, it leaves us in a position not unlike conventional speakers of the mother tongue who invoke "rationality" in explaining politics. One might say it was rational of the United States to bomb Hiroshima. The only difficulty with the claim is that another might say it was irrational. There being no commonly accepted set of assumptions from which to arbitrate the claims, the debate cannot be resolved. Similarly, if Tip O'Neill says Ransom Gillet was silly, that the tax reform or tariff coalition ought to be as large as possible rather than as small as possible, then political scientists have no acceptable positive theory with which to instruct O'Neill of his folly. Indeed, he might be right.

RUSSELL HARDIN

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Freedom and Distributive Injustice TO THE EDITOR:

Douglas Rae's review of Anarchy, State and Utopia by Robert Nozick (APSR, 70 [December, 1976], 1289-1291) is an example of the highest level of the reviewer's art. He takes a position he disagrees with seriously, and attempts to criticize where he finds it weak and to learn where he believes that it makes a contribution.

Rae regards Nozick's "Wilt Chamberlain" critique of systems of distributive justice which are based on their results rather than upon a method embodying freedom as "deadly serious," because such end-state principles contradict the "freedom to dispose of one's just share." Rae attempts to transcend this dilemma, although he is too intelligent to think this solves it, by contrasting the Chamberlain freedom example with "the very different cases of Satchel Paige and Jim Thorpe. It is not that they earned too much but too little, and they earned so little because of raw and simple prejudice worked out in the marketplace" (p. 1291). So Rae finds Nozick's minimal state "unresponsive to our trouble," "innocent," and "too optimistic to suit our case" (1290-1291).

Yet is Nozick's minimal state unresponsive to the Satchel Paige and Jim Thorpe cases? For Thorpe, the criticism that he earned too little cannot stand. In the market, he was paid one of the highest salaries of his day both in professional baseball and football. When he signed with the New York Giants, he was paid "unusual money for a rookie." Rae must be thinking of the AAU decision taking Thorpe's Olympic medals away. But the market place had a reaction to this. His biographer says that the AAU decision was made and sustained "despite the scorn and criticism of public and press."2 The marketplace then paid Thorpe handsome salaries as compensation during his best years and later elected him the first president of the American Professional Football Association.

Now what was the non-Nozickian government doing? Well, it had an end-state program not only for Thorpe but for all Indians. It had earlier redistributed the land of Thorpe's tribe to more worthy beneficiaries first in Kansas and then again in Iowa.³ It, likewise, had a population distribution policy which reduced the tribe from 4500 members in Kansas to 387 who

reached Oklahoma. The state so believed in redistributing property that it repeated the process for a third time in the "land rush" of 1891. The government was so concerned with the welfare of its clients that it even closed the Carlisle school (it was begun with partial financing from the government) the school which had given Thorpe his opportunity to escape from end-state justice for a while. It seems that the school was not meeting current educational standards and that the discipline procedures shocked congressional sensibilities. So to protect the students, they were sent back to reservation end-state justice.

Rae is on stronger grounds with the case of Satchel Paige, for there was marketplace prejudice which cost him income. But it is instructive to look at the situation more closely. Baseball originated in 1839, and by 1867 there were 100 professional clubs in the United States. At that time, blacks played for several teams. On December 12, 1867, the National Association of Baseball Players Nominating Committee voted to exclude clubs which hired Negroes.

It is significant that the ban was instituted by a union. Peterson says that, although the succeeding players' association did not formally adopt this rule when it was organized in 1871, it kept the rule as a "gentlemen's agreement." The owners in most of the early leagues, however, did not accept the ban for the obvious reason that several had valuable players already on their clubs. Moreover, they willingly traded for blacks when this was to their advantage. Thus, the Northwestern League, called one of the four strongest at the time, had Negroes; so did the International League, then a major league, the American Association (later league), and the Western and Middle States leagues.

So, at this time, baseball offered a classical example of union restrictions on a labor market. There even is evidence that money, rather than raw prejudice, was the issue. Thus, the great black star for the International League in the 1880s, Frank Grant, was resented by some of the white players because he asked for more money. Yet, one also notes the usual lack of success of these techniques when they are not backed by law. They fail because the market incentives to "cheat" on the other owners to gain profit are too great to make the restrictions work. Even when the International League formally adopted the color line in 1886, not only were the five black players in the league not ejected but the Syracuse club signed

¹ Jack Newcombe, The Best of the Athletic Boys: The White Man's Impact on Jim Thorpe (Garden City, N.Y.: Doubleday, 1975).

²*Ibid.*, p. 209.

³*Ibid.*, pp. 15, 21.

⁴Robert Peterson, Only the Ball Was White (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 17.

two more at the end of that season. The same year the Ohio State League rescinded its previous color ban.

Peterson, who is not a political scientist, finds that, mysteriously, the era of black players in white leagues ends in 1895.⁵ Of course nothing in social life is this simple, but we political scientists know that the government gave us *Plessy v. Ferguson* the following year. The informal exclusion was now backed by the power of law, and between 1898 and 1945 it was so effective that blacks played only in Negro leagues.

It is interesting that John McGraw tried to "sneak" a black on the Giant team as an Indian in 1901⁶ and that two dark-skinned Cubans played between 1911 and 1921. I find it critical, though, that major-league baseball integration preceded Brown v. Board of Education by almost a decade. The facts are revealing. In 1943 Branch Rickey told the major stockholder in the Dodgers that he needed to rebuild the team with young talent and that this might mean signing Negroes. The major owner - a bank of all things - told Rickey to go ahead if it meant getting a pennant-winning and, therefore, successful team. An economic boycott of Yankee Stadium in 1945 also helped prepare the way for Jackie Robinson's breaking the color ban in 1946.

Paige was not hired until Bill Veeck signed him in 1948. Since Paige played his first game in 1924, he was underpaid for perhaps 24 years. But was it the market that was to blame or the state acting through the Supreme Court? Or rather, was the market not being bold to defy the separate-but-equal doctrine of the state? Of course, Plessy did not actually mandate segregation as an end-state but several state and local laws did, especially in St. Louis, Baltimore, and Washington and for spring training, and in the minor leagues. The market eventually permitted the hiring of Paige (although not his qualified early teammates) and even his election to the Hall of Fame. When the state finally did act, all it did for Paige was restore the status-quo of pre-1896 integrated ballfields which he had already experienced some six years earlier.

Is the minimal state too optimistic in its unresponsiveness to problems of distributive justice as apparently believed by Rae? Or is it naive to believe that the state can do much more than give equality before the law as Nozick thinks? As Rae says, the question "is no newer than the old story of freedom versus equality" (p. 1291). Yet, it seems to me, if one

looks at the Chamberlain, Thorpe, and Paige examples, these chapters favor freedom.

DONALD J. DEVINE

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The Strained Alliance

TO THE EDITOR:

Professor Byung Chul Koh has written a specious comment on my recent The Strained Alliance: Peking, Pyongyang, Moscow and the Politics of the Korean Civil War (APSR, this issue). He sets up a straw man and, apparently to his own satisfaction — surprise — has knocked it down. This has been accomplished by ignoring much of the volume's thrust and the data which support it. Instead, he has devoted his limited space to constructing porous and stilted criticisms.

The review's cynical tone is constant; e.g., "... sets out to demolish... hypotheses are so precariously grounded... no one, including the author, denies... apparent lack of integrity... admits at the outset it is 'not a definitive work.' "Each atmospheric phrase is designed to implant doubt upon the book's major points, which the review largely neglects.

The volume itself, in contrast, is more restrained. For example, the full context of the above last phrase which "admits at the outset it is 'not a definitive work,' "reads:

Because of this lack of facts regarding each of the four involved countries, this is not a definitive work. Rather, it attempts to present data and raise hypotheses not usually considered. This is done with the belief that it is as worthwhile to ask questions and to suggest new viewpoints as to repeat what appears to have been the unsatisfactory answers previously advanced about the war. Therefore, not much space is devoted either to arguments which are now generally accepted, and to which we agree, or to earlier hypotheses which seem inadequate explanations to important questions (p. xvii).

... In this exploratory book I have attempted to walk a middle line between widely known and less familiar data, hoping that others will be prompted to research further some of the hypotheses pursued here. At points in the narrative where the facts of a situation are well-known and widely accepted, the story is touched upon only to provide context (*Ibid*).

Although not the principal purpose of a review, this one, in its eagerness to nit-pick, did not accept my invitation to offer, however briefly, reasonable substantive additions.

The only factual criticism which Professor Koh does deal with concerns the term "civil war." He is not persuasive. Of course I do not

⁵*Ibid.*, p. 49. ⁶*Ibid.*, pp. 54–56.

'deny' that there was "massive external intervention in the war." Neither does Seoul, P'yongyang, Moscow, Peking, or Washington. However, it is a nonsequitur to assert that. because there were outside participants, the war, therefore, was not in any important measure a civil one. If outside forces are involved, are internal ingredients of the origins of a war to be ignored? This is the implication of the reviewer's logic. It should be noted that civil wars (either potential or current) do, in fact, often attract outside participants. My book discusses the war as primarily, but explicitly not solely, originating from internal causes, which were externally encouraged. It was (and is) a reality that Koreans in both halves of the peninsula considered themselves to belong to the same, artificially divided, nation. Does Professor Koh 'deny' the fact that the civil aspect of this war was an important dimension? The review states: "Setting aside its origins for the moment...." But it never returns to this vital problem. Does Professor Koh dismiss the idea that looking at the war from its partially internal nature provides a valuable perspective?

Related to this is the reviewer's denial that "the Korean language makes no rigorous distinction between civil and other types of wars." Aside from his approving reference to the Korean term for "fratricidal rebellion" (remembering that in 1950 both Seoul and P'yongyang claimed to represent the legitimate Government of the nation, this would be a reasonable synonym for civil war), is the fact that English-Korean dictionaries have entries for "civil war." More importantly, while spending time wondering about the translation of this term, Professor Koh might have taken advantage of the citation which carefully traced its utilization by each state.

A major interest of the book was which state was saying what to whom and when. The volume quotes a Soviet domestic broadcast of January 1951, contrasts it with a Korean message sent to the Soviet Union in August 1952, and then speculates "may well have been meant as an indirect rebuke to the Soviet Union." (As in each of the review's cripplied quotes, the cautioning phrase — "may well have been meant" — was dropped.) I was questioning the timing and frequency of the term's use by Moscow. The "preface" explains why such attention had been paid to the transcripts of radio broadcasts:

From close textual readings of the transcripts from the 1950-53 period, it is frequently possible to determine shifts in attitudes be-

tween the three communist states. This is particularly true when a radio broadcast is being beamed to one specific country in that state's language.

Was this the first time that the Soviet Union had translated this Korean phrase as "civil war"? Would this not suggest Moscow's intensifying interest in separating itself from the war?

The review's use of my phrase "As a 'harder' indication that June 25 was not the prearranged date..." as a prelude to its editorial "... simply strains one's imagination..." is dramatic, but of small significance. The word "'harder'" was punctuated purposefully with quote marks in the book itself (p. 118) in order to suggest to the reader that this specific document should be dealt with cautiously. On the other hand, this 'red herring' allows the reviewer to avoid the fact that this single quote is but one from a lengthy list of indicators which, I stated, may suggest an original invasion date of early August 1950. Moreover, it should be noted that the validity of this document did not "strain the imagination" of the scholars who are cited, Profs. Rodger Swearingen and Paul Langer.

In a similar fashion, while Prof. Koh is accurate in suggesting that weapon supplies often continue after an invasion, the pattern of supply and the overall context of timing and flows indeed do contribute insights into intentions and strategies.

Among the review's overconfident assertions is this statement: "That the relations among the three communist states on the eve of, during, and after the Korean War were consistent with this general pattern [of strains] therefore does not come as a surprise." It may, however, have come as some "surprise" to readers of several earlier books which touched on these states' relations of this period (including Prof. Koh's 1969 work) which had not examined tensions within the alliance during the war. The Soviet Union, moreover, had been viewed as the sole factor causing the North's invasion. My book readily acknowledged the Soviet's crucial military assistance, but supplements this fact with the equally important factor of North (and South) Korean politics.

The reviewer mixes oranges and apples when he asks why doesn't Peking "set the record straight with respect to its perception of Moscow's role in the U.N. in 1950?" (It also should be mentioned that the review does not comment on my argument that the Soviet Union consciously 'froze' China out of the United Nations in 1949-50.) After all, he notes, "In November, 1975, Peking even distorted Soviet Ambassador Malik's U.N. speech..." The apparent answer to the review's question is that

Joseph Stalin and his policies generally are praised, not criticized, by today's China. He likewise misleads by failing to note the depth of the Chinese sources traced in the footnotes.

Prof. Koh's gratuitous comment on the respect for "father" in Korea is irrelevant; firstly because the text explicitly was referring to a Moscow radio broadcast. Secondly, and of more import, North Korea had become increasingly disenchanted by the unsatisfactory support which the Soviet Union was supplying, and was relying more on China. Analogies to "indirect rebukes" are not uncommon (e.g., Saigon's similar-style messages to the U.S. during its last decade of war).

Unfortunately, the review ignores other data and hypotheses, partly drawn from thenclassified U.S.-Army Intelligence sources, which might interest readers. Three examples: (1) In spite of the frequency of the claim, there had been no actual Taiwan Straits 'interdiction' in the summer of 1950 of a strength sufficient to prevent a successful Chinese invasion of Taiwan; (2) the Soviets pulled most of their personnel out of Korea after the U.S. strafed a Soviet air field inside of Siberia in October 1950. I then discuss the effect this had on China and North Korea; (3) the book details the inadequate and insufficient Soviet aid given to China and North Korea throughout the war, and discusses how this reality affected the alliance.

Perhaps most important, it is necessary to respond to the reviewer's statement about my "apparent lack of integrity." The allegation is serious, and would be applicable if I were taking credit for original points. In fact, however, I attribute unique points to their author's scholarship. Moreover, the review fails to tell the reader that my book is replete with citations to unpublished Ph.D. theses and conference papers. As carefully noted in the book, my purpose was both to present commonly agreed-upon information and to reinterpret some of this information, as well as to offer "new" data.

It is paradoxical that the review faults the book even as it demonstrates how carefully the source material was handled, e.g., the Korean term rendered as "civil war," via Pravda, via an English translation. I am not alone in building upon background material (as distinct from data and hypotheses which are presented as original) which is widely shared. My "preface," only a part of which is quoted above, made this sharing of widely accepted material explicit. Further, if an interested reader wished to look at our two respective volumes and the phrases which are briefly parallel, they will note that

Prof. Koh has selected only the congruent wordings, with ellipses filling in the nonsimilar thoughts. Preceding or following sentences and paragraphs often diverge from the quotes presented by the reviewer; moreover, these phrases are well known to students in this subfield. This same exercise of seeking agreed-upon information can be followed with other works dealing with widely accepted material.

In this context, Prof. Koh's remarks on three specific sources in the book are all drawn from 28 pages of text. These in turn - again the reader of the review would be unaware of this - are supported by 76 footnotes which frequently drew on original, and sometimes contradictory, sources. Another comment, which unsubtly implies possible plagiarism, refers to a chapter of 33 pages, with 34 notes (most of which have multiple entries). This includes three unpublished theses. The footnote which caused Prof. Koh to wonder has both the Korean and English publication data, and is followed by a relevant Ph.D. thesis by Prof. Benjamin Min, with his page number given. It should also be noted that in the remainder of the book the Korean volume (Fifteen-Year History of North Korea) is referred to only in its English-translation citation. Interestingly, after this chapter, only English language translation, with its page numbers, are given. I am sorry if Prof. Koh felt slighted, but there was no appropriation of original ideas.

Perhaps it is necessary to point out that in the workings of memory, one retains not only background, but the language in which it has been encased. The review would have been more useful if it had weighed my intentions and caution seriously, examined my use of translation from Chinese to English, and told the reader if there were data (or hypotheses) in the book which he considered informative, interesting, or helpful in taking a fresh look at several of the book's various themes.

ROBERT R. SIMMONS

University of Guelph, Ontario

TO THE EDITOR:

A basic and explicitly articulated premise of my review was that the scholarly value of the type of Kremlinological exercise which Professor Simmons performs in his book hinges on the plausibility of inferences in terms of the rules of elementary logic. If the inferences are not plausible, such an exercise remains at the level, not of serious academic discourse, but of trivial armchair speculation. It was and remains my considered judgment that his work is

seriously encumbered by logical fallacies, of which a few examples were given in the review. What is more, its credibility is severely undercut by what I view as questionable ethical standards of the author.

It is noteworthy that the author's lengthy letter does not contain any explicit denial of my implied charge of plagiarism. Instead, he refers to my failure "to tell the reader that my book is replete with citations to unpublished Ph.D. theses and conference papers" and to my alleged selection of "only the congruent wordings, with ellipses filling in the nonsimilar thoughts." He further argues that "these phrases are well known to students in this subfield" and points out that "Koh's remarks on three specific sources in the book are all drawn from 28 pages of text [which] in turn are supported by 76 footnotes which frequently drew on original, and sometimes contradictory, sources." Finally, the author finds it "necessary to point out that in the workings of memory, one retains not only background, but the language in which it has been encased."

It is precisely because of the deceptively impressive scholarly paraphernalia in which the work is packaged that I felt compelled to point out the evidence of questionable scholarly practice. The latter calls into question the integrity and credibility of the author, which in turn considerably dilutes the worth of scholarly trappings. As for his allegation that I have "selected only the congruent wordings, with ellipses filling in the nonsimilar thoughts," it should be pointed out that "ellipses" are used only in the third example, where they occur at the end of the quote from Simmons. In the quote from my own 1969 work, the omitted phrases are as follows: "after operating under the banner of the Korean Independence League for a while"; "(Sinmindang)"; "as demonstrated by its growing membership"; and, finally, "(NKWP)." Not only do these phrases not contain any "nonsimilar thoughts," they also illustrate how Simmons makes use of other people's works without citation - i.e., by leaving out a few phrases here and there and by making slight changes.

If "these phrases are well known to students in this subfield," is Simmons implying that it was I who have plagiarized them in the first place? Contrary to Simmons's contention, the three quotations are drawn, not from "twenty-eight pages of text," but from four pages (pp. 23-26). The author's plea that "the workings of memory" may have been responsible for the inclusion of "the language" in question is understandable but, in my opinion, unconvincing. For I found other examples of a similar

practice (p. 22 of Simmons and p. 2 of Koh; p. 26 of Simmons and pp. 9-10 of Koh).

What makes this episode rather perplexing is that the author is obviously a gifted writer; unlike many foreign students from non-English speaking countries, he had no compelling necessity to borrow, stealthily or otherwise, the words of other people — particularly those of a foreign-born scholar writing in an alien language. Moreover, as I pointed out in my review and the author concurred in his letter, none of the passages in question involved any novel ideas.

The author writes in his letter that I took issue with "one from a lengthy list of indicators which, I stated, may suggest an original invasion date of early August 1950." I discussed two of his "indicators" in my review; let me take up some of the others here. One is the paucity of Russian advisers attached to the Korean People's Army at the time of the invasion. However, whether the presence of "between 3 and 8" Russian advisers "per division" bespeaks inadequate preparation or the consummation of the Soviet plan is a matter of judgment. Another "indicator" is the reported absence of the chief of North Korean military intelligence from North Korea at the time of the invasion. If his alleged presence in Moscow at the time is confirmed, that would, in my view, buttress rather than undercut Moscow's preeminent role in the affair.

Still another "indicator" is that, "of 100,000 North Korean troops in the ranks of the Chinese People's Liberation Army (PLA) in 1949, [o] nly 12,000...had returned to North Korea before June 25" (p. 118). This assertion must be examined against the backdrop that the PLA-affiliated Korean troops owed their primary loyalty not to Kim Il-song but to the Yenan faction led by Kim Tu-bong and that Kim Il-song's power position was by no means unassailable in June, 1950.

Simmons also cited Khrushchev's statement in his memoirs that "Kim [Il-song] was the initiator" of the Korean War. The authenticity of at least parts of the Khrushchev memoir. however, remains to be corroborated. The author also cites a State Department study which states that "[t] op secret work plans of the Standing Committee of the Labor Party headquarters dated January-June 1950 make absolutely no reference to the forthcoming invasion," that "a number of fairly highly placed North Korean officers that were interviewed, including the Chiefs of Staff of two divisions, stated that they had only the barest presentiment of the coming of hostilities, and that they were given no concrete indication of

their onset until approximately one week before the invasion took place" (pp. 120-121). This would suggest, in my view, the extreme care with which the secret invasion plans were guarded, for if the vital advantage of a surprise attack was to be safeguarded, it was absolutely imperative that the number of people privy to the plans be kept to the barest minimum until the latest feasible moment. It is also significant that Professor Simmons conveniently overlooks the major thrust of the same State Department study - that "the U.S.S.R. was able to keep close check over the movement of the North Korean army by allocating gasoline to the army on a monthly basis" and that, given the nature and extent of Soviet control in North Korea in 1950, it is highly improbable that the decision of the North Korean regime to attack South Korea could have been taken without "Soviet approval if not inspiration." (See Department of State, North Korea: A Case Study in the Techniques of Takeover [Washington, D.C.: Government Printing Office, 1961], pp. 113-120.)

Another important "indicator" cited by Simmons is the rivalry between Kim Il-song and Pak Hon-yong in the period preceding the June 1950 invasion. While the existence of such rivalry is well documented, it still remains to be demonstrated that it "led to the war" (p. 110). That the two North Korean leaders may have tried to outmaneuver each other in an effort to bolster their respective nationalist credentials remains a conjecture, as the author makes clear. Taken together, the preceding "indicators" do not constitute a credible basis for the author's hypothesis, no matter how imaginative the latter may be.

Professor Simmons argues that my review is

"specious," "porous," "stilted," "cynical," and based on a "straw man," while his book "is more restrained." The author challenges the views of such established scholars as Allen S. Whiting (p. 48, p. 75, and pp. 149-152), George Liska (pp. 249-250), Robert Osgood (p. 251), and Raymond Aron (pp. 251-252). Not that there is anything wrong with such an approach; in fact, it is to be applauded. Whether it represents "restraint" on the author's part is an entirely different matter, however.

The author betrays his style of reasoning when he argues in his letter that the use of material which is two steps removed from the original "demonstrates how carefully the source material was handled." He avoids the central issue involved — i.e., the hazard of making inferences from something which has gone through two translations in such disparate languages as Russian and English. More important, the author is amply aware of the problems caused by errors in translation, for his own critique of Whiting rests largely on this very point. (See, especially, pp. 150–151.)

Needless to say, my review represents but one man's appraisal of the author's study which is the product of "two years [of research] in Hong Kong, Taiwan, the People's Republic of China, North and South Korea, Japan, and Washington, D.C." (back flap of the book). Regretfully, however, I am constrained to report that, whatever merits it may possess are eclipsed by the various flaws I have discussed above and in my review.

В. С. Кон

University of Illinois at Chicago Circle

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EDITORIAL COMMENT

In Conclusion. It is said that somebody once asked Lord Reith near the end of his life what the best form of government was. "Tyranny," replied the old man, "tempered by assassination."

We do things more gently than that around the APSR. The managing editor may get away with an occasional whim, and that is about as close to tyranny as a scholar comes — or ought to come. Assassination, likewise, is fortunately stronger medicine than we provide for superannuated managing editors. They are, rather, periodically just let out to pasture.

And what a pasture it is! Insofar as we have been tired out by our editorial labors, there is the refreshing prospect of first-hand reengagement with some of the fundamental problems of our age and place, which, after all, is what writing and thinking everyday political science is all about for most of us. Insofar as editorial activity has been a stimulus and a goad to thought, we can contemplate a rededication to our own work, to see if any of the vagrant notions we have entertained from time to time will sustain a concentrated dose of attention.

To our successors we leave the faint rumble of complaints about the *Review* that one perennially hears around the profession. Sheer relief at a changing of the guard will certainly give them the honeymoon they need — and deserve — as they discover for themselves what they want to do and how they want to do it.

Errata. In "Industrial Conflict in Advanced Industrial Societies" by Douglas A. Hibbs, Jr. (December, 1976) in footnote 19 on page 1045, Equation (a) and the sentence that follows should read

(a)
$$SV_t = \beta_0 + \beta_1 \left[\Delta R_{t-1} - ((1-\lambda) \sum_{i=0}^{\infty} ((\lambda+\delta)B)^i \Delta R_{t-2}) \right] + \beta_2 U_t + \beta_3 \Delta C_t + \epsilon_t.$$

 $\sum_{i=0}^{\infty} ((\lambda \delta)B)^i$ may be viewed as a convergent geometric series with solution $1/1 - (\lambda + \delta)B...$

And on page 1051, in the sentence following Equation (20)

$$\Delta W_{t-1}$$
 should read $\sum_{i} \Delta W_{t-i}$, and ΔP_{t-1} should read $\sum_{i} \Delta P_{t-i}$.

An error by our printer supplied each June APSR with two copies of page 639 and omitted page 638, which contained the bulk of a review of Plato's Republic, translated by G. M. A. Grube, by Thomas L. Pangle. The entire review can be found on pages 1336-1337 of this issue.

Articles Accepted for Future Publication

Joel D. Aberbach, University of Michigan, "Power Consciousness: A Comparative Analysis"

Paul R. Brass, University of Washington, "Party Systems and Government Stability in the Indian States"

Paul Burstein, Yale University, "Social Cleavages and Party Choice in Israel: 'A Log-linear Analysis'"

Melissa A. Butler, The Johns Hopkins University, "Early Liberal Roots of Feminism: John Locke and the Attack on Patriarchy"

Arthur DiQuattro, University of Washington, "Alienation and Justice in the Market"

Lloyd S. Etheredge, Massachusetts Institute of Technology, "Personality Effects on American Foreign Policy, 1898-1968: A Test of Interpersonal Generalization Theory"

John A. Ferejohn and Roger G. Noll, California Institute of Technology, "Uncertainty and the Formal Theory of Political Campaigns"

Morris P. Fiorina and Charles R. Plott, California Institute of Technology, "Committee-Decisions Under Majority Rule: An Experimental Study"

Mark N. Franklin, University of Strathclyde, and Anthony Mughan, University College (Cardiff, Wales), "The Decline of Class Voting in Britain: Problems of Analysis and Interpretation"

Norman Frohlich, Joe A. Oppenheimer, Jeffrey Smith, and Oran R. Young, University of Texas at Austin, "A Test of Downsian Voter Rationality: 1964 Presidential Voting"

- Norman Furniss, Indiana University, "The Political Significance of the Property Rights School"
- Dante Germino, University of Virginia, "Eric Voegelin's Framework for Political Evaluation in His Recently Published Work"
- James L. Gibson, University of Wisconsin at Milwaukee, "Judges' Role Orientations, Attitudes, and Decisions: An Interactive Model"
- Robert T. Golembiewski, University of Georgia, "A Critique of 'Democratic Administration' and Its Supporting Ideation"
- George D. Greenberg, Jeffrey A. Miller, Lawrence B. Mohr, and Bruce C. Vladeck, University of Michigan, "Developing Public Policy Theory: Perspectives from Empirical Research"
- John G. Gunnell, State University of New York at Albany, "The Myth of the Tradition"
- Douglas A. Hibbs, Jr., Massachusetts Institute of Technology, "Political Parties and Macroeconomic Policy"
- Barbara Kellerman, Tufts University, "Mentoring in Political Life: The Case of Willy Brandt"
- Samuel Kernell, University of Minnesota, "Explaining Presidential Popularity"
- James H. Kuklinski, Wichita State University, "Representativeness and Elections: A Policy Analysis"
- Arthur J. Lerman, Yeshiva University, "National Elite and Local Politician in Taiwan"
- Richard D. McKelvey, Peter C. Ordeshook and Mark D. Winer, Carnegie-Mellon University, "The Competitive Solution for N-Person Games Without Transferable Utility, With an Application to Committee Games"
- Manus I. Midlarsky, University of Colorado, "Analyzing Diffusion and Contagion Effects: The Urban Disorders of the 1960s"
- David W. Moore and B. Thomas Trout, University of New Hampshire, "Military Advancement: The Visibility Theory of Promotion"
- Edward N. Muller and Thomas O. Jukam, State University of New York at Stony Brook, "On the Meaning of Political Support"
- John M. Orbell, University of Oregon, and L. A. Wilson II, University of Nevada, "Institu-

- tional Solutions to the N-Prisoners' Dilemma"
- A. Kenneth Organski, University of Michigan, and Jacek Kugler, Boston University, "The Costs of Major Wars: The Phoenix Factor"
- Charles W. Ostrom, Jr., Michigan State University, "A Reactive Linkage Model of the U.S. Defense Expenditure Policymaking Process"
- John G. Peters and Susan Welch, University of Nebraska-Lincoln, "Political Corruption in America: A Search for Definitions and a Theory, or, If Political Corruption Is in the Mainstream of American Politics Why Is It Not in the Mainstream of American Politics Research?"
- David E. Price, Duke University, "Policy-making in Congressional Committees: The Impact of 'Environmental' Factors'
- Steven J. Rosen, Institute of Advanced Studies (Camberra, Australia), "A Stable System of Mutual Deterrence in the Arab-Israel Conflict"
- Howard Rosenthal, Carnegie-Mellon University; and Subatra K. Sen, University of Rochester, "Spatial Voting Models for the French Fifth Republic"
- Arlene W. Saxonhouse, University of Michigan, "Comedy in Callipolis: Animal Imagery in the Republic"
- Lars Schoultz, Miami University, "The Socio-Economic Determinants of Popular-Authoritarian Electoral Behavior: The Case of Peronism"
- Donald D. Searing, University of North Carolina, "Measuring Politicians' Values: Administration and Assessment of a Ranking Technique in the British House of Commons"
- Goldie Shabad, University of Chicago, and Sidney Verba, Harvard University, "Workers' Councils and Political Stratification: The Yugoslav Experience"
- Donald T. Studlar, Centre College of Kentucky, "Policy Voting in Britain: The Colored Immigration Issue in the 1964, 1966, and 1970 General Elections"
- Robert Weissberg, University of Illinois at Champaign, "Collective vs. Dyadic Representation in Congress"

Robert A. Dahl and the Study of Contemporary Democracy: A Review Essay*

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More than ten years ago Robert Dahl set forth his approach to the systematic study of politics in a slender volume entitled Modern

*This review will focus on the following major articles and books:

"The Science of Public Administration: Three Problems," Public Administration Review, 7 (Vinter, 1947), pp. 1-11.

Congress and Foreign Policy (New York: Harcourt

Congress and Foreign Folicy (New York: Harcourt Brace and Co., 1950. Pp. 305.)

Domestic Control of Atomic Energy. With Ralph S. Brown, Jr. (New York: Social Science Research Council, 1951. Pp. 110.)

Politics, Economics, and Welfare. With Charles E. Lindblom. (New York: Harper and Row, 1953. Pp. 552)

A Preface to Democratic Theory (Chicago: University

of Chicago Press, 1956. Pp. 154).
"The Concept of Power," Behavioral Science, 2 (July, 1957), pp. 201-215.

"A Critique of the Ruling Elite Model," American Political Science Review, 52 (June, 1958), pp. 463-469.

Who Governs? (New Haven: Yale University Press, 1961. Pp. 355.

"Letters to the Editor: Thomas J. Anton's 'Fower, Pluralism, and Local Politics'," Administrative Sci-

ence Quarterly, 8 (Sept. 1963), pp. 250-268. Modern Political Analysis (Englewood Cliffs, N.J.: Prentice Hall Inc., 1st ed., 1963, 2nd ed., 1970. Pp. 118 (1st ed.).)

Pluralist Democracy in the United States: Conflict and Consent (Chicago: Rand McNally and Co., 1st ed., 1967. Pp. 471. 2nd ed., 1972, entitled Democracy in the United States: Promise and Performance.)

Political Oppositions in Western Democracies. Editor and Contributor. (New Haven: Yale University Press, 1966. Pp. 458.)

"The City in the Future of Democracy," American Political Science Review, 61 (Dec., 1967), pp. 953-970.

After the Revolution? (New Haven: Yale University Press, 1970. Pp. 171.)

Polyarchy: Participation and Opposition (New Haven: Yale University Press, 1971. Pp. 257.)

Regimes and Oppositions. Editor. (New Haven: Yale

University Press, 1973. Pp. 411.)

Size and Democracy. With Edward R. Tufte. (Stanford: Stanford University Press, 1973. Pp. 148.)

The above is by no means a complete bibliography of Dahl's published writings. In the text I have referred to, and footnoted, several further essays. On various Political Analysis. No political scientist of the period could claim that title for his treatise with more justice. A decade later the claim remains unshaken. The corpus of Dahl's works has not merely hastened the transformation of American political science from a subdivision of historical studies into a vigorously autonomous discipline; it continues to epitomize that transformation. His name, now as for nearly twenty years, inevitably appears on any list of the most distinguished contributors to the field. To review his writings is to inform oneself of the standards of excellence most generally accepted within the profession.

So much is by now no longer in controversy. And yet Dahl's pre-eminence among political scientists cannot readily be explained to those who have not read his work. At no point in his career has he revolutionized our way of looking at politics through a radical reconceptualization of the categories we employ to discuss its elements. Nowhere has he drawn the organizing premises of his multitudinous essays into a single sweeping metatheory. His writings, though often elegantly parsimonious, have generally lacked the architectonic qualities of rational behavior theory at its best. Save in Who Governs?, he has not sought to attain the anthropological richness, the literary imaginativeness, and the sophisticated realism of the outstanding monographs in the field. Dahl's name is associated with no one major methodological innovation, nor can he be said to have established any one finding or disproved any one hypothesis as conclusively as several less celebrated contributors to this Review. His incisive critiques and fine-honed distinctions have left their imprint throughout political science, yet the number of top-quality studies

grounds - mostly because, in my judgment, the principal ideas have been more forcefully stated in one of the above works — I have not included these other essays in what I take to be "the essential Dahl."

of contemporary governments that have borrowed wholesale from his constructive schema are surprisingly few. He is remarkable neither for the force of his moral concern nor for his capacity to stimulate fresh appraisals of familiar philosophical positions. While Dahl is in his own way as "rare" at definitions as Thomas Hobbes, and certainly far more ingenious in his use of diagrams and data, one seldom quotes him with the aesthetic satisfaction the *Leviathan's* leading passages inspire.

Why then is Dahl so esteemed by all but those who wish to belabor him as the prime symbol of Establishment complacency toward the evils of American pluralist democracy? The answer is in part, of course, that while along any one dimension one can cite more outstanding practitioners of the art, the list is seldom a long one. If some political scientists are more adept at system-building, if others draw more freely and more richly on a wider range of comparative data, if some have developed and applied the techniques of behavioral analysis to greater advantage while others have seen more deeply into the inner meaning of these findings. there are few who have combined these various capacities at so high a level. But Dahl's standing rests on much more than his readers' ability to calculate excellence along multidimensional margins. His special strengths are those that have always served social scientists well logical acumen, skill in analytic classification, sound judgment, sheer knowledgeability, a strongly developed sense for a line of argument, and a quick eye for the significance of others' work in relation to his own. In these respects, he has few rivals. Dahl's writings all have the stamp of purposive authority on them; there are few grey patches, few that are logically slipshod, tendentious, manifestly untenable or trite, and none at all that exhibit mere technical virtuosity for its own sake. They are classical if the term can be applied at so short a time horizon — in the sense that Aristotle's writings are classical: one turns to them repeatedly to remind oneself what a judicious social scientist, habitually inclined to assimilate and categorize all data in its most fruitful form, regards as the most important questions to ask of his world.

Continuity Amid Diversity

As one surveys the totality of Dahl's published work to date, one is struck not only by its volume, density, and fecundity, but by its sheer variety. Public administration, secrecy in government, foreign policy, judicial decision making, the formal logic of political argument, economics and politics, community govern-

ment, pluralism, the concept and study of political power, comparative oppositional politics, governmental size and civic participation — on all these topics (a small fraction of the ones he has taken up) Dahl has left his distinctive mark, and the subject has never been the same again. Exceptional effort is required to identify central concerns of the discipline to which he has not addressed himself with more than ordinary penetration.

Yet behind this superficially disparate range of interests one can discern a single unifying thread of inquiry. For more than a quarter of a century, Dahl has sought to analyze modern democratic government - its operation, the conditions that encourage or threaten it, the logical status of the propositions in terms of which it is discussed and evaluated. This inquiry has led him in many directions. In his early years, it pointed toward assessing the competence required of citizens in a democracy in an era of nuclear war and unprecedented expansion in the scope of governmental responsibilities. Later, it drew him toward conceptual clarification of the relationships purportedly prevailing between governors and governed; still later, toward establishing the social matrix within which democratic government occurs. Always, however, his questions have been organized by a continuing effort to illuminate our understanding of the distinctive political institutions - all relatively recent social inventions, as Dahl has repeatedly reminded us - that have enabled the members of large-scale societies to enforce some significant degree of correspondence between their self-defined preferences and the decisions of those who govern them.

But if Dahl's focus on modern, large-scale democratic government has remained unwavering, his treatment of the subject exhibits marked discontinuities of content, scope, and level of abstraction over the years. Periodization of a scholar's work is inescapably an artifice. It ignores ongoing research and thought, underplays incongruent subordinate themes, magnifies differences and minimizes continuities in central tendencies, and it too often confuses publication dates with states of mind. These well-known pitfalls acknowledged, Dahl's publications do lend themselves to being instructively grouped into four periods. Thus from the end of the Second World War until the

¹The following periodization is based strictly on Dahl's published work, and not at all on more direct evidence as to the evolution of his scholarly interests. Hence publication dates, rather than the period of active research and writing, are treated here as controlling.

beginning of the Eisenhower Era, he mainly addressed himself to selected operational and moral aspects of American governmental institutions and policies. Thereafter, until perhaps the close of the decade, his work became notably more universalized, more formal, more explicitly concerned with the logic of his own and others' arguments. A third period, inaugurated with his close empirical study of politics in New Haven and ending amid the growing disorder of American public life at home and abroad, marked a return to the study of contemporary American politics - this time, however, with the declared object of locating and assessing the capacity of various groups to control the terms of governmental policy decisions. The fourth (and so far, final) period has been characterized by a dramatic expansion of purview in both space (from the United States to the globe) and time (from Ancient Athens to serious consideration of utopian futures) as Dahl has sought to enhance our understanding of the implications of governmental size for civic participation and to explicate the relationship of various social and political factors to a peculiarly significant form of such participation the participation of loyal oppositional groups in the work of democratic governments.

Continuities and incongruities show that this scheme is not thoroughly accurate. Thus, Politics, Economics, and Welfare (1953) is as much a summation of Dahl's previous work on American public policy as it is a bold effort to resolve all policy-making processes into four basic techniques of control. Modern Political Analysis (1963) appeared after Who Governs? (1961), but looks back to Dahl's earlier preoccupation with the general dimensions of political power. Pluralist Democracy in the United States was published one year later than Political Oppositions in Western Democracies. A record of organic scholarship is less amenable than Gaul to valid segmentation. Nevertheless, these subdivisions should provide a useful perspective on the evolution of Dahl's interests.

Democracy and Civic Competence

Dahl first attracted general scholarly notice with a subject to which he was never again to return. "The Science of Public Administration: Three Problems" (SPA, 1947) is very much a piece of its times. It addresses itself to the case several "theorists" of public administration had made out for their "science" over the previous two decades. Using awesomely complex diagrams, systematically numbered propositions, and sometimes even algebraic notation intimating quantifiability in principle, the leading

writers in this field had presented their ideas in universalistic terms suggesting fusion of pure deductive science with practical prescription.² Although many were less interested in public administration than in other kinds of formal organization, their proclaimed achievements necessarily compelled the attention of all political scientists who aspired to introduce rigorous systematization into their own discipline.

Dahl's object was to show that these imposing claims rested on a fragile foundation. Writers in public administration could not expect to construct a pure science of administration, he contended, because "[S] cience as such is not concerned with the discovery or elucidation of normative values....[S] cience cannot construct a bridge across the great gap from 'is' to 'ought'," whereas the appraisal of administrative design and performance inescapably entails some choice as to the ends an administrative arrangement is to serve.³ Terms like "efficiency" and "responsibility" either are empty of content or contain unacknowledged assumptions regarding the ends to be served or ignored.⁴ This point could only be overlooked by the proponents of "value-free principles" of administration because of their commitment to an empirically inadequate model of organizational man in which he appears as a "rational" creature who seeks only to maximize the goals of his organization.

Once it is recognized that participants in administration may seek a multiplicity of goals both inside and outside the formal organization of which they are members, the notion of an abstractly universal science of administration, wholly independent of the character of the organization or of the cultural and political setting in which its members operate, necessarily vanishes into thin air. Administrative science must therefore become comparative. It must rest on field research in different cultural and social settings, and it must recognize the diversity of human motivations. It must take into account the special constraints on human behavior in publicly accountable administrative organizations, and it must investigate the implications for administration of the different

²The locus classicus of these now gleefully belabored pretensions was, of course, the *Papers in the Science of Administration*, ed. L. Gulick and L. Urwick (New York Institute of Public Administration, 1937).

 $^{^{3}}SPA$, pp. 1; 2–4.

⁴Cf. the similar – and, as a matter of historical record, the more memorable – formulation of this argument that same year in Herbert Simon, *Administrative Behavior* (New York: Macmillan Co., 1947), chap. 2.

terms on which the political life of the various nations has been organized.⁵

These arguments mark Dahl's debut in the arena of scholarly controversy. It was an impressive debut. One notes already in this article the qualities for which he was later to be celebrated — among these, a sure sense for the truly fundamental issues in the field, and an ability to characterize other people's positions (and his own as well) concisely, carefully, and with great lucidity. Likewise evident is a keenly developed critical orientation that presses controversies up to a plane of logical generality at which the implications of the resolution become compelling for all members of the discipline.

It must also be said that in this article Dahl succumbed to one of the rare recorded fallacies in his long career. For on inspecting his conception of a "science" of administration, one realizes that he uncritically accepted his opponents' conception of administrative "science" as instrumental prescription, adding only that the values served should be made explicit. In thus confounding positive cause-and-effect propositions with "If ... then" technological prescriptions, Dahl simply obscured the possibility that descriptive statements of the former order about administration can be formulated and tested. It may well be, as Gunnar Myrdal has argued for the last forty years, that most bodies of purportedly scientific doctrine in the social sciences reveal under critical inspection many opportunistically obscured value premises, and that the best safeguard against this tendency is to address oneself to explicitly formulated ideals.⁶ But Dahl has argued otherwise on many occasions; and certainly it cannot be said that in this essay he self-consciously denied the possibility of a purely descriptive social science of administration. Indeed, it is all too clear he did not consider that alternative at all.

Whatever the epistemological niceties of this seemingly unresolvable debate, Dahl clearly adhered to his own prescription in his first full-length book. Congress and Foreign Policy

⁷In the intervening three years, two of Dahl's least influential articles appeared in the professional journals of the period. "Worker Control of Industry," American Political Science Review, 41 (October, 1947), 875–900, is essentially a summary of the debate within the postwar British Labour Party concerning the institutional implications of its commitment to increase worker participation in the management of industry. "Marxism and Free Parties,"

(CFP; 1950) is an unabashedly value-oriented treatise. Given, Dahl writes, that the policy-making processes of a democracy should facilitate civic agreement while resulting in "rational" decisions and the maintenance of "responsible" leadership, how well does Congress perform at present in relation to these ends in the foreign policy arena, and what are the prospects for improvement? Such questions obviously derive from a tradition of normatively self-conscious scholarship that was on the verge of being displaced by the "behavioral revolution." Even in their more general form these questions have seldom received a better answer.

Some purely verbal ambiguity attended two of Dahl's three key concepts at the outset. If "rational" policies are those "best designed to achieve the purposes agreed upon ... that are, in other words, best calculated to satisfy, in the real world of international politics, the desires and aspirations underlying the agreement," then "rationality" would seem to have less to do with the state of mind of the decision makers - the clear focus of Dahl's subsequent chapters - than with the proven effectiveness of their choices. Policies might well prove retrospectively "rational" in this sense through a happy conjunction of outcomes brought about through a president's reliance on private astrological forecasts and outcomes he was publicly pledged to seek. How rationality can aid in establishing a proper trade-off among the policy objectives of different constituencies is also unclear. The term "responsible leadership" invites many alternative constructions, of which Dahl adopted one - "that it selects policies based upon the preferences of the citizen" without further argument.8 And no one has shown more clearly than Dahl himself in his subsequent work how many logical problems a strict constructionist would confront in seeking

Journal of Politics, 10 (Nov., 1948), 787–813, is a somewhat more ambitious enterprise. Nowhere else has Dahl assessed in print at such length the adequacy of Marxism as a theory of politics. The relative obscurity of the article is presumably attributable to Dahl's more extensive development in his later work of its central ideas on majority rule and the role of parties in political conflict.

⁸For Dahl's formal definitions of his three value concepts, see *CFP*, pp. 6–7. In effect, he sided with Herman Finer in the latter's celebrated debate with Carl Friedrich on the concept of "responsibility." See C. Friedrich, "Public Policy and the Nature of Administrative Responsibility," *Public Policy*, 1940 (Cambridge, Mass.: Harvard University Press, 1940), pp. 3–24; and the rejoinder by Herman Finer, "Administrative Responsibility in Democratic Government," 1 *Public Administration Review*, (Summer, 1941), 335–350.

⁵SPA, pp. 5-11.

⁶See, e.g., his *Objectivity in Social Research* (New York: Pantheon Books, 1969) and the reference to his other works cited therein.

to make such a selection.

Once under way, however, Dahl removes all doubt as to his meaning. The stability - indeed, the survival - of democracy in America is threatened, he argues, by a rapidly widening disjunction. On the one hand, the level of informed competence regarding foreign affairs so far attained by the American citizens at large and all but a handful of their representatives in Congress remains alarmingly modest. On the other, small groups of decision makers (largely centered in the executive branch) have accumulated a quite overwhelming expertise based on their unity, specialized training, intensely focused interest, and access to pertinent and sometimes secret information. This distinctive competence has enabled these policy elites to make plausible their claim that they alone are equipped to assess and respond to the crises arising from America's newly assumed global responsibilities in an era of potential nuclear war.

From this perspective, Dahl's three value criteria become intimately related, with "rationality" the strategic factor. Unless civic and congressional competence in foreign affairs can be drastically upgraded, Dahl contends, congressional efforts (and those of the electorate) to hold foreign policy experts accountable for their decisions can only be ineffectual or disastrously misguided. Yet Congressional insistence on detailed accountability, however unfortunate its effects, must be expected as long as a gap in shared understanding of foreign policy problems impedes the development of mutual agreement on goals and a consequent willingness on the part of Congress to entrust the executive branch with the discretion necessary for their implementation. In the absence of such agreement, congressional abandonment of this claim would signal the ebbing vitality of the American democratic system.

Unhappily, Dahl finds good reason to argue that neither the general circumstances of the electorate in a mass democracy nor the specific policy processes of the American form of government hold out much promise of overcoming the estrangement of the president and his foreign policy experts from the other politically accountable decision makers of the system. The round of life of the citizen does not encourage the accumulation of reliable, low-cost, pertinent information on foreign affairs; neither do the qualities required for success in American electoral politics. The constitutionally prescribed separation of powers builds in a fundamental tension between the legislative and executive branches of the national government that the weak party system

rarely bridges. Dahl's prognosis is gloomy indeed: since little will result from efforts to raise the citizens' level of informed participation in foreign affairs, the prospect is one of increasing drift toward unfettered presidential discretion and the resultant growth of a "plebiscitary dictatorship" in the foreign policy arena.⁹

Even this sketchy outline of an intricate, richly elaborated thesis should be sufficient to show the larger dimensions of Dahl's treatise. Although his immediate subject is the role of Congress in the foreign policy process, he uses his analysis to bring into focus the more general question of a democracy's capacity to cope with challenges demanding swift, united responses based on specialized knowledge in an atmosphere of secrecy and potential emergency. Moreover, the discussion is as rewarding in its parts as in the whole. Its pages abound with analytic observations clearly foreshadowing major arguments in his later works. Thus, for example, he already points out here that "majorities" in a pluralistic society are largely made up of minorities; and that in democratic societies the range of discretion tends to decrease as the range of knowledge increases. 10 Many of his theses retain a disturbing contemporary relevance. Readers in the 'seventies will find no reason to dismiss his warnings against permitting presidents to seek triumphs of secret diplomacy unencumbered by a sense of responsibility for enlightening Congress and securing its comprehending assent to the ends pursued. With memories of Vietnam still fresh, we can perceive a painfully deepened force to his plea that, in the long run, democratic theory does not offer sanction "for a majority of fifty-one per cent to ask a minority of fortynine per cent to offer up their lives to support a foreign policy with which they, the members of the minority, fundamentally disagree."11 And those who have become accustomed to think of Dahl as an uncritical apologist for American pluralism might be startled to encounter the following passage:

[C] ompetent choice on questions [of foreign policy] requires a preliminary process of discussion and inquiry, not only to clarify reality, but to discover what one's preferences are in that context of reality and what, if anything, may be done to realize those preferences in the real world ... [But] Modern urban life is not conducive to the kind of intermediate organizations implied by, say, the neighborhood discussion group. Individual hostility, anonymity, and

⁹CFP, pp. 63-65, 263-264.

¹⁰Ibid., pp. 55, 142.

¹¹*Ibid.*, pp. 180–181, 223.

moral isolation; the physical layout of the city; the bothersome aspects of transportation; the lack of facilities for group life; the intensified satisfactions of passive participation, exploited by highly organized and influential commercial and advertising groups; the centralization of control over techniques of influencing attitudes and the consequent standardization and banality of opinion — all militate against the spontaneous growth of local discussion and inquiry groups.¹²

Indeed, perhaps the most enduringly interesting theme within Congress and Foreign Policy is Dahl's concern at this point with improving the quality of democratic participation. Again and again, in tones reminiscent of John Stuart Mill and John Dewey, he deplores the extent to which the outcomes of political activity in America represent manipulative bargains among self-interested elites rather than the fruit of an informed sense of collective purpose. The process by which agreement is reached appears to him quite as important as its substance. "[I]t is one thing to arrive at policy by a process that searches out those common purposes on which various and often conflicting minorities may nevertheless agree," he wrote, but "...quite another to make policy by patchwork-compromising that concedes everything to the special claims of an articulate minority, and nothing to the preferences of an unmobilized and inarticulate majority."13 It is this perspective, as much as anything, that leaves one feeling that Dahl's first book is still one of his most satisfying.

The implications of secrecy and technical expertise for competent popular participation and effective control of bureaucratic activity by democratically elected bodies were obviously very much on Dahl's mind at this time. For the following year, acting on a commission from the Social Science Research Council, he coauthored with Ralph S. Brown, Jr. a pamphlet on possible subjects for research on the Domestic Control of Atomic Energy (1951). Once again one encounters the themes made familiar in his previous work - the difficulties in persuading an apathetic public to take an intelligently informed interest in collectively consequential matters of little immediate benefit or loss to any one citizen; the problems of lay supervision of experts; the deadening effect of security regulations on public debate, and the consequent possibility of ignorant or irrational public reaction to technically rational policy decisions. These are themes, as Dahl had shown, that can be linked to the most funda-

mental and searching questions about the prospects for democracy. They do not receive that treatment here. Partly the problem lies in the format. The pamphlet is, in effect, a 110-page exhortation to other political scientists to undertake research the authors find interesting but do not propose to undertake themselves. The inconclusive hypotheses, the many questions without answers, do not make stimulating reading. But there is also an extreme unevenness of treatment, with many thin chapters and a concluding section on "general theory" that is little more than a recapitulation. It is all too plain that this time Dahl did not rise to the potential of his subject. In sharp contrast to his preceding work, Domestic Control of Atomic Energy does not reward the reader who returns to it.14

The Logic of Democracy

Shortly after this final foray in the field of substantive public policy, Dahl's published work underwent a dramatic change of character. Hitherto, despite the markedly analytic quality of his arguments, Dahl had kept his specific subjects in firm focus at a relatively short range. Now, however, he apparently felt ready to make explicit, and to organize into sustained theses, the general concepts and theoretical propositions implicit in these arguments. The result was an outpouring of essays and books that firmly established him among the very front rank of political scientists. 15

Politics, Economics, and Welfare (PEW; 1953), the first of these publications, is in some respects the most creative book Dahl ever

¹²*Ibid.*, pp. 88, 90.

¹³Ibid., p. 51.

¹⁴A somewhat similar problem beset an article by Dahl that appeared some eight years later in this Review on the possibilities for research on the interconnections between business firms and politics. The individual points are generally well made; but most had already been made better in actual research reports, and without exceptional effort one can think of other important questions that might well have been included. This particular genre of essay seems to be falling out of favor. On the evidence provided by even so able a social scientist as Dahl, its demise does not seem indisputably regrettable. See Dahl, "Business and Politics: An Appraisal of Political Science," American Political Science Review, 53 (March, 1959), 1-34.

¹⁵This change of character was seemingly quite evident to his readership. Among the ten recorded reviews of *Congress and Foreign Policy*, nine were published in the popular press. Henceforward, although Dahl's work has been largely lost to sight for the casual lay reader, it has regularly received widespread coverage not only in the political science journals but in the book review sections of the standard sociology and economics journals as well.

wrote.¹⁶ In this book he and his economist collaborator Charles Lindblom addressed themselves uncompromisingly to the most general properties and problems of rational social action. These they found to be embedded in the techniques of calculation (including both the scheduling of goals and the processing of information pertinent to the selection of appropriate strategies) and in the social mechanisms relied on to control behavior in ways required to implement the resulting decisions.

In one of the boldest and potentially most fruitful simplifications of the decade, the two authors contended that calculation and its accompanying controls formed the central core of most organized social interaction. Furthermore, they showed that activities directed toward carrying out these two operations could be resolved into one or more of four elemental control mechanisms - market pricing, bargaining, hierarchy, and the institutionalized form of competition among leaders for needed support from nonleaders that Dahl and Lindblom called "polyarchy." Having drawn attention to structural similarities among control processes conventionally discussed in nominalistic terms, they undertook repeated comparisons of the four control techniques in an imposing variety of specific policy contexts. Conversely, they demonstrated the sterility of many contemporary controversies that failed to disaggregate such hypostasized compounds as "bureaucracy" or "planning" into their component control techniques.17

Looking back through the political science — or, for that matter, the social sciences generally of this period — one can find few analytic schemata as comprehensive in scope, as systematically elaborated in detail, and as searchingly applied to societal processes. Yet many of Dahl's later works — works less adventurous in

16PEW was coauthored with Charles E. Lindblom of the Yale Economics Department. According to the Preface: "Many works appear as 'collaborations,' but in few can the dialectical process — in the Greek, not in the Marxist, sense — have been more heavily relied on. As much as any work by two people can do, each chapter represents the single product of two minds" (p. xxiii). I have taken the authors at their word, and shall make no effort here to detect and disentangle their individual contributions. For present purposes, this decision entails ascribing to Dahl whatever ideas appear in the book. Those interested in assessing Lindblom's contributions might wish to consult his subsequent essays on bargaining and decisional strategy, and most particularly The Intelligence of Democracy (New York: Macmillan Co., 1965), although the danger of unwarranted inference is real.

17See, e.g., the instructive series of charts in *PEW*, pp. 10-17, on the scope of governmental economic controls.

conception, less general in their application — were to attract far more sustained attention than this one. It may therefore be instructive to ask why *Politics, Economics, and Welfare* was seemingly more often footnoted than closely read within the discipline, and why it has now been steadily dropping out of the footnotes.

One reason may simply be that it was too original for its time. Reviewers obviously had difficulty in classifying the work and in relating it to other studies. Everyone could see that it stood at the interface of economics and political science. But in precisely what sense was it interdisciplinary? Many economists judged it harshly, correctly contending that it made no direct contribution whatever to the development of positive economic theory. Sociologists and political scientists were generally more respectful but were unable to see in it either a report on specific findings or a satisfactory explanation for such findings. ¹⁸ Like Talcott Parsons's and Neil Smelser's Economy and Society - with which, in my opinion, Politics, Economics, and Welfare would repay close comparison - it offered numerous stimulating metatheoretical perspectives without pointing directly to any particular line of research. 19

It may well be that the authors themselves did not clearly understand what they had accomplished. There is some evidence to support this suggestion. Despite their extreme concern for establishing systematic linkages among their concepts, they never really show how their four central sociopolitical processes (or techniques - the designation remains unstable) might be arranged along an explicit set of dimensions. Perhaps this omission is attributable to the nonanalytic origins of their terms. The concepts of market pricing, hierarchy, polyarchy, and bargaining appear in fact to be drawn from descriptions of the dominant decisional practices within four substantive societal arenas - the conventionally conceived economic marketplace (i.e., the competitive or semicompetitive private sector of capitalist systems and the semi-autonomous productive sector of such decentralized socialist systems as

18Interestingly enough, one of the most favorable reviewers was C. Wright Mills, who found in the constructive utopian potential of the book's discussion and appraisal of alternative techniques a welcome break from the fact-bound descriptiveness of statusquo political science. See his review in the American Sociological Review, 19 (August, 1954), 495–496.

19Cf. Talcott Parsons and Neil Smelser, Economy and Society (Glencoe, Ill.: The Free Press, 1956). Unfortunately, Parsons has never addressed himself as directly to Dahl and Lindblom's scheme as he has to the work of several other contemporary political scientists.

the Yugoslavian); the internal organizational arena of monopolies and administrative agencies; the arena of competitive electoral politics in large-scale modern democracies; and the intragovernmental arena of lobbies, parliamentary committees, and executive elites (with, as an economic analogue, the relations prevailing among trade unions, oligopolistic producers, and monopsonies). But Dahl and Lindblom are quite insistent that their scheme is analytic, not merely descriptive. Quite correctly, they argue that combinations of these techniques are to be found to some extent in all of these arenas.

Alternatively, their scheme might be viewed as a characterization of the terms on which decisions are made in allocative systems having structural properties that range from those of competitive markets (the pricing system) through oligopoly (polyarchy) to monopoly (hierarchy). Arranged this way, the three techniques so cited would be graded according to the "vertical" control relationships prevailing between "producers" and "consumers"; but then bargaining could then only be fitted into the scheme as the "horizontal" control dimension of systems structurally similar to polyarchies.20 Again, market pricing, bargaining, and hierarchical allocation of resources would seem to occur continuously, whereas the polyarchal component of government appears confined to the selection of leaders and perhaps to the scheduling of goals. With some ingenuity, no doubt, a reviewer could fit together these various processes into something like a systematic fourfold table. The significant fact remains that the authors did not do so themselves.21

Compounding these conceptual problems is one of exposition. *Politics, Economics, and Welfare* is a book that generates an enormous number of cells the authors feel compelled to fill. Sometimes the results are dazzling. The two short chapters on hierarchy bear favorable

comparison with Max Weber's celebrated passages on bureaucracy. The comparison of collective with market choice deftly disposes of a great body of fallacious reasoning on the subject, while the subsection on aids to rational calculation truly transforms the reader's analytic perception of some thoroughly familiar political techniques.²² On the other hand, the discussion of social economizing and the pricing system is simply stupefying for anyone who has been exposed to the elements of economic theory, while the obligatory condensed treatment of "freedom" leaves the conviction that the authors should have said either much more or much less.23 The most determined reader's mind clots somewhere amid the many long, grey, embarrassingly banal passages on contemporary governmental activity, particularly in the unsatisfactory sections on bargaining. The limp postscript does disservice to the authors' achievement. Politics, Economics, and Welfare, one concludes, is a good two- to three-hundred pages too long. The pity of it all is that these superfluous pages so inextricably interlard some of the most brilliant paragraphs in the social sciences as to deter the scholar from reading any at all.

These blemishes were notably absent from Dahl's next book, A Preface to Democratic Theory (PDT; 1956), one of the leanest works in the history of American political science. Dahl has never written another book as rigorous, as condensed, as foundational, and as pellucid in style as this one. In barely 150 pages he moves assuredly through the central controversies that must arise in any serious, close discussion of democratic theory, crisply indicating not only how one might resolve these questions but even more how they should be formulated so as to admit of orderly and conclusive answers. The treatise attracted much attention at the time as one of the first in political science to make extensive use of the notational devices of symbolic logic. Yet Dahl does not gloss over conceptual ambiguities in the interest of formalization; on the contrary, he elucidates them. In explicating the choices one must make in order to convert loose verbal statements into the demandingly precise propositions of formal logic, he serves as a teacher of the highest order.

Understandably, but somewhat unfortunately, the attention of most critics quickly settled on the opening chapter of the book. Here Dahl set out to show that James Madison,

²⁰Moreover, Dahl and Lindblom specifically assert that in their scheme the pricing system includes far more than purely competitive pricing practices. See *PEW*, pp. 193 ff.

²¹One consequence of not working out explicit dimensions in *PEW* for the four politico-economic techniques is that they are certainly not exhaustive and possibly not exclusive. To judge from the chart on p. 193, and the discussion elsewhere, "pricing" appears to contain elements of both bargaining and hierarchy. And among the various control techniques not considered in the scheme, the control induced by appeal to previously accepted standards — as, for example, in much judicial decision making — is a particularly conspicuous omission. As a general rule, but without systematic critical defense of the position, Dahl has been inclined to discount heavily the efficacy of this particular form of control.

²²*Ibid.*, pp. 227–271;413–438;64–88.

²³Ibid., pp. 129-226; 28-38.

often praised as a polemicist of matchless lucidity, in truth was censurably careless in both definition and construction of argument. On his own grounds, Dahl was indisputably successful. Lucidly and conclusively, he showed that Madison begged crucial questions in his use of "faction" and "majority tyranny"; and that when they are not begged, Madison's most celebrated arguments became substantially less determinate than admiring commentators had previously perceived. And this perception is not trivial. "Madisonian ideology," Dahl contended, "is likely to remain the most prevalent and deeply rooted of all the styles of thought that might properly be labeled 'American'."24 Since Madison's persuasive definitions have been operationalized to sanctify patterns of authority and procedural rules at odds with many powerful alternative conceptions of democracy, his ambiguities have clearly invited opportunistic exploitation.

Many readers, nevertheless, have been left with the unsettling sensation that a formidable logical apparatus has been brought to bear against its target with a power disproportionate to the demands of the subject matter. Dahl's critique springs out of a philosophical tradition, and is directed at a contemporary audience, that in principle profess to take nothing for granted. But Madison's world was a very different one. His argument becomes much more defensible as practical ideology, if not in ultimate philosophical principle, if one allows that he was writing for an eighteenth-century post-Revolutionary War public to which such terms as "natural rights" and "tyranny" (the severe deprivation of "natural rights") had a reasonably operational shared meaning. Because one cannot specify the scale on which tyranny shades into imposition of unwelcome policies, the former concept does not thereby necessarily become meaningless.

In other ways, Dahl presses his critique too hard. He persuasively argues that any arrangement requiring the assent of more than simple majorities reciprocally invites tyranny by minorities; but he is (characteristically) rather too inclined to discuss the issue in terms of veto power, and too little disposed to give weight to the educative value of the dialogues that may arise when constitutional checks can be invoked to retard instant alteration of basic rules.²⁵ He is also not above treating as a serious definitional proposal Madison's rhe-

torical declamation to the effect that "the accumulation of all powers, legislative, executive, and judiciary, in the same hands ... may justly be pronounced the very definition of tyranny."²⁶ Again, he converts Madison's belief that governmental systems must guard against the possibility of tyranny — an outcome that Madison (unlike Hamilton) seems to have regarded as of relatively low probability — into a flat hypothesis that any given individual or group of individuals will tyrannize over others if unrestrained by external checks. ²⁷ So construed and scrutinized, Madison's thesis quickly becomes more of an intellectual shambles than a more sympathetic reading might suggest.

Perhaps because the rest of the book deals with no specific treatise, its argument seems less captious, more truly elucidative. One chapter offers, and criticizes, a maximizing model for populist democracy; the remaining three seek to state the necessary and sufficient institutions for maintaining an operative polyarchy.²⁸ These latter chapters are of particular interest because they contain, in essence, the propositions that Dahl was later to expand into a full-scale descriptive treatment of American pluralist democracy. Their object, overall, is to establish the overriding importance of elections (as against constitutional rules and the division of governmental authority) as the crucial institution for keeping governmental elites responsive to the preferences of the governed. He is equally bent, however, on demonstrating the logical invalidity of elections as indicators of majority preference, and on showing further that the principle of majority rule in elections promotes democratic stability only when the preferences of a society are distributed in one of a certain limited number of patterns. This thesis in turn leads to a close and careful discussion of the problem of measuring and comparing intensities of preference, and of accommodating the conclusions in a general

²⁴*PDT*, pp. 30–31.

²⁵Ibid., pp. 27-29. On p. 50, in discussing the problem of intensity, Dahl does indirectly acknowledge this argument.

²⁶Ibid., pp. 6, 12. On p. 6, Dahl promptly reformulates the proposal through the insertion of the empirical premises that are clearly implied; nevertheless, on p. 12 he again seriously entertains the possibility that Madison was solving his argument by definition before once again dismissing the point. Madison's quotation is drawn from *The Federalist*, No. 47

²⁷*Ibid.*, pp. 6, 8.

²⁸Strictly speaking, "polyarchy" is at most, as we have seen, only one processual component among many in the actual operation of a concrete governmental system, but Dahl here extends the term to mean any government characterized by the selection of its effective leaders through freely competitive elections. For his formal definition of "polyarchy" in this sense, see PDT, pp. 84-85.

theory of democratic rule.²⁹ Since, Dahl argues, there is ultimately no wholly satisfactory resolution of this problem, he resolutely avoids a utopian tenor in his brief final appraisal of the American polyarchal system: it has not guaranteed all citizens an equal voice in the government, but it has enabled most active and "legitimate" groups to make themselves effectively heard at some crucial stage in the decision process; it has not headed off slavery, civil war, two world wars, or numerous periods of economic instability, but it has encouraged agreement, moderation, and a reasonable degree of social peace in a powerful, diversified, and incredibly complex society.³⁰

By almost any standard, A Preface to Democratic Theory is a more conservative work than Politics, Economics, and Welfare. Its scope is less general; its classifications are less ambitiously simplified and systematized; its careful, precise argument is directed to more familiar themes. At the same time, it constituted a decisive step forward for Dahl in the direction he was actually to pursue over the next ten years. For the first time one sees clearly in this book how he intends to adapt the economists' models of man to political analysis. Given certain distributions of preferences and political resources within a population, he will ask how these patterns will affect probable policy outcomes, and to what extent these outcomes can or will be affected by such factors as information, interest, organization, access, and rules. By attributing rational selfinterest to a high proportion of the key participants, he is able to explain rather than merely to describe outcomes; his correlations are used to test propositions rather than to generate them.

This approach produces costs as well as gains. Social structure often appears as little more than an aggregate of the traits of individual actors. Dahl shows hereafter less interest in how attitudes are acquired and modified through participation, more disposition to discuss politics as a process by which individuals seek to control others in order to realize a fixed set of preferences. Since Dahl has never discussed the general aspects of his metatheoretical approach, one cannot be sure how self-consciously he paid that price. One can only be sure that he paid it.³¹

The Concept of Power

Once Dahl had settled on a general paradigm of inquiry, it was a natural next step for him to attempt to clarify the ambiguities of its central term. His major work for the next several years may be read as a recurrent attempt to unveil the mysteries of power.³² It is not too much to say that, in purely expository terms, no other political scientist succeeded more completely. No one has written with more perfect clarity on this most protean of political concepts, and no one has inventoried more concisely and carefully its varied dimensions. If even Dahl's most finished formulations leave one ultimately dissatisfied, the dissatisfaction arises mainly in reaction to his conviction that what cannot be stated clearly should not be stated at all.

So sophisticated and so tautly drawn have been Dahl's essays on power, and so extensive has been the critical literature they have engendered, that a review of this compass cannot

rights." His first formulation of the issue appeared in PDT, pp. 105-112; later he expanded the argument with additional data in "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," Journal of Public Law, 6 (June, 1958), 279-295. In these articles he showed that, contrary to the nonempirical supposition of most legal scholars, the Court has not in fact effectively protected minorities from "majority tyranny," but instead has followed the election returns — at least in most instances involving an intense and persistent congressional majority.

This finding certainly does call into question many fundamental propositions about the role of the Court within the American polity. At the same time, the leading inference is by no means conclusive. Dahl's approach tends to convert judicial decision making (as he himself quite openly acknowledges) too much into terms of "who gets what?", with minority appeals being treated as if they were merely for discriminate benefits for that limited group alone. Leaving purely legal considerations aside, one gets little indication in such an approach of the Court's role as a teacher, and of the importance not only of the result in the instant case but of the terms in which the result is framed and justified.

32 His major conceptual essays on power from this period include "The Concept of Power" (CP; 1957); "A Critique of the Ruling Elite Model" (CREM; 1958); Modern Political Analysis (MPA) Chaps. 5-6; and the article on "Power" in the International Encyclopedia of the Social Sciences, Vol. 12 (New York: Macmillan Co., 1968), pp. 405-415. Although the I.E.S.S. article must be taken as Dahl's most recent (and therefore presumably most authoritative) statement of his views, it is by its nature rather more a survey of the literature than a detailed exposition of his own formulations. I have therefore drawn most heavily in the following discussion in the chapters in MPA, which, though less technical than the early CP article, are also in some respects much clearer. Dahl's "Critique" raises highly important but somewhat special issues that can best be discussed together with Who Governs?, his major treatise in empirical power analysis.

²⁹Ibid., Chap. 4. ³⁰Ibid., pp. 145-151.

³¹One illustration of both the costs and gains of this approach may be found in Dahl's discussion of the U.S. Supreme Court as a protector of "minority

do justice to even the most central points. At most, one can sketch in broadest outline Dahl's most distinctive contentions. These, in turn, may prove sufficient to make comprehensible a severely limited appraisal at a high level of generality.

At the core of all Dahl's writings on power is the insistence that power is not an attribute but a relation. This contention was not at all original with him. What is distinctive is his uncompromising development of its implications. For Dahl, "power," "influence," and "control" appear as interchangeable terms: all have in common the attribution of causal efficacy to a specified actor or set of actors in relation to other specified sets of actors.³³ In particular, we may say that "A influences B to the extent that he gets B to do something that B would not otherwise do."34 Influence (or "power"), then, is an active concept; it is not a condition, not a structure, but a demonstrably asymmetrical causal connection among actors, the activation of which alone suffices to explain the observed changes of behavior in which we are interested. Like Herbert Simon before him, Dahl therefore argues that when conditions preclude observation of acts of power (or attempts to exercise power) - whether because of complete consonance among the objectives of all relevant actors, or because of perfect anticipation and adjustment by the presumptively less powerful - we cannot distinguish true from falsely attributed power relationships.35 By their acts alone shall we know the powerful.

Insistence on the active, relational nature of power led Dahl directly to several distinctions

³³In CP, p. 203, Dahl explicitly eschews the language of causality out of deference to Hume. But since the rest of his argument makes no sense without the attribution of asymmetry to relations among people, I shall reintroduce it.

34MPA, p. 40. Although in CP Dahl declined to distinguish between "power" and "influence" (p. 202), he found reason to do so by the time he wrote MPA. However, the distinction does not turn, as in ordinary language, on the question of intent: when A is causally responsible for altering B's behavior in ways unknown to — and immaterial to — A, or when B's causally evoked response is different from and/or contrary to A's intent, Dahl prefers to use the unilinear scalar language of "negative" influence (or — presumably — zero influence; Dahl does not really deal adequately with unintended causal relations or those to which the actors involved are indifferent) rather than qualitatively distinguishing such linkages from acts of "power" as acts of "influence."

³⁵CP, pp. 203-204. Cf. the more elaborate discussion of these problems by Herbert Simon, "Notes on the Observation and Measurement of Power," *Journal of Politics* 15 (August, 1953), 500-516.

of crucial importance. In the first place, he argued (as Simon had done before him) that the exercise of "power" must not be confused with its possible bases — wealth, military might, information, or whatever. That the wealthy are powerful is an interesting, testable, empirical proposition that should be regarded as probable but falsifiable in principle, not tautologically true.36 The amount of resources available to an actor may prove, at the least, a significant constraint on the power he is able to exercise. and it may serve as a useful index of his power potential. But no reliable conversion rate between nonpolitical resources and political power has yet been worked out (not all elections can be bought), and the effectiveness even of purely political resources remains a function of the actor's interest, skill, and morale (as the history of presidential campaigns has repeatedly illustrated). Hence an actor's actual power must be sharply distinguished from his estimated potential power at the moment, and both of these from estimates of his probable future power.³⁷ Many of Dahl's most prolonged controversies with his critics turned on the difference between his documentation of the power an actor had actually exercised and their estimates of the potential power such actors possessed. Without slighting the importance of the latter enterprise, I am driven to remark that it is not the former, and that - given the numerous possibilities for slippage Dahl has outlined - it is necessarily a far more speculative undertaking.

Dahl asserts, moreover, that if "power" is a relational concept, it cannot logically be treated as a homogeneous quantum. Any comparative assessment of the power of two actors requires, as a preliminary condition, careful specification of the dimensions along which one proposes to compare their ability to control other actors. Dahl suggests five such dimensions: the number of people controlled, the extent of behavioral modification induced, the resistance (subjective costs of compliance) overcome, the probability of compliance, and (perhaps most importantly) the scope of the acts with respect to which control can be exerted.38 Whether this list exhausts the range of relevant dimensions of comparison is less consequential than Dahl's underlying point: that the statement "A is more powerful than B" is logically vacuous until one has gone on to say "with respect to X." How, Dahl pertinently inquires, can one

³⁶CP, p. 203. Cf. Simon, "Notes," p. 501.

³⁷MPA, pp. 47-49.

³⁸MPA, pp. 41-47.

validly compare a Senate majority leader's success in changing the votes of five southern senators on a major civil rights bill with a president's ability to elicit one hundred thousand letters of support for his latest initiative in nuclear arms control? Unless one can devise a single persuasive formula incorporating the number of people influenced into a comparison of resistance overcome regarding policy issues with very different historical and affective dimensions, one is left in the unsatisfactory position of assigning addivity to the dissimilar properties of apples and oranges. Too many attributions of power, Dahl argues, attempt that task.³⁹

If we accept that power is best thought of as a general control relationship among actors, one that involves a specified actor's ability to change the behavior of other actors, then Dahl's analysis is as lucid a discussion of the phenomenon as we shall find in the literature. The most telling and suggestive criticisms have therefore been those that challenge the very terms of Dahl's formulation. Some critics, for example, have pointed to Dahl's failure to elaborate the implications of the interdependent nature of control relationships.40 Power, they note, can be asymmetrically ascribed to one actor in relation to other actors only because we have made certain assumptions about the context in which the act occurs. To treat an initiative by A as a sufficient condition for a determinate reaction by B is to deflect attention (as John Stuart Mill pointed out more than a century ago) from the many other conditions that must be present before the particular act in question will have the stated result.

Dahl would doubtless concede the general point quickly enough. What he does not do is to focus systematically on these "other" conditions. These range from (at the most proximate level) the immediate determinants of B's reaction to A's efforts to change his (B's) course of action to (at a hierarchically more general level of control) the complex set of cooperative arrangements that provide the social context within which A's initiative eventuates in a

modification of B's behavior.41 Yet a concern with the emergent properties of these interdependent contexts of agreement and collaboration is precisely what distinguishes the subject matter of the political scientist from that of the student of interpersonal relations. 42 To compare President Johnson's and President Nixon's capacity to control an individual airman's decision to release bombs over Cambodia seems less fruitful and more misleadingly formulated than to inquire into the political structure that implicates a president and an enlisted man of whom he is totally unaware in a relationship that eventuates in such an outcome. Viewed in this light, Dahl's treatment of political power as merely a sectorally specialized case of a general relationship between Actors A and B becomes much more than an exercise in logical clarification. It represents a theoretical stance toward politics - and a dubiously simplified theoretical stance - as well.

The alternative theoretical consequences of explicitly considering the emergent properties of power in political systems are most completely illustrated in Talcott Parsons's essay on the subject. 43 Parsons's subtle and inordinately complex argument defies summary; here we can only consider his direct criticisms of Dahl. Parsons locates Dahl's treatment of power in a tradition reaching back to Hobbes insofar as it conceptualizes the phenomenon "as simply the generalized capacity to attain ends or goals in social relations, independently of the media employed or of the status of 'authorization' to make decisions or impose obligations."44 Closely allied with this tradition, though not logically derivative from it, is the tendency to discuss power in zero-sum terms as an interpersonal relationship of dominance and submission. Parsons offers as an alternative an elaborate analytic paradigm in which power is conceptualized, on an analogy with money in

³⁹To state the problem is not, of course, to preclude solutions. One might conceivably link degree of change induced to resistance overcome. Dahl's point is that one must state control relationships in ways that make clear the need for, and logical foundations of, such solutions.

⁴⁰A notably lucid short statement of the following argument may be found in R. Harrison Wagner, "The Concept of Power and the Study of Politics," in *Political Power: A Reader in Theory and Research*, Roderick Bell, D. V. Edwards, and R. H. Wagner, eds. (New York: The Free Press, 1969), pp. 3–12.

⁴¹It is true that Dahl lists the "subjective psychological costs of compliance" as one of the five dimensions along which to compare influence (MPA, 43-44). But the few accompanying paragraphs in no way suggest the perspective outlined above.

⁴² An "actor," of course, may be a corporation or a nation-state as well as a concrete person. What is at issue is not the nature of the concrete entity but the terms in which *relationships* are conceptualized.

⁴³ Talcott Parsons, "On the Concept of Political Power," Proceedings of the American Philosophical Society, 107 (June, 1963), 232–262. This essay has been reprinted in Talcott Parsons, Sociological Theory and Modern Society (New York: The Free Press, 1967), pp. 297–354, and in Bell, Edwards, and Wagner, pp. 251–284. I have used the page numbers of the latter edition.

⁴⁴Parsons, p. 251.

the economic system, as a facilitative medium within a defined network of political relationships. This system-specific medium he defines as the "generalized capacity to secure the performance of binding obligations by units in a system of collective organization when the obligations are legitimized with reference to their bearing on collective goals and where in case of recalcitrance there is a presumption of enforcement by negative situational sanctions."45 A particularly important consequence of this conception is that it permits Parsons to discuss intelligibly, as Dahl cannot do within the terms of his paradigm, the phenomenon of power "inflation" (e.g., the overcommitments of governmental capacity by 1966 in President Johnson's Great Society Program) and of power "deflation" (e.g., the paralyzing distrust induced by McCarthyism) at the macro-systemic level.46 So defined, "power" unquestionably becomes a more elusive phenomenon than in Dahl's straightforward vector diagrams. In this instance, however, it may be argued that Dahl achieved lucidity at too high a price.

Political Power and American Society

After several years of unremittingly conceptual writing, Dahl felt the need to test the utility of his leading theses in empirical research.⁴⁷ The outcome was the two books — Who Governs? (WG; 1961) and Pluralist Democracy in the United States (PDUS; 1967) — which (together with his slender introductory text, Modern Political Analysis, 1963) are probably the titles other political scientists most readily associate with his name.⁴⁸ Who Governs?, indeed, has generated so large a polemical crossfire that less knowledgeable readers might be forgiven for taking it to represent Dahl's primary — perhaps his exclusive — contribution to the discipline. Any

⁴⁵Ibid., p. 257. In this definition, "power" becomes complexly intertwined with "authority" and "obligation." It is a curiously significant fact that one can find no sustained analysis of the latter two concepts in all Dahl's voluminous writings.

46 Ibid., pp. 277-278.

⁴⁷Personal communication, Feb. 2, 1974.

⁴⁸Since I have already discussed the most central chapters of MPA in connection with Dahl's views on power, I shall say nothing further about it in this section. The book is otherwise remarkable chiefly for the highly effective graphs, diagrams, and charts that Dahl uses to illustrate his limpid text; throughout, the book bears the stamp of a great teacher. A quite extensively revised second edition in 1970 adds a great mass of up-to-date cross-national data to the tables, but unhappily fragments and truncates a splendid chapter (Chap. 7) on the factors reducing the probability of a coercive resolution of conflicting aims.

review of the totality of his writings cannot fail to suggest in the most forceful terms how grave a misperception this belief entails.

From the moment of its publication, Who Governs? elicited stronger reactions than any of Dahl's other works. These reactions fell into two reasonably well-defined phases. At first, the reception was overwhelmingly favorable. It is easy to see why. Dahl's chosen approach to his subject committed him (and his research team) to reconstructing in great detail a set of critical decisions in several different issue areas. By adopting this approach, he was able to accumulate evidence bearing directly on the demonstrated ability of the participants in each of these areas to initiate proposals later adopted as actual policy or to successfully block proposals initiated by others. The solid factual base of the research was illuminated, moreover, by explanatory propositions and conceptual distinctions formulated in sophisticatedly universal terms with Dahl's customary linguistic precision. Ingeniously arranged tables and graphs supplemented the verbal arguments, and the study contained several other features not normally found in studies of local politics among them, an historical chapter that illustrated a critical thesis rather than merely serving as inert "background"; disquisitions on such topics of general interest as citizenship without politics and the nature of homo politicus; and an ambitious concluding chapter on stability and change in the American democratic creed. Only later, as a second generation of critics began to contemplate the possible ideological implications of these conclusions and then the methods by which they were reached, did Who Governs? begin to attract censure as a primary exhibit in the apologetic literature on American pluralist democracy.49

That Who Governs? should have had so striking an impact on the discipline of political science is of some significance in itself. As the subtitle ("Democracy and Power in an American City") indicates, the book is a report on research conducted in the single city of New Haven, Connecticut. On its face, the specific findings of the study would therefore seem to have rather limited significance for those not deeply engrossed by the politics of New Haven. But since quite the opposite has proved true,

49 Because a review of this compass must of necessity focus on Dahl rather than on his critics, citations from the latter have had to be kept to a minimum. Several of the better-known essays of that genre have been gathered together in Charles A. McCoy and John Playford, eds., Apolitical Politics: A Critique of Behavioralism (New York: Thomas Y. Crowell Co., 1967).

there is perhaps some excuse for underlining the point. New Haven is not Floyd Hunter's Atlanta, nor is it the Lynds' Muncie, Indiana let alone the United States of America. Criticism based on the supposition that it somehow stands for these other locales as well and that its empirical findings must be reconciled with conclusions drawn from studies of such places would seem misplaced, to say the least. The urgency of such efforts seems predicated on movement from the questionable empirical generalization that governments everywhere are only a front for overtly nonpolitical economic interests to the conclusion that any given study of government must be judged by the extent to which it unmasks such connections. Those not committed to that syllogism need experience no difficulty in entertaining the possibility that, whatever may be true of Community X, New Haven is a pluralist democracy led by multiple coalitions centering on the elected mayor (the finding reported by Dahl and his associates).50

Something like this syllogism must be invoked to account for the intensity of the criticisms to which Dahl's research strategy has been subjected. For, taken on its own terms, the approach is surely more defensible than most. Instead of presupposing a unified power elite, it treats the matter as an open question to be settled by investigation; instead of employing a terminology that begs the question of whether inequalities are cumulative and stable over time in all sectors of society with regard to all issues, it disaggregates the concept of a power structure into elements that become joined empirically, if at all. Rather than collecting data on opinions as to who holds

50Certain passages and chapter titles in Who Governs? contribute to the propensity of critics to read into its findings some general theses on American politics. For reasons not elaborated, Dahl wishes to argue that New Haven is in many respects "typical" of American cities (cf. the Preface and Appendix A). The second half of the book is subtitled "Pluralist Democracy: An Explanation," and several of the succeeding chapters are premised on the contention that New Haven is a pluralist democracy in miniature. The final chapter offers a brilliant general explanation of why American democratic values prevail in political life despite evidence of apathy or hostility toward these values by the general public. Leaving the problems of circularity aside (why are there payoffs to the political elite for affirming rather than attacking these values?), one must ask on what evidence this extrapolation from New Haven is based.

51 For two exceptionally vigorous defenses, see Raymond Wolfinger, "Reputation and Reality in the Study of Community Power," American Sociological Review, 25 (Oct., 1960), 636-644, and Nelson Polsby, Community Power and Political Theory (New Haven: Yale University Press, 1963).

"power," it calls for direct investigation of the phenomenon through reconstruction of issues in which actors actually seek to effect their will against opposition. There is nothing in the logic of this procedure that precludes the possibility of uncovering a pyramidal pattern of power.⁵² It is consequently as misleading to call this research procedure the "pluralist" approach as it is to suggest that those who uncover a pattern of dispersed inequalities approve, as citizens, of what they have found. To call a political scientist a "pluralist" is therefore to traffic in a triple ambiguity.

More serious questions are raised by the unfortunately labeled "nondecisions" tique.⁵³ For this critique is really an affirmation of the underlying logic of Dahl's research strategy, conjoined with a sharpened focus on the question of which issues are to be selected for study. On this point - clearly of paramount importance for a research strategy that proposes to draw general conclusions about the distribution of political power in a community from an examination of the terms on which specific issues are resolved - Dahl is uncharacteristically brief and unsystematic. In his 325-page report he devotes only one short paragraph to explaining and justifying his choice of the three issue areas that are to form the subject of the succeeding three chapters, which in turn form the base for the remainder of the book.54 The criteria for the choice seem reasonable enough (though rather too ad hoc to serve as much of a guide to others wishing to undertake similar research elsewhere); and if one has considerable faith (as I do) in Dahl's grasp of the dynamics of New Haven politics, one may not be inclined to question the conclusions he draws from examining this particular set. Nevertheless, we are dealing at

 52 Particular formulations may run this risk by giving undue weight to peripheral issues.

⁵³The original – and in many ways still the best – statement of this thesis appeared in Peter Bachrach and Morton S. Baratz, "Two Faces of Power," American Political Science Review, 56 (Dec., 1962), 947–952.

⁵⁴The crucial paragraph appears on p. 64. An even briefer resume is offered (p. 333) in Appendix B, in which he outlines the methodology of the study. On this point see Bachrach and Baratz, Part III. But one must assume that Dahl did not see the force of this criticism, for in a surprisingly ill-tempered and uncharitable response to a mildly critical article by Thomas Anton he rebutted 17 points in Anton's article but did not even refer to Anton's questions about how issues are defined. See Thomas J. Anton, "Power, Pluralism, and Local Politics," Administrative Science Quarterly, 7 (March, 1963), 425–457, at p. 454; and an exchange between Dahl and Anton in the same journal, 8 (Sept., 1963), 250–268.

this point with matters of faith and authority, not with the usual demanding standards of the behavioral sciences. In particular, we are obliged to assume, on the basis of fragmentary evidence, that Dahl remained as alert as his critics to the possibility that issues of potentially crucial political importance were kept off the public (governmental) agenda altogether, or reached that agenda only after several crucial decisional premises had already been established. In as apparently open, pluralistic, and politically vigorous a community as New Haven, such a "mobilization of bias" does not in fact seem especially likely. But it is surely warranted to suggest, as David Easton has done, that the "gatekeeping" activities of the political system deserve as much attention as the management of conflict within the political arena, and that a comprehensive research strategy should systematically direct attention to both elements of the process.55

Pluralist Democracy in the United States, in scope at least, would seem a more appropriate target for many of the strictures directed at Who Governs?⁵⁶ In fact, it has attracted much less attention. Possibly the lower ratio of new information about a better-known system, together with greater familiarity with Dahl's approach, accounts for the relative neglect the

55Cf. David Easton, A Systems Analysis of Political Life (New York: John Wiley, 1965), Chap. 6. One serious objection to the "nondecision" thesis is that it simply raises hypothetical possibilities in a manner leading potentially to an infinite regress of irrefutable speculations. Clearly, no researcher can check out every possibility that might occur to a critic; and since, by definition, "nondecisional" activities are of low visibility, there would appear to be no control on this process. Indeed, it might be said that the issue between Dahl and his critics is how far to push back the defining boundary between the "political" and "nonpolitical" stages of a developing issue, since Dahl's method clearly does allow for the suppression of alternatives within the political arena before an authoritative decision is reached.

56In 1971 Dahl prepared a second edition under the title Democracy in the United States: Promise and Performance. The change in title was deliberate. On the one hand, Dahl sought to get away from the ambiguities — and the question-begging controversy — over "pluralism"; on the other, he wished to stress more explicitly the gap between the traditional ideals of democracy and the current record of the American government (which he calls, harking back to PEW, a "polyarchy" in order to take direct account of the unequal distribution of power within the system). The greater concern for evaluation carries over into the addition of several preliminary chapters on "Democracy as an Ideal and as an Achievement" and a final chapter assessing the performance of the American polyarchy; none contain ideas more novel than that great inequalities of wealth and income remain in the country and that racial discrimination violates American ideals.

book has suffered. The textbook format has also something to do with the matter.

It is, nevertheless, no ordinary textbook. Dahl has one clear focus - the determinants of conflict and consent in American politics around which the entire book is organized. Once again, one confronts Dahl's ability to abstract the essentials of a question and jettison the detritus. The first two pages immediately involve the reader in the implications of a national history marked both by the triumph of the Philadelphia constitutional convention and the tragic and massively bloody civil war three quarters of a century later. This dramatic juxtaposition leads directly to several sharply pointed questions about the dilemmas of modern large-scale democratic government. It is the object of the remainder of the book to answer these questions.

Certainly Pluralist Democracy is not an unmitigated apology for American government. Dahl treats as contingent what most textbooks take for granted: the stability and efficacy of American political institutions. He highlights the failures - and the accompanying violence as directly as the achievements. It is the explanations themselves - and the paradigm for the explanations - that are somewhat too unexhilaratingly conventional. His categories and conclusions will be familiar to all readers of American political science over the past quarter century - the stress on diversity, on crosscutting cleavages as a source of stability, on the evidence that American voters are startlingly apathetic, ill informed, and unstructured in their views by the standards of traditional democratic expectations. The book has more to say than most about strategies open to those who seek to achieve their goals within - or against - the system; it is in this sense more dynamic, less purely descriptive, than the traditions of "status quo" behavioralism would lead its critics to anticipate. But a 471-page treatise on American government in which, for example, the only indexed reference to the military is to the Military Reconstruction Act of 1867 is not likely to persuade the previously unpersuaded that Dahl has addressed himself squarely to the central issues in American political life.57

57Another theme of some interest is Dahl's ambivalence toward reduced political participation. Like all attentive readers of empirical survey research reports on civic orientations, he is struck by the fact that most "extremists" are nonvoters and that a significant fraction of nonvoters hold "extreme" orientations. This perspective emerges most clearly in a controversy with Jack L. Walker over "The Elitist Theory of Democracy," American Political Science

Political Oppositions in Comparative Perspective

By the mid-1960s, Dahl's position within the American political science fraternity was well defined. On virtually any subject relating to American politics - from school board elections to foreign policy in an era of nuclear weapons, from the technical requisites for national economic planning to the empirical delineation of local power elites - he could be counted on to write with singular acumen, clarity, and cogency. Yet a paradox remained at the heart of his work. The most universalistic of political scientists in his abstract formulations. he had restricted the territorial domain of his substantive references to his native land - a land regarded by all foreign observers since its foundation as radically deviant in its politics from the patterns of the Old World. 58 As Aristotle had once taken the Hellenic polis to be the natural unit from which to derive generalizations about political man, so now Dahl appeared - in a decade of rapidly cumulating monographs on chaotic foreign systems to be using the United States of America as his universe. The risks of misleading overgeneralization seemed similar.

The publication of Political Oppositions in Western Democracies (P.O.W.D.; 1966) accordingly marked much more than the appearance of yet another hundred pages by Dahl on the subject of contemporary democracy. For in three concluding essays to a collection of papers on the politics of opposition in ten North Atlantic countries, Dahl took on the challenge of relating the appearance, survival rate, and differing patterns of Anglo-European oppositional groups to the varied socioeconomic national contexts in which they have operated. It was an ambitious task. That his answers proved less conclusive here than his previous analyses of the American polyarchy

Review, 60 (June, 1966), 285-305. It is a frustrating controversy: Walker appears to brush aside the evidence referred to above in his highly normative structures, while Dahl is more convincing in his general assertion that he favors an increase in some kinds of participation than in a long footnote (No. 13) designed to show that Walker's characterizations have no foundation.

spoke more for the inordinate complexity of the undertaking than for any flagging of his insight and application. Granted that in their brevity the three essays lacked the rich texture of history customarily found in essays on European politics, it could still be said that Dahl's comparative inquiries into the social foundations of democracy now brought him within the great tradition of Tocqueville, Bryce, and (more recently) Seymour Martin Lipset.

These were, at any rate, the figures with whom Dahl was compared, and not unfavorably, in an efflux of long, thoughtful review articles that for once were almost uniform in their praise. Critics differed largely over the question of parsimony. Harry Eckstein, for example, complained in an otherwise favorable review that Dahl had not been sufficiently audacious in reducing the number of his theoretical variables. Conversely, Sidney Verba, who applauded the "disciplined configurative approach" of the volume, remarked with oracular wryness that in Dahl's chapters, "the typology is too simple to capture the richness of real patterns of oppositions; on the other hand, it is complex enough to impede the development of any simple and parsimonious explanation of oppositions – which probably means that it is just about right."59

And indeed, both charges would appear to be merited. Dahl does not clearly establish the theoretical utility of listing "distinctiveness" as a major dimension of oppositional patterns alongside "concentration," "competitiveness," and "site of encounter" - the three variables of which it is said at the same time to be a function. Conversely, categorical explanatory variables like "the record of governmental response to grievances" are obviously so summary as to require immediate decomposition into typologies that must themselves be explained by a complex of singular historical statements such as commonly occur in conventional political histories of the region. There are also intricacies to the levels of explanation that, if systematically elaborated, would so complicate the model of explanation as to render it rather obviously unmanageable. One has only to consider the manifold interlocking relationships among cultural premises, the record of historical grievances, and the governmental and socioeconomic determinants of that record to see the magnitude of the problem. Dahl himself points to some of these difficulties in certain

⁵⁸The one conspicuous exception to this generalization is the cross-nationally comparative structure of *Politics, Economics and Welfare.* But the multitudinous references to planning experiences in Great Britain, Sweden, the Netherlands, New Zealand, et al., seem chosen (save in the case of Great Britain) at a quite superficial level for their illustrative effect: the book neither stands not falls on their validity. One suspects that Lindblom's independent contributions as an economist were particularly important here.

⁵⁹Eckstein, American Political Science Review, 61 (March, 1967), pp. 158–160; Verba, "Some Dilemmas in Comparative Research," World Politics, 20 (Oct. 1967), 111–127, at p. 118.

passages, and they are worth underscoring mainly because the substance of his analysis — cumulative and crosscutting cleavages, problemsolving vs. ideological orientations, the significance of late and grudging admission of the urban working class to the political arena — has been the staple for some years of monographs on European politics. It is for their simplification and arrangement of ideas, rather than for any novelty of content, that these chapters have won such favorable notice; and it is as such that they must be judged.

Though less discussed by his reviewers, Dahl's evaluative observations in this volume regarding the "democratic Leviathans" of the contemporary North Atlantic region may now strike many readers, at a remove of a decade, as being worthy of as much attention as his explanatory paradigms. For Dahl - hitherto best known as the sympathetic exponent of an Anglo-American, incrementalist, problem-solving political culture - was nevertheless moved to draw attention from a comparative perspective to the limitations of the two-party polyarchy as a model of rationally oppositional politics. A dichotomous organization of political conflict, he noted, might promote the favorable outcomes conventionally attributed to a disciplined two-party system - among others, the dampening of extremism, the generation of popular support for effective governmental action, and the preparation of alternative teams of leaders for the responsible exercise of power. But whether it does in fact do so depends heavily, he argued, upon the distribution of preferences within the electorate. Where these could be described through congruent bell-shaped curves for every crucial dimension of political choice, the outcomes would indeed resemble the controlled purposiveness of modern British politics. Bimodal or multimodal distribution of constitutional or policy preferences, on the other hand, created conditions in which electoral competition between two disciplined parties might well accelerate political polarization. In such countries, two very loosely structured parties or a multitude of splinter parties would therefore, while reflecting a dissensual society, also possibly serve as the necessary vehicles for civic governance.60 Dahl predictably concludes that intense ideological conflict among purely representational parties generates irrationalities of its own. But

60POWD, pp. 375-376, 392, 395. The general theoretical argument was, of course, already advanced in its essentials in his PDT, Chap. 4, and in Anthony Downs, An Economic Theory of Democracy (New York, Harper & Row, 1957), pp. 117 ff.

the serious attention he offers these alternatives, and his detached appraisal of many venerated Anglo-American institutions, convey in their totality an openness to comparative experience not evident in his previous work.⁶¹

The concluding three chapters of the volume on Political Oppositions can now be seen to have prepared the way for what is undoubtedly Dahl's most ambitious publication of the past decade. Polyarchy (1971), despite its slender size, is breathtaking both in its scope and in the foundational character of its analysis. By any standard, it is a major contribution to the systematic study of democracy. Not since the days of Lord Bryce had any scholar dared to undertake a full-scale empirical exploration of the principal social, economic, cultural, and political conditions necessary for a democratic order, and in Bryce's time it was still possible to confine one's attention to Great Britain and its dominions, Western Europe, and a select group of nations in the Western Hemisphere. 62 Yet Dahl's short chapters deal so directly with the leading explanations for the emergence and survival of democratic politics that one quickly comes to take for granted an enterprise that, in the range of countries considered and in the explicitness of the propositions regarding them, has in fact no real parallels in the literature of comparative politics. Much as Dr. Watson reacted to Sherlock Holmes's deductions, one marvels after the fact that no contemporary political scientist had thought to take up so obvious a project before Dahl did so himself.

For *Polyarchy* is, in its broad structural outlines, a treatise of striking simplicity. In its first sentence it poses a problem of immediately compelling interest: "Given a regime in which the opponents of the government cannot openly and legally organize into political parties in order to oppose the government in free and fair elections, what conditions favor or impede a transformation into a regime in which they can?" The remainder of the book is strictly

⁶¹Even more striking is Dahl's anticipation, at the high-water mark of President Lyndon Johnson's Great Society, of disenchantment among "young people" and intellectuals with the "remote and bureaucratized" state of the "democratic Leviathan" (POWD, pp. 399-400). In these final pages of his Epilogue to the volume, Dahl faintly foreshadows the concerns that were soon to lead to his systematic exploration of the ultra-democratic critique of polyarchy in After the Revolution?

⁶²A few recent studies, on which Dahl himself draws heavily, offer statistical correlates of various categories of political regimes, including democracy, but do not purport to relate these findings with any degree of complexity to the distinctive institutions and processes of the democratic order.

limited to answering that question. In so doing, Dahl feels compelled to note that he is not concerned with identifying the social foundations of an ideal construct termed "democracy" (in his terms, a political system in which the government is completely responsive to the preferences of its citizens, all being political equals). His purpose is rather to explore the environments of a global set of currently operative, large-scale national governments distinguished by (1) their guaranteeing of eight necessary conditions for open political competition and choice and (2) their extending of these guarantees to a very large and institutionally defined proportion of their total adult population.63 It is these inclusive, internally competitive political systems, which Dahl calls "polyarchies," that form the dependent variable of the propositions he wishes to test.64

In performing his tests, Dahl takes into account the evidence provided by a carefully constructed list of twenty-nine polyarchies and six "near-polyarchies" throughoùt the world that seemed to be meeting his criteria during the late 'sixties. It goes almost without saying that his detailed knowledge of many countries on the list falls short of what more specialized scholars in the politics of each might wish. The objection, while obvious, is also somewhat beside the point. The gains as well as the costs of performing global comparative analysis deserve some emphasis. In Polyarchy, plausible generalizations evolved in Norway are tested in Japan and Israel and disconfirmed in Uruguay. Some countries appear especially well suited to examining the interplay of economic and political processes; others provide apt illustrations of

⁶³Polyarchy, pp. 1–5.

64To me, at least, an unsatisfactory element in Dahl's delineation of his dependent variable is his insistence on employing two dimensions, one of which is patently more important than the other. "Competitive oligarchies" (e.g., Britain, 1832–1884) shade indistinguishably along a quantitative continuum into full polyarchies, whereas only Communists and Fascists would seriously contend that "inclusive hegemonies" resemble polyarchies in any critically significant sense. The problem in attributing a formally equal weight to public contestation and inclusiveness can be seen at its logically absurd extreme in Dahl's hesitancy at including Switzerland among the world's 25 polyarchies because it did not at that time grant suffrage in national elections to the female 50 per cent of its adult population (Appendix B, p. 246). Thus his diagram (p. 6) of "two theoretical dimensions of democratization" is characteristically ingenious, but utterly misleading in its formal implication that countries far along the 15 line (high in inclusiveness, low in public contestation) can properly be regarded as no farther from the 45 line than those far along the 75° line (low in inclusiveness, but high in public contestation).

the impact of social structure on party organization. Still others offer the occasion for reflections on the possibility of externally imposing an effectively polyarchal political order on a conquered nation.

So great is the scope of the book, and so brief the compass, that some sacrifice of complexity predictably occurs. The sacrifice is far less than one might anticipate. Dahl's concise discussion of the failure of democracy in Argentina should suffice to dispel the impression that Polyarchy contains no more than superficial correlations of questionably aggregated variables.65 It is hard to think of a more pointed short discussion of the complex relationship between socioeconomic inequality and political democracy than the one Dahl offers here.66 Some topics do suffer more severely than others from Dahl's extreme compression - particularly the chapter on the alternative historical paths of regime transformation and the discussion of the relation of levels of economic development to the prospects for political competition.67 On the whole, however, Polyarchy must rank as one of the most impressive instances in the entire political science literature of saying much in little space.

It is inevitable that, in discussing the sociopolitical matrix of competitive politics in some
thirty nations on six continents, Dahl should
posit several disputable empirical connections.
Each regional specialist will have his or her own
list. It is perhaps more profitable to discuss at
this point the approach Dahl employs. In
essence, his strategy may be characterized as a
succession of sophisticated applications of John
Stuart Mill's Methods of Difference and of
Agreement. Noting that polyarchal politics prevails in some countries but not in others, he
seeks to account for its presence or absence by
referring to the relative position of the coun-

⁶⁵Polyarchy, pp. 132-140.

⁶⁶ Ibid., Chap. 6.

⁶⁷Thus, in his discussion of economic development, Dahl leaves himself no room to examine the independently important factor of rate of growth. Moreover, his insistence (Polyarchy, pp. 76–80) that the educational and structural requisites of an "advanced economy" generate demands for a competitive political order seems to grow mainly out of the exigencies of his general argument. It scarcely constitutes a refutation of writers who, taking into account the enhanced opportunities for deliberately controlling socialization, for monitoring increasingly sophisticated informational networks, for pyramiding economic and coercive resources, and for buying off potential dissidents, have argued otherwise in the case of the now relatively affluent Soviet and East European Communist nations.

tries under consideration on a succession of scales that middle-range theories of politics plausibly indicate to have some connection to these outcomes.

These methods have served comparative analysts very well. They may indeed be said to comprise — as Mill was wont to argue — the core of the comparative method. They invite explicit identification of the key causal and dependent variables, and explicit specification of the relationship among them. The approach tends to suffer, on the other hand, (as Mill himself quite clearly saw), from the uncertainties introduced when other theoretically relevant factors are neither totally different in all save one respect, nor totally similar in all save one. More disturbingly, it encourages examination of the variables in analytic isolation rather than as elements of organic systems.

Dahl's treatise suffers significantly from these limitations. He seldom makes use of a cumulatively refined structure of partial correlations; rather, each causal variable is separately examined in relation to his countries as part of an aggregating causal chain. Thus, in one chapter he will consider the relationship of agrarian society to polyarchy in New Zealand, Costa Rica, and India; in another, the significance of per capita income level in these countries; in a third, the degree of ethnic pluralism. But how the possibilities for competitive politics are affected in any one country by one of these variables, given the potentially synergistic effects arising from the specific levels attained by the other two in the presence of still other theoretically relevant variables, this approach does not permit us to judge.69 The theoretical yield from his study is therefore not so much a theory (despite the title of Dahl's final chapter) as a tabulation of loosely related propositions about conditions favoring or opposing polyarchy. It is perhaps for this reason that Dahl's analysis is at its least persuasive in accounting for such anomalies as polyarchy's lack of resilience in Uruguay, its surprising durability in pre-modern Switzerland and contemporary India, and its prospective

68See J. S. Mill, A System of Logic (London: Longmans, Green & Co., 1957), pp. 575-577.

69 Dahl is, of course, aware of this problem. He frequently diagrams the interactive potential of a complex set of variables — see e.g., the very well worked out models on pp. 56, 79, and 91. The difficulty is that to quantify them for each country, given the state of other theoretically relevant variables for that country and with due attention to threshold effects and emergent properties, would render the approach unmanageably difficult.

emergence with significant modification in Mexico, Kenya, and Tanzania. More contextual forms of analysis clearly have their drawbacks as well; it may well be that *all* approaches on a global scale must remain merely searchlights in the dark.⁷⁰

Having discussed the general conditions favoring polyarchal government, Dahl evidently felt the logical obligation to extend his inquiry beyond those nations that formally and effectively institutionalize toleration for oppositional politics to those that could serve as a control for his propositions. This reasoning led him to examine those nations in which oppositions have held a precarious status as, at best, politically marginal hold-outs from the dominant party's quest for consensual governance, or - more commonly - as deplored and presumptively treasonous outgrowths the dominant political elites have lacked the resources to quash. Regimes and Oppositions (RO; 1973) records what one must hope to be merely the first fruit of this extension. As in the earlier volume on the West European democracies, the bulk of the book consists of monographs by area specialists on countries ranging, in this instance, from Japan and India through tropical Africa and Latin America to the hegemonies of Franco's Spain and the Soviet-East European Communist bloc. Dahl as editor contributes an introductory chapter in which he offers several universalistic propositions and categories that are intended to help account for the central findings concerning political opposition in the remaining chapters.

Enterprises of this nature contain many intrinsic hazards. Despite the global span of the volume — a scope that rather obviously taxes both the expertise and the powers of condensation of the authors of the short chapters on Africa and Latin America — the reader is left wondering how the plausibility of Dahl's propositions might have been qualified by studies of

70One approach that trades some of the universalism and ready testability of Dahl's propositions for a richer interactive analysis is illustrated by such works as Barrington Moore's Social Origins of Dictatorship and Democracy (Boston: Beacon Press, 1966) and Samuel P. Huntington's Political Order in Changing Societies (New Haven: Yale University Press, 1968), in both of which a set of not very rigorously formulated general models is evolved out of a comparative study of the sources of fragility and strength of democracy (and other forms of government) in particular countries. A notable feature of Huntington's approach is that it points to the creative potential of political leaders and political movements as historical agents combining the specific resources of nations to build political orders — a potentiality that Dahl's cross-sectional categorical analysis obscures despite his chapter on the political beliefs of political activists.

Communist China, Southeast Asia, and the various Muslim states of the Middle East and North Africa. Moreover, because the oppositional movements under consideration comprise an unsystematically defined subset of a purely residual category, the structural and processual analogies between the politics of opposition in pre-1974 India and in post-Dubček Czechoslovakia (let alone between contemporary Japan and the Soviet Union under Stalin) are scarcely more striking than the differences. Dahl copes with this diversity through abstract generalizations notably less intimately in contact with the complex patterns delineated by his contributors than in his earlier, more ambitious effort to develop typologies to fit the historical experiences of the North Atlantic polyarchies. Although he brings into relatively sharp focus some dilemmas of governmental responses to manifestations of opposition in an illiberal regime, his leading conceptual arguments were already quite explicitly set forth in Polyarchy. There is, indeed, an uncharacteristic analytic weariness in Dahl's excessively brief introduction, a sense that he has casually taken on too much and made too little of it. Nowhere, perhaps, does this trait become more evident than in his invocation of the familiar metaphor of reinforcing and cross-cutting societal cleavages to explain the degrees of polarization in socially heterogeneous polities without any apparent concern for the corollary implication that cross-cutting cleavages at once explain the checks to polarization in pluralist America and the often catastrophic fragmentation of partisan constituencies in France.⁷¹ It is surely premature to conclude that Dahl cannot discuss the emergent dynamics of intraparty politics and hegemonic violence in India, Tanzania, Brazil, and Yugoslavia with the same sensitive acumen he displayed in his earlier work on Anglo-American democracy. One can only record that this volume does not disconfirm that suspicion.

Size and Democracy

In his early work, as we have seen, Dahl focused on the practical obstacles to rational civic participation in democratic politics in an

71RO, p. 5. Of course, the differential impact of cross-cutting cleavages in these nations can be dealt with through introducing such flexible contextual concepts as the catalytic significance of varying political cultures; but Dahl does not enter into such complexities. Instead, at the bottom of this same page, he alternately deploys cross-cutting and reinforcing cleavages as a means of suggesting why political cohesiveness is reduced to a minimum in African politics.

era of nuclear technology and far-flung international involvement. This essentially normative orientation (whatever its instrumentalist overlay) was then superseded by a prolonged engagement with conceptual clarification, with the construction of analytic paradigms, with close empirical field investigation, and with theoretical syntheses of others' research. It is, of course, those conceptual and empirical concerns that have become the hallmark of his writings, and it is probably through them that his lasting contributions to the field will prove to have been made. Yet Dahl's work now appears to be coming full circle; for even while steadily expanding his purview of comparative politics, he has simultaneously reverted, though in a more explicitly systematic form, to his earlier philosophical concerns.

"The City in the Future of Democracy" ("CFD"; 1967), Dahl's presidential address to the 1967 American Political Science Association convention in Chicago, heralded his return to the question of how best to improve the quality of democratic participation. A summer of unparalleled racial conflict in several metropolitan areas gave poignant urgency to his topic. His more general theme was, however, the spectacle of an apathetic mass public's loss of control over developments that had left America's major cities

mean, ugly, gross, banal, inconvenient, hazardous, formless, incoherent, unfit for human living, deserts from which a family flees to the greener hinterlands as soon as job and income permit, yet deserts growing so rapidly outward that the open green space to which the family escapes soon shrinks to an oasis and then it too turns to a desert.72

"City-building," he observed, "is one of the most obvious incapacities of Americans." 73

The problem so grimly symbolized by America's disintegrating megalopolises was nevertheless, Dahl reminded his audience, the embodiment of a very ancient philosophical dilemma. Pericles, Plato, and Aristotle had insisted that civic virtue thrived only in political units sufficiently small to permit full participation in the total range of governmental activities by the entire body of legally qualified adult male citizens. Two milennia later, Machiavelli and Rousseau still voiced the same argument. But since the advent of representative government, the center of gravity of philosophical discourse had shifted toward stressing the external costs that small, uncoordinated political units imposed

^{72&}quot;CFD," p. 964.

⁷³Loc. cit.

on one another. Political writers had furthermore made much of the power of provincial prejudice in small units and the ease with which such units were dominated by their largest single actors. The pivotal figure in this shift of perspective was clearly Madison, whose cogent advocacy of large units in Federalist No. 10 lay midway between "Montesquieu and Rousseau, who still see the city-state as the only proper and indeed viable unit for a democratic republic ... [and] John Stuart Mill, who in Representative Government dismisses as irrelevant in a single sentence at the end of a chapter, almost as an afterthought, the two-thousand-year-old tradition."74 But changes in perspective do not necessarily insure progress; and it was Dahl's object to show that in accurately highlighting the limits of the city-state as an autonomous political unit, contemporary writers had too often failed to assess systematically the costs of its expansion.

His own leading thesis begins with a crucial distinction. Political participation, Dahl suggests, may be analyzed into two categories acts involving exchanges of opinion about what should be done, and processes designed to register and implement these preferences. The distinction is crucial because actions directed toward the latter ends (such as voting) can be carried out more or less simultaneously by an indefinitely large number of actors even within the giant nation state, whereas the necessarily sequential nature of deliberation sets severe upper limits to the size of an authentically democratic political unit. "Time's relentless arrow," he notes, "flies directly to the Achilles' heel of all schemes for participatory democracy on a grand scale."75 Thus the acknowledged drawbacks to small-scale politics, together with the invention of representative government, have impelled us toward the creation of ever larger electoral units, but they have meanwhile blunted our sensibilities to what is lost when democratic participation is limited for most citizens to such passive threshold acts as voting. The predictable result is the spreading disorder to which the citizenry of our metropolises must now bear impotent witness.

Can the dilemma be resolved? Dahl rejects at a minimum two currently favored alternatives — the efforts to recreate the simplicity of village life (as in the flight from the central city to the suburbs), and the hopes for transferring the active forums of democracy to the workplace. The first he sees as an irrelevant exercise

in nostalgia, abetted by blindness to "the oppressive weight of repressed deviation and dissent [in small communities] which, when they appear, erupt explosively and leave a lasting burden of antagonism and hatred." The second, on the other hand, turns on a misplaced effort to make the workplace co-extensive with life itself without recognizing the distinctive exigencies of corporate action that demand some form of external regulation.⁷⁶ Dahl suggests in passing that the medium-sized city in contemporary America - a city of between 50,000 and 200,000 inhabitants - may very well optimize most of the significant institutional economies of scale while providing "a political unit of ... truly human proportions in which a citizen can acquire confidence and mastery in the art of politics."77 But his most important and most defensible thesis is that empirically minded political scientists have so far provided us with little solid evidence on which to base any assertions whatever about the connection between societal scale and political democracy.

It is quite characteristic of Dahl that he should himself already have taken the logical next step. Size and Democracy (SD; 1973), co-authored with Edward R. Tufte, is a ground-breaking effort to operationalize the various politically relevant dimensions of jurisdictional "size" so as to make possible their correlation with the presence and quality of "democratic" political life. 78 The investigation of such correlations was clearly overdue. In a provocative introductory chapter the authors manage to gather together twenty-seven plausible but radically conflicting hypotheses — some very ancient — that they have culled from the

⁷⁶Ibid., pp. 961-962. Cf. his earlier exposition of the problems of co-determination in British industry (note 7 above).

77 Ibid., p. 967. A list of cities in contemporary America providing such opportunities would make interesting reading: It would include such centers of civilization as Fresno (California), Amarillo (Texas), Peoria (Illinois), Gary (Indiana), Scranton (Pennsylvania), and (of course) New Haven (Connecticut), while clearly excluding such metropolises as Rochester (New York), Birmingham (Alabama), Tulsa (Oklahoma), and Wichita (Kansas) on grounds of size.

78Though published in 1973 — three years after the "Yale Fastback" (After the Revolution?) discussed below — the first draft of SD was written in 1967 during a stay at the Center for Advanced Study in the Behavioral Sciences in Palo Alto. It seems accordingly appropriate to discuss SD in direct conjunction with "CFD," which draws heavily on ideas developed during the course of research for the larger volume. Since no effort is made to distinguish the contributions of the two co-authors, I have here attributed to Dahl full responsibility for all statements appearing in the book.

⁷⁴"CFD," p. 955. ⁷⁵*Ibid.*, p. 962.

history of political thought on the relationship between their two key variables. Their inference states itself: after some two thousand years of controversy the time has come to reduce this list through systematically using the available evidence to confront paired statements that cannot both be true.

The two authors embark on this enterprise with great care. The guiding theoretical considerations receive axiomatic formulation. Propositions from one chapter are used to qualify and extend the hypotheses advanced in the next; indicators for elusively abstract variables are chosen imaginatively but persuasively. The scope is as global as in Polyarchy sweeping, even, for in addition to the full subset of twenty-nine "polyarchies" the authors also make comparisons among subnational levels of jurisdiction within single nation-states. The authors profitably borrow from analogous discussions by economists of market size – a practice that in Dahl's case goes back to Politics, Economics, and Welfare but that he nowhere employs to better advantage. They argue rigorously on theoretical grounds that no one size of jurisdiction can be held to be optimal for democracy, and that at every stage of the argument, a tradeoff must be anticipated between the opportunities for effective civic participation in politics and the capacity of the system to respond to the full range of the collective preferences of its citizens. Even so, they do not lamely conclude that there are both costs and gains to any given size of polity; they inventory those costs, construct plausible cost-benefit ratios containing relative magnitudes, and in some instances disconfirm widely accepted suppositions regarding the relation of size and democracy.

A review of this scope necessarily precludes detailed discussion of the resultant conclusions - largely based, it should be noted, on secondary analysis of the disparate findings of other scholars. Some are at least nonobvious (such as the finding that threshold effects for the quality of participation are so low that citizens in the very smallest nation-state democracies feel no more efficacious than those in the largest). 79 Some are paradoxical but potentially misleading (the evidence that the military costs of survival are, if anything, lower in the small democracies than in the large); some demand more discussion than the authors offer (e.g., the fragmentation and specialization of civic participation arising from indefinite proliferation of governmental jurisdictions designed to be functionally optimal for limited

⁷⁹SD, pp. 61–65.

purposes). Dahl and Tufte are surely justified, however, in concluding that "one task of democratic theory may be to specify not an optimal unit but an optimal number of units . . . their characteristics, similarities, and differences, the nature of a good political life in each type of unit, and the proper relationships among them." 80

Unhappily, Size and Democracy falls far short of this goal. Little more can be positively affirmed regarding the connection between the two than before the book appeared. The reasons have more to do with the nature of the subject matter than with the limitations of the authors. Among the more prominent difficulties are the apparent unavailability at the time of directly pertinent data beyond a single study of local government in Sweden; the recurrent indications that cross-national comparisons yield less reliable conclusions than comparisons of subnational governments within one country; the useful but essentially negative finding that size alone is seldom as important as institutional, historical, and socioeconomic factors in explaining differences among democratic regimes; and the inherent problems - already referred to in the discussion of Polyarchy - of any single-variable causal analysis.⁸¹ It remains, of course, the singular merit of Dahl and his coauthor to have carried the inquiry as far as they did on the basis of such limited and qualitatively uneven evidence. They carried through their task, moreover, with a clarity of formulation that reveals the precise obstacles to a more extended pursuit of the topic. It is not, therefore, too much to say that their ultimate failure is in many ways more instructive to contemplate than other more conclusive comparative studies.

Participation and Authority

Meanwhile, the field as a whole was facing a challenge of a more inflammable nature. By the

⁸⁰Ibid., 141, 142. A further virtue of the book is that it contains, in a discussion of the relation of size to system capacity, the most enviable sentence in the spectrum of Dahl's vast writings: "A small-democracy man may put his bets on the mouse outlasting the elephant and the whale; doubtless he will find takers among the Chinese, Russians, and Americans" (p. 111).

81On the limited conclusions that can be drawn from cross-national comparisons regarding size see, e.g., SD, pp. 52-53. Accepting this limitation, several students of Heinz Eulau at Stanford — among them Gordon Black — have taken up systematic empirical investigation of the consequences of size for local government inside the United States. Cf. also Sidney Verba and Norman Nie (both formerly of Stanford), Participation and Democracy (New York: Harper and

late 'sixties, the manifest disintegration of a politics of intervention abroad and pluralist conciliation at home had alarmed most thoughtful Americans. Within the discipline, these developments were beginning to evoke correspondingly strident demands for a political science of relevance and engagement. The target of these demands was, of course, the tradition of abstract and (relatively) detached political inquiry that Dahl, through his contributions, had come in considerable measure to personify.

After the Revolution? (AR?; 1970) should be read with this context in mind. It is, of all Dahl's essays, the one most directly responsive to a specifically identifiable mood during a particular historical crisis. More, perhaps, than any other of his works, its agenda is set — on first reading, at least — by the issues of the hour. Its eye-catching cover, with its banners and headbands, is aimed squarely at an undergraduate clientele. From the question-begging title to the avuncular conclusion it serves as an emblem of the period it addresses.

Yet Dahl himself does not merely float with the stream. Despite the first-person mode of address, which often sounds alternately patronizing and irritable in cold print, After the Revolution? is an intensely serious short treatise for readers whom Dahl takes seriously. Its topical prolegomena lead quickly to more enduring matters. "Revolutions," Dahl notes with commendable acidity, now occur at the level of women's hemlines, but the authentically revolutionary potential of the democratic ideal can never be wisely discounted. After the latest "revolution" is over, the problem that troubled the ancient Greeks will remain with us - how to reconcile the exigencies of public governance with the belief that only those governments are legitimate that permit direct and continuous participation by the citizen in the exercise of their authority.

This issue has a familiar ring; and indeed, attentive readers of "The City in the Future of Democracy" and Size and Democracy will find little new in two of After the Revolution?'s three sections. In neither of the former works did Dahl elaborate quite so explicitly and so thoughtfully as in these chapters his critique of those who uncompromisingly advocate either small or large-scale governmental units. Both of the former works are, however, rather clearly predicated on the same set of theses that Dahl here summarizes as "the dilemma of primary democracy"; and, as already noted, both reach

the conclusion - as Dahl does in After the Revolution? - that no one level of jurisdiction can maximize all the values of importance to those strict democrats who also acknowledge the inability of small-scale governments to cope with the external costs their actions engender. Insofar as there is novelty in these chapters, it lies in Dahl's application of his "Chinese Boxes" theorem to three problems of the day - the unequal distribution of socioeconomic resources in most political democracies; the quest for worker control of the "corporate leviathan"; and the somewhat analogous quest of the citizen for control of the "democratic leviathan." Yet each of the latter two topics is disposed of predictably, while the ten-page discussion of inequality soon becomes almost startlingly platitudinous. That much here is well said does not obviate the fact that it has also been said - very often - before.

The lasting claim on most readers' attention comes almost surely in the opening section. Dahl's treatise is subtitled "Authority in the Good Society," and it is here that he seeks to explicate the roots of our habitual compliance with the commands of government. He distinguishes three criteria - the criteria of personal choice, of competence, and of economy that must be satisfied if the attempted exercise of any such authority is to be judged binding by those who must comply. His leading thesis is that the frequently conflicting implications of the three will necessitate some degree of institutional tradeoff among them to attain an optimal solution. Dahl is himself clear that any doctrine of authority that ignores or denies this proposition will prove unpersuasive to those who have understood it. His object, then, is to establish the independently binding importance of these three criteria, and to show (in the remaining two sections of the book) how acceptance of them precludes belief in the sufficiency of any single level of government.

Two of the three criteria are immediately recognizable to any student of classical political philosophy. Like Plato, Dahl insists that rational citizens will often delegate to a smaller group their authority to make collectively binding decisions in the expectation of thereby attaining a superior degree of competence in decision making from those who decide on their behalf. But like Aristotle, he suggests that those who are bound by others' decisions are generally (though by no means invariably) best qualified, as a group, to assess periodically the competence with which these decisions have been made, and like Hobbes, he assumes that all such acts of delegated authority will remain rationally persuasive to the individual citizen

Row, 1972), pp. 231-243, although it treats Dahl's theme only briefly.

only so long as they are seen as clearly more congruent with individual self-interest than the outcome of the unrestricted exercise of personal choice by all. The criteria of personal choice and decisional competence are therefore not necessarily in conflict, but they are always potentially so. Their reconciliation depends crucially on acceptance of the third criterion, the Criterion of Economy. It is a criterion which, Dahl observes, "seems never to have found a prominent place in classical political theory and lacks the allure of the other two criteria."82 Yet it is, as he says, "a criterion that a reasonable man will apply to any system of authority," for it specifies that reasonable citizens will always weigh the opportunity costs of political participation - and of a system of authority heavily dependent on it - in terms of the alternative uses to which they could put the same time, personal energy, and resources. It is for this reason that, as Dahl had remarked previously, "time's arrow" so fatally indicates the limits to visions of unrestricted participatory democracy.83

Dahl's argument in the remaining chapters of After the Revolution? flows directly from these premises. A few arithmetical calculations suffice to show - surely for all time - that only in very small political units (those with memberships of only a few hundred at most) can even the most active speakers hope to engage in sequential, cumulative discussion of a severely limited agenda.84 Yet the leading principle of participatory democracy - one that Dahl calls The Principle of Affected Interests - holds that everyone who is affected by the decisions of a government should have the right to participate in that government. Assuming that "participate" means something more than the right to indicate a readily countable reaction to a slate of proposals formulated in other forums, the dilemma is at once evident. The participatory corollary of The Principle of Affected Interests presupposes collectivities sufficiently small in their number of citizens to permit sequential interaction with reference to the terms of a governmental decision. Yet the Principle of Affected Interests implies that even the boundaries of the giant nation state exclude vast numbers of people who should be entitled to participate in formulating governmental policy regarding pollution, war, and developmental assistance. Acceptance of the principles most instantly appealing to advocates of participatory democracy consequently leads to an immediate impasse. The impasse occurs because political jurisdictions sufficiently large to cope effectively with the problems of the age, and to encompass the people affected by them, are by those very criteria vastly too large to allow for the preliminary civic interchanges required to give their actions the necessary moral authority.

Dahl can offer only one response to this dilemma. Once again he cautions against elevating any one form (size) of governmental unit to a position of pre-eminent legitimacy. His response (as he quite openly acknowledges) does not resolve the underlying problem of how to formulate an alternative conception of democratic legitimacy. Fragmenting governmental jurisdiction along functional lines merely diffracts attention and encourages special-interest thinking. It undermines the elemental human attachment to the governments we now possess.

But a prior question is whether Dahl's essay actually addresses itself to its subtitle. The criteria for "authority" do not appear in these pages as the test of the rightness of a government. They focus on its capacity to facilitate achievement of one's interests. Does a governmental process insure that the resulting decisions correspond to our personal choices? Does it secure the services of those who are seen by us to be peculiarly qualified by knowledge or skill to render a correct judgment? Does it economize in the consumption of the scarce hours we would otherwise put to other highly valued uses? These are questions any instrumentally oriented consumer would rationally wish to ask of a public service agency. They do not differentiate decisional processes we support because they are effective in meeting our needs from those we regard as legitimately binding without prior assessment of their outcomes. In pressing the very defensible thesis that no government can retain the support of its politically relevant constituency without meeting these three criteria to some extent, Dahl further implies that these necessary conditions for the acceptance of the legitimacy of governmental authority are in some sense also sufficient.

After the Revolution? is in many respects a courageous book. Rather than labeling the

⁸²AR?, p. 55.

⁸³Cf. note 75.

⁸⁴Technology has no bearing on the problem; for while advanced technology can link together people who are spatially dispersed, it cannot — or at least, cannot yet — reduce the time required by any one participant to process, assimilate, and respond to the ideas conveyed by the immediately preceding speaker, and therefore cannot overcome the limits to the number of sequentially interdependent exchanges that can take place within any given period of time.

⁸⁵AR?, pp. 59-67, 82-88.

positions he criticizes, Dahl analyzes them. Courage—and also self-discipline—are required to listen attentively to new formulations of old arguments, and to restate these formulations accurately and connectedly. It requires a clear head and much courage to rethink one's own principles in their light, and yet to affirm without fear of fashion those elements of one's previous commitments that have survived this test. Dahl's brief and timely essay is a work of notable integrity. Yet in that integrity lie also its limitations.

Dahl on Democracy

Any synoptic view of Robert Dahl's contributions to the discipline of political science should suffice to establish one point. To label Dahl a "pluralist," to identify him with the "behavioralist" faction in political science, to write as if he was author of a single book on politics in New Haven is to judge him narrowly and misleadingly. Dahl is all these things. He is also very much more. He is - to put the matter summarily - the most knowledgeable, the most persistent, the most rigorous, the most methodologically varied, and the most prolific student we have of contemporary democracy. In the full history of our discipline, no member has formulated so ingeniously so many central questions about the subject, and none has indicated so clearly and so authoritatively how they might be answered.

To seek some overall assessment of his contributions to our understanding of democracy is therefore a hopeless enterprise. He asks too many questions in too many ways to give validity to summary generalizations. Proper appraisal can only be rendered contextually. This long review has already pursued that end. Even so, his favored paradigms for inquiry do not defy all characterization. And like all such characterization, to highlight is implicitly to suggest the costs and gains of the approach.

Thus in reviewing the totality of Dahl's works one notes repeatedly his nominalist tendency to make more of differences than of similarities. Rigorous and subtle in thought, and keenly sensitive to the texture of human motives and objectives, he is inclined to differentiate where others synthesize. It may seem odd to apply this characterization to a social scientist far better known for his simplified categories and diagrams than for his interest in — or flair for — contextual historical narration. Yet it accounts, I believe, for a rather predictable division of perspective between Dahl and his critics. Where they see capitalism and socialism, he sees a mixed assortment of control

techniques. Where they see social structures, he writes of classless actors, or actors bearing categorical background traits, who pursue multiple objectives with limited resources. Their ruling elites become; by his criteria, contingent coalitions identified through detailed investigation of particular issues. When they treat the phenomenon of "power," he asks "over whom, with respect to what, and with what probability of compliance?" Dahl's formulations are generally by far the more logically compelling in their analytic rigor. Neither Dahl nor his critics, however, have explicitly and persuasively addressed themselves to the perhaps ultimately unanswerable problem of how to decide at what point "marginal" differences become "fundamental" similarities. Accordingly, it seems likely that Dahl's critics will continue to attach more significance than he to the structural elements defining the political arenas he chooses to investigate.

This decompositional – literally analytic – propensity is undoubtedly linked to Dahl's commitment to methodological individualism. Like the economists, whose approach to social science he quite evidently finds congenial, he is most in his element when discussing the rational responses of individual actors to a parametrically established environment. This commitment gives his work a solidity of motivational grounding and a sharpness of focus that much sociological theory so conspicuously lacks. At the same time, he has not yet found a theoretical paradigm for discussing systematically the aggregate consequences of individual choice that result in the emergence of new properties at the systemic level. In this respect his work lags behind the achievements of both classical and contemporary economic theory, and even behind the contributions of the rational behavior theorists in political science. It is partly also for this reason that he is at his least illuminating in discussing at the systemic level such phenomena as power deflation, regime breakdown and transformation, the dynamic relation between coercion and persuasion, and the like.86 Recently, Dahl has taken up aggregate analysis in his comparative studies of democracy; but in doing so, he has adopted a relatively conventional correlational

86For one example among many of the use of rational behavior analysis to identify the emergent properties of large-scale interaction, see Mancur Olson, *The Logic of Collective Action* (Cambridge, Mass.: Harvard University Press, 1965). On the systemic properties of power, see Parsons, "On the Concept of Political Power," on regime transformations, see David Easton, *Systems Analysis of Political Life*. Cf. fn. 55.

approach that obscures many vital questions of systemic functioning.

Perhaps it is also this affinity for economic reasoning that accounts for the curious status of value formation in Dahl's works. When not altogether ignored, the binding force of a commitment to publicly defined values is either discounted without argument or reduced to a formula for realizing private preferences. Dahl's political actors pursue fixed goals derived from their personal utility curves; and while their behavior is a function of the resources available to them, of the degree to which their interests are affected, and of the manipulative skills of their competitors and opponents, they do not appear to change their course of conduct in light of an emerging understanding of their shared responsibilities as members of a collective social order. Thus the U.S. Supreme Court becomes, in one of Dahl's early essays, merely a decision-making agency whose members express preferences that may be overridden by other power groups whose vital interests are affected. Later, the relative stability of the various democracies is accounted for through configurations of interests and resources, and authority itself is discussed in terms of personal needs and individual calculations of opportunity costs. Seen in these terms, "authority" becomes scarcely distinguishable from "influence," and is exercised in much the same manner in relation to very similar objectives. Citizens are consumers who experience no schizophrenia regarding their public and private roles because they have never undergone the transformation described by Rousseau. Such an approach substitutes a welcome realism concerning enduring human motives and calculations for the question-begging terminology in which the concept of political authority is too often discussed. Its price is that of making the game of politics appear far simpler than it is.

These paradigmatic elements in Dahl's work continually inform his analysis of contemporary democracy. His nominalist sensitivities, together with his sympathy for the organizing behavioral premises of traditional economic theory, preclude his acceptance of theories of modern large-scale democracy in which the actions of democratic governments are presented as the enactment of a consensual popular will. Like Joseph Schumpeter before him, he sees the concept of a unified, rationally informed, disinterested, operational, unambiguously articulated popular will as a will-o'-thewisp in complex societies, an artifact conjured up by populistic demagogues and dictators to cover the lack of mechanisms for authentic

public consultation.^{8.7} Such ideational entities have no motive force within his analytic framework. Government in any modern society, he insists, is necessarily predicated, one way or another, on the enduring reality of differences of opinion and interest. Democratic governments are to be distinguished from other regimes by the legitimacy they grant to the expression of such differences, and by their commitment, in undertaking collectively binding actions that unavoidably favor some interests over others, to adhering to formulas that enhance continuing cooperation among people who do not agree.

In Dahl's view, moreover, modern, large-scale democratic (or polyarchal) government is, like other governments, a government by elites. It rests on a political division of labor in which some voices are heard more loudly than others. It is crucially differentiated from other forms of elite government by the effective guarantees it offers to oppositional elites who publicly contest the continued tenure in office of incumbent teams of leaders. For it is mainly such contests, Dahl has contended with unmistakably Schumpeterian logic, that enable citizens to make the choices necessary to enforcing some degree of correspondence between their preferences and the politics of the governing elites. Not by the values a government espouses, nor yet by its class composition. can we judge the authenticity of a democratic order, but rather by the political provisions it makes for holding governmental leaders effectively accountable to the led.

Dahl's major treatises reflect without exception this emphasis on process. Congress and Foreign Policy and Domestic Control of Atomic Energy explored the problems of democratic accountability in governmental policy sectors in which not only citizens but also lay politicians were explicably apathetic and uninformed. The final chapter of Who Governs? develops the paradox of elite protection of democratic values neither comprehended nor shared by the general public, while Pluralist Democracy in the United States amplifies substantively the processes balancing equality with intensity first described in A Preface to Democratic Theory. Politics, Economics, and Welfare presents polyarchy as one of four principal techniques of social control. The social conditions retarding or promoting the institutionali-

87See Joseph Schumpeter, Capitalism, Socialism, and Democracy (London: Allen & Unwin, 1943), Part IV. Dahl shows somewhat greater optimism than Schumpeter regarding the possibilities of rational self-clarification through participation in political discussion.

zation of this technique are then explored comparatively in Political Oppositions in Western Europe, Regimes and Oppositions, Polyarchy, and Size and Democracy before the technique itself is offered a philosophical defense against the strictures of egalitarian participatory democrats in After the Revolution?

The centrality to Dahl's thinking of an institutional conception of democracy - of democracy (polyarchy) as an arrangement requiring political elites to seek the legitimation necessary for governance through a relatively fair and open competition for popular support - is thus beyond serious question. It is a concept that presupposes conflict rather than transcendent consensus; that focuses on actors and coalitions rather than on cybernetic equilibrations; that pays more heed to rational strategies to attain determinate goals than to unconscious psychic needs that may have been fulfilled in the process. It asserts the intrinsic importance of political institutions independently of the class interests that may at any particular time be served by the outcomes. Dahl's theoretical understanding of democracy assumes that democratic politics is more a process for implementing interests than for transforming values, and it is generally inclined to discount rather heavily the controlling efficacy of sentiments of sharing, caring, and mutual trust. As the defining contours of its principal lines of thought were already visible in *The Federalist Papers*, it will, in all probability, remain subject to the kinds of praise and criticism that that treatise on republican government has long evoked.

Dahl's choice of paradigms, and the implications of his choices, are of exceptional significance to all students of politics because, as was suggested at the beginning of this article, his outstanding contribution to the discipline is as its teacher by example. It is through the problems he selects, through the questions he asks, and through the criteria he establishes in answering them, far more than through the findings he reports or the research methods he has developed for obtaining them, that he commands our continuing attention. By and large, he has proved an admirable teacher. He has set a standard of rigor and accuracy, of universalistic theoretical speculation and relentlessly subtle and detailed analysis, that few have matched and none surpassed. In an era of esoteric specialization and turbid expression, of casual scholarship too often propelled by rancorously partisan rhetoric, these qualities cannot readily be overvalued. One must also acknowledge the limits they necessarily entail.

Trends in German Political Science: A Review Essay*

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German political science has come a long way, both in numbers and in sophistication, from those days in the mid-'fifties when a Berkeley political scientist returning from a trip to the Federal Republic could say: "There are fewer people teaching political science in all of Western Germany than in this [Berkeley] department alone." Those were the days when German universities had to be dragooned by their Rektorenkonferenz into establishing at least one chair of political science each and frequently left the discipline precariously dangling between the well-entrenched Faculties of

*Klaus von Beyme, Die politischen Theorien der Gegenwart (Muenchen: Piper, 1972).

Wilhelm Hennis, Die Missverstandene Demokratie (Freiburg: Herder, 1972).

Fritz Scharpf, Demokratietheorie zwischen Utopie und Anpassung (Konstanz: Universitaetsverlag, 1970).

Juergen Habermas and Niklas Luhmann, Theorie der Gesellschaft oder Sozialtechnologie? (Frankfurt: Suhrkamp, 1971).

Bernd Guggenberger, Wem nuetzt der Staat? Kritik der neomarxistischen Staatstheorie (Stuttgart: Kohlhammer, 1974).

Claus Offe, Strukturprobleme des kapitalistischen Staates (Frankfurt: Suhrkamp, 1972).

Juergen Habermas, Legitimationskrisen im Spaetkapitalismus (Frankfurt: Suhrkamp, 1973).

Helmut Schelsky, Die Arbeit tun die anderen. Klassen-kampf und Priesterherrschaft der Intellektuellen (Opladen: Westdeutscher Verlag, 1975). Juergen Dittberner and Rolf Ebbighausen, eds., Par-

teiensystem in der Legitimationskrise (Opladen: Westdeutscher Verlag, 1973). Ossip K. Flechtheim, ed., Die Parteien in der Bundes-

republik (Hamburg: Hoffman & Campe, 1973).

Heinz Laufer, Das foederative System der Bundesrepublik Deutschland (Muenchen: Bayerische Landeszentrale fuer politische Bildung, 1973).

deszentrale tuer politische Bildung, 1913).
Werner Kaltefleiter, Die Funktionen des Staatsoberhauptes in der parlamentarischen Demokratie (Opladen: Westdeutscher Verlag, 1970).
Richard Loewenthal and Hans-Peter Schwarz, Die zweite Republik: 25 Jahre Bundesrepublik Deutschland, eine Bilanz (Stuttgart: Seewald Verlag, 1974). lag, 1974).

Law, Economics, and Philosophy (history). Many universities indeed appointed to the new chairs constitutional lawyers or historians such as Gerhard Leibholz or Karl D. Bracher who then excelled in constitutional law or history.1 Others were fortunate enough to find returning German emigrés, mostly from the United States, who at least were well-trained political scientists or sociologists and sometimes contributed a distinctive Anglicized-German vocabulary to the discipline. And then there was the peripatetic Carl J. Friedrich, equally at home at Harvard and at Heidelberg, whose influence on the discipline was and continues to be profound.

Today German political scientists are far more numerous. They seem to have overcome the generational problem of recruiting and training well-qualified newcomers. They no longer need to lean on other academic disciplines nor be afraid of their intramural rivals. A few of them even became Rektoren (university presidents), Oberbuergermeister, Bundestag deputies, or Ministers of Culture and Education, a source of status as well as of power for the discipline. Yet they are also deeply dissatisfied with themselves and with each other - a circumstance which has created extraordinary intellectual ferment and an unusual awareness of the problematic nature of political knowledge and research. The student revolt and the resulting upheaval in German university life unsettled and continues to challenge the very

¹This review essay could not cover all the current specialties of German political science. I decided, in particular, to leave out international and comparative studies while drawing the line toward sociology rather loosely. The following colleagues generously responded with suggestions for books to include in this review: Kendall Baker, Klaus von Beyme, David Conradt, Charles Foster, Wilhelm Hennis, John Herz, Hans D. Klingemann, P. C. Ludz, Ralph Mendershauen Erzer Persis and Kurt Sontheims. sen, Franz Pappi, and Kurt Sontheimer.

foundations of their understanding of politics and of their role as political scientists. This anguish and uncertainty is reflected in their continual debate over principles and methodology as well as in their formation of antagonistic groups.

Some efforts have been made recently to understand and place in the appropriate context the variety of approaches now current in West Germany, including some in English. David P. Conradt in a recent review article testified to the growing methodological sophistication of a sampling of empirical studies.² But this represents only one of several schools which can be categorized along historical lines or by academic subfields. The "historical schools of political science and sociology" were recently identified by Peter Christian Ludz as follows: (1) the empirical-analytical (Cologne) school of René König and Erwin K. Scheuch from which the Mannheim school of current empirical studies (Rudolf Wildenmann) and Ludz's work on the social structure of the DDR are derived; (2) the critical-dialectic (Frankfurt) school of Max Horkheimer and Theodor W. Adorno, which numbers Jürgen Habermas and many neo-Hegelians and neo-Marxists among its current disciples and has stressed cultural criticism and the analysis of "class rule" in the Federal Republic; and (3) the normative-institutional school of Helmut Schelsky which includes political theorists such as Wilhelm Hennis and many others.³ German scholars have long tended to form schools, and another advantage of categorizing their work in this fashion lies in supplying the appropriate philosophical context for what might otherwise seem like isolated, even obscure investigations or arbitrary methodologies. On the other hand, these historical schools have also tended to intermingle under conditions of high academic mobility, proliferation, and differentiation.

A new generation of German political scientist-sociologists is now clashing over new issues and problems and producing a spate of publications far too numerous to cover in a single

² "The Development of Empirical Political Science Research in West Germany," Comparative Political Studies, 6, No. 3 (October, 1973), 380–391. Cf. the simpleminded dichotomy between empirical political science and historical-institutional-philosophical approaches underlying the content analysis of David Pfotenhauer, "Conceptions of Political Science in Germany and the US, 1960–1969," Journal of Politics, 34, No. 2 (1972), 555–601.

³For further names and details, see Peter Christian Ludz, "Focus on Human Behavior: The present state of sociology and political science in the Federal Republic of Germany," Bildung und Wissenschaft, No. 10-74(e), available through Inter-Nationes, Bonn.

review article. One of the most prolific, Klaus von Beyme⁴ (Heidelberg) in a recent book, Die politischen Theorien der Gegenwart, proposes distinguishing conflict theorists vs. integration systems theorists, depending on their underlying concepts. But he also starts out with the three "theories of science" enumerated above ("ontological-normative," "analytic-scientific," and "dialectic-critical") and, by relating them to various "approaches" (historical-genetic; institutional; behavioristic; functional-structural; and comparative), depicts a quasi-American pluralism of methods that in its interconnections is unlikely to be acceptable to any single writer or school among von Beyme's German colleagues.⁵ Moreover, the bitter debates of the 1960s between the American-style empiricists on the one hand and the dialectic and normative antipositivists on the other soon gave way to the highly political confrontations between the cadres of the New Left and the liberal establishment at German universities. For political scientists in particular, the challenge of the rebels calls for the "democratization" of the whole West German political system, whose painful emergence from the ravages of National Socialism and cold war confrontations they had witnessed, was impossible to ignore. The neo-Marxist critique of Spaetkapitalismus (the advanced industrial state) has been a second decisive challenge. Furthermore, a great debate between Niklas Luhmann and Habermas addressed the questions of functional systems or dialectical analysis which still occupy the social sciences in West Germany. Yet all the while, the intellectual ferment generated by these conflicts also left its imprint on the other, more conventional pursuits of German political scientists who have been producing better and more solid monographs on parties and institutions than ever. I shall attempt to review representative works on various subjects, but the reader must understand that in each category of book reviewed

⁴Among von Beyme's recent books are Die politische Elite in der Bundesrepublik Deutschland (München: Piper, 1971); Interessengruppen in der Demokratie (München: Piper, 1974); books on Italy and on Spain, and his monumental comparative study of European parliamentary systems. See also his new series of translations from German political science with Sage Publications.

⁵Von Beyme, Die politischen Theorien der Gegenwart (Munich: Piper, 1972), esp. pp. 320-322. To be sure, there is more separation than cross-connections among the three theoretical schools when we consider individual scholars and subfields and, especially, if we formulate the "approaches" and "basic concepts" with the caution befitting a sociology of sociology and political science.

there are many more that are equally interesting.

The Challenge of Democratization

"In every segment [of our society]," Wilhelm Hennis⁶ said in an address in 1969, "the general tenor of all demands for change in our social environment can be summarized most concisely in the word 'democratization.' We can state that it epitomizes the most universal socio-political demand of our time." He went on to enunciate what can be considered the consensus underlying two decades of the democratic state of Bonn, namely a pride in a notion of democracy based on Western liberal constitutionalism and belligerently opposed to Nazi as well as Communist totalitarianism. This sense of pride also includes the consensus between the two largest parties, CDU and SPD, on this democratic system, a consensus that "has only recently been challenged as a lack of genuine alternatives." The sweeping demands for "democratization," however, went far beyond political democracy, in the political parties, in local government, or in public administration, and addressed themselves to "the power structures" of schools or universities, business enterprises, and the family. The prevailing German practices in all of these institutions were frequently characterized by the rebels as typical remnants of Germany's authoritarian past.

Hennis's major argument against the advocates of "democratization," at least in 1969, was a flat rejection of the indiscriminate use of the concept of democracy outside of the political sphere. Drawing upon Aristotle and the whole pre-Rousseauean tradition of political philosophy, Hennis argued that it is a fundamental error to confuse the social order of a family or any other nonpolitical organization with the political order and its constitutional-democratic remedies. The demands for democratizing the structure of the traditional German university by giving representation in university governance to the students and to the nonprofessorial academic staff (Mittelbau),

6"Demokratisierung, zur Problematik eines Begriffs," reprinted in Hennis, *Die missverstandene Demokratie* (Freiburg: Herder, 1973), 26-51, at pp. 26-27.

⁷Hennis supplies a good sampling of citations from the press and notable documents, such as the Frankfurt (Friedeburg) memorandum of several professors concerning principles for a new university law and manifestoes of the German SDS (Sozialistischer Deutscher Studentenbund). See also the discussion in von Beyme, *Die politischen Theorien*, pp. 210 and 218ff.

as well as the professoriate, naturally occupied a prominent place in Hennis's critique of the demands for democratization. But he was content to point out that this demand for representing these three elements equally (Drittelparität), had corporatist overtones rather than those of a model case of extrapolitical democratization. "Democratizing" the nonpolitical sphere, to him, was a sure way of politicizing schools, universities, economic production, or the family. "Listening to all parties, participation, even codetermination should be practiced wherever possible — but none of this could be legitimized with 'democracy.' "8

The controversial nature of the concept of democracy is discussed with more detachment, but along essentially similar lines, by Fritz Scharpf (Konstanz), who received some of his legal training and teaching experience at Yale Law School. Scharpf also locates the Western understanding of democracy in its historical context of antifascism and the cold war, and then relates normative democratic theory to different concepts of the political system. Noteworthy are Scharpf's well-aimed asides directed at the contemporary German critics of parliamentary democracy, who often borrowed their ammunition from Carl Schmitt and other authoritarian jurists and theorists of the state. It is a cheap shot at the reality of representative government today, he states, to judge it by the standards of 19th-century liberalism and thus to condemn it for having fallen prey to party discipline, lobbies, executive dominance, and public relations.⁹ The author thoroughly discusses the American debate over pluralism and "democratic elitism" whose critical attitude has been eagerly adopted by German neo-Marxists frustrated by the resistance of their own pluralistic system to radical change. 10 After wrestling bravely with the paradoxes of political participation, the author concludes that the complex nature of contemporary democracies requires a complex democratic theory which is "empirically informed" and normative, but not utopian. It must spring from the kinds of historical traditions and situations that created a responsive two-party system capable of absorbing a pluralism of goals and interests in a

⁸Hennis, p. 47. See also his *Die deutsche Unruhe* (Hamburg: C. Wegner, 1969).

⁹Demokratietheorie zwischen Utopie und Anpassung (Konstanz: Universitätsverlag, 1970), p. 23.

¹⁰ Ibid., p. 55. See also, for example, J. Agnoli and P. Brückner, Die Transformation der Demokratie (Frankfurt: Fischer, 1968), and Scharpf, Die politischen Kosten des Rechtsstaates (Tübingen: Mohr, 1970).

centralized and authoritative procedure for decision making.

Von Beyme's review of the bitter debates over the concept of democracy since the beginning of the protest movement carefully disects the various models of democracy historically and analytically. Von Beyme stresses particularly the democratizers' emphasis on participation in the existing structures of state and society rather than on the old ideal of self-governing communes. Thus he sees particular significance in the interest of political scientists such as Frieder Naschold¹¹ in the democratization of political subsystems such as parties and mass organization and in the organizational and communications aspects of subsystem participation and efficiency. But the increased participation should not keep the organizations and agencies from increasing their effective output at a time when the public makes ever greater demands on governmental services. 12 Participation also has to take account of a number of other peculiarities of the organizations to be democratized, such as the multiple levels and hierarchies of status which often defy the simple dichotomies so popular with the neo-Marxists. Von Beyme shies away from Naschold's assertion that participation increases meaning in the life of the individual or that it is an end in itself. The author concludes that democratic theory must avoid the extremes both of romanticizing participation and of seeing absolute limits of participation in the old division of state and society. 13

The Great Debate: Luhmann vs. Habermas

The democratization issue had barely gotten under way, when there ensued a great intellectual debate which has enthralled large numbers of students and scholars. Much of its significance obviously stemmed from the upheaval at German universities in the late 1960s and beyond — an upheaval which put the two antagonists on opposite sides of the fence. Niklas Luhmann, with his sociological approach, had filled a crucial gap in the field of jurisprudence.

He had also written on the sociology of

organizations.14 His books Soziologische Aufklaerung, Aufsaetze zur Theorie sozialer Systeme¹⁵ and Legitimation durch Verfahren 16 are particularly relevant to "the great debate." In the former, he addressed himself critically to the writings of the criticalanalytical (Frankfurt) school, leading some to comment that his "sociological Enlightenment" was really a Gegen-Aufklaerung (counterenlightenment), an attempt to enlighten the academic public about the "Enlighteners" of the Frankfurt School and their intellectual manipulations. In Legitimation durch Verfahren, Luhmann argued that in a complex modern society, legitimacy is produced by "correct procedures" which, because of the complexity of bureaucratized society, make for public acceptance of the decisions of public agencies regardless of the motives or persons involved. Thus Luhmann's conservative "social technology" clearly made him the defender of the established system against all challengers and democratizers.

Jürgen Habermas had long been recognized as a chief figure of the Frankfurt School by virtue of his writings on sociological theory and politics. ¹⁷ He became identified with the movement for German university reform as one of the signers of the Friedeburg Memorandum on reforms in 1969 and through his writings. ¹⁸ The great debate was initiated by him and took the form of a joint seminar at the University of Frankfurt in about 1969. From this issued the co-authored book *Theorie der Gesellschaft oder Sozialtechnologie – Was Leistet die System-*

¹¹See Frieder Naschold, Organisation und Demokratie (Stuttgart: Kohlhammer, 1969), and, by the same author, Probleme der Demokratie (Opladen: Westdeutscher Verlag, 1971).

¹²See von Beyme, Die politischen Theorien, pp. 218-220.

¹³Ibid., pp. 223-226.

¹⁴Niklas Luhmann's other writings include Theorie der Verwaltungswissenschaften (Berlin: Grote, 1966), Funktionen und Folgen formaler Organisation (2nd ed., Berlin: Duncker & Humblot, 1972), Rechtssoziologie (Hamburg: Rowohlt, 1972), Rechtssystem und Rechtsdogmatik (Stuttgart: Kohlhammer, 1974), and others cited below.

¹⁵ Luhmann, Soziologische Aufklärung, 3rd ed. (Opladen: Westdeutscher Verlag, 1973).

¹⁶Luhmann, Legitimation durch Verfahren (Neuwied: Luchterhand, 1969).

¹⁷See especially Habermas's Strukturwandel der Oeffentlichkeit, 3rd ed. (Neuwied: Luchterhand, 1968); Theorie und Praxis, Sozialphilosophische Studien, 3rd ed. (Luchterhand, 1969), (available in English as Theory and Practice [Boston: Beacon Press, 1969]; Beacon Press also published his Legitimation Crisis [1975]). See also his Erkenntnis und Interesse (Frankfurt: Suhrkamp, 1968).

¹⁸Habermas, Protestbewegung und Hochschulreform (Frankfurt: Suhrkamp, 1969); Technik und Wissenschaft als Ideologie, 2nd ed. (Frankfurt: Suhrkamp, 1969); and Zur Logik der Sozialwissenschaften (Hamburg: Zerschlagt das burgerliche Copyright, no date, 1969?).

forschung?¹⁹ which has already sold the astounding number — for the German academic market — of more than 100,000 copies and given birth to several supplement volumes in which other scholars have argued the merits of the Luhmann-Habermas debate.²⁰ Lest the reader conclude that this debate was a dramatic confrontation between the revolutionary demands for Systemveraenderung (system change) in Western Germany and Luhmann as the "great white hope," we should note that there was rather less to the debate than meets the eye. The two principals, in fact, became quite friendly and continue to meet and to exchange their manuscripts for comment.²¹

The book of the great debate, Theorie der Gesellschaft oder Sozialitechnologie - Was Leistet die Systemforschung? consists of an essay on systems theory and on meaning by Luhmann, an elaborate critique of all of Luhmann's writings by Habermas, and a rebuttal by Luhmann. Luhmann's systems theory is broadly philosophical and owes much to Edmund Husserl's phenomenology as well as to Hegel, Max Weber, Heidegger, and Bertalanffy. It sets off the social system (and subsystems) from the outside world by boundaries at which the complexity of the world is "reduced" by the selective human understanding that creates systems of meaning (Sinn). As societies evolve toward greater complexity, they separate persons from the increasingly differentiated and interrelated roles which constitute their structures and expectations of behavior. Luhmann finds Talcott Parsons's general theory of action problematic as a theory of the social system and prefers instead to base his systems theory on subjective meaning in the sense indicated above, as the "form" in which people selectively "experience" certain aspects of the world. Their "reduction" of the complexity of the outside world operates by excluding some of the many possible alternative systems of meaning, thus "constituting" their particular social

system.²² Luhmann relates meaning further to problem-related *functions* in decision processes²³ and calls his approach "functional-structural" with appropriate stress on functions.²⁴

Habermas prefaces his critique with "preliminary remarks on a theory of communicative competence," a seemingly esoteric essay on linguistic theory which suddenly turns into a pointed discussion of democratic legitimacy and consensus. True discourse among rational equals, without manipulated barriers to communication, the author appears to say, is the only way to legitimize action on behalf of the community. "Communicative action" presupposes rational understanding in the people affected by it, and "the expectation of [its] legitimacy is based on the assumption that the general principles of [such action] must be capable in the eyes of the actor of justification by uninhibited . . . discussion" (p. 119). Ideologies, for example, can be barriers to communication that make a fiction of the rational individual and create a false sense of legitimacy. No genuine consensus can be formed without a common sense of truth, a rational ability to distinguish reality from appearance, and an absence of coercion of any sort. This is the Kantian foundation of Habermas's "critical social theory."

According to Habermas, Luhmann's central thesis, namely that functional analysis is the only admissible route toward rational decision is wrong, and his formulation of legitimacy in highly complex societies is a technocratic and scientistic attempt to stifle all tendencies toward democratization. Habermas further charges that Luhmann does not posit the rational individual as the source of legitimacy, but recognizes instead all kinds of other sources such as systems efficiency. Habermas also doubts that a system theory based on meaning (Sinn), can ever achieve the precision of theories of mechanistic or organic systems. He questions in particular Luhmann's prescription

¹⁹Theorie der Gessellschaft oder Sozialtechnologie (Frankfurt: Suhrkamp, 1971).

²⁰See, for example, Franz Maciejewski, ed. *Theorie der Gesellschaft oder Sozialtechnologie*, Theorie-Diskussion, Supplement 2, Beitraege zur Habermas-Luhmann-Diskussion von Wolf-Dieter Narr et al. (Frankfurt: Suhrkamp, 1974).

²¹Since 1971 Habermas has been doing research and theoretical work on change and crisis in postindustrial societies at the Max Planck Institute in Starnberg together with several other representatives of the Frankfurt School. In these pleasant surroundings he is nonetheless painfully cut off from normal student and academic contacts. The University of Munich denied him a teaching appointment and has refused him the normal courtesies of academic cooperation.

²²Meaning, according to Luhmann, is not simply transmitted but mutually actualized in intersubjective communication. It is carefully distinguished from mere information and distinguished from simplistic models of communication. On the "reduction of social complexity," see also Luhmann's Vertrauen, Ein Mechanismus der Reduktion sozialer Komplexitaet (Stuttgart: Enke, 1968).

²³On Luhmann's concept of function, see his "Funktion und Kausalitaet" (1962), reprinted in Soziologische Aufklaerung, pp. 9ff.

²⁴For a systematic review see also Heinrich Busshoff, *Systemtheorie als Theorie der Politik* (Pullach: Verlag Dokumentation, 1975).

for ad hoc crisis management divorced from any analysis of the causes of social crises.²⁵ But even where Luhmann applies his macrotheory of the social system, it appears to this writer, it can serve to manage crisis and conflict just as the macrosocietal theory of the New Left is designed to aggravate them. Either way society is "steered" by a macrosocietal understanding of its structure and process. The two antagonists, in fact, also share, with a bow to Husserl's phenomenology, a skepticism about the universal claims of a behaviorally oriented social science which does not recognize the structuring of society by meaning (Sinn). Their concepts of ideology, on the other hand, differ in that Luhmann's is based on systemic function while Habermas's is based on the deception and manipulation of the individual, who is thereby prevented from grasping his or her true interests. The nub of their disagreement, in any case, remains Luhmann's technocratic concept of legitimacy:

No one is in a position to form convictions for all the decisions [facing his society]. A concept of legitimacy by conviction misreads the extreme complexity, variability, and contradictory nature of the subjects and premises of the decisions that need to be dealt with in the political-administrative system of modern societies. This complexity of modern societies. This complexity of modern societies the generalization of acceptance of decisions, less by well-motivated convictions than by an acceptance free of motive and independent of the idiosyncracies of particular individuals.²⁶

To Habermas, this is an obscurantist ideology which battens on the depoliticization of the public and reverses all representative principles of democracy — in short, it is a "social technology" as the title of the book puts it.

There is no need to go into great detail about Luhmann's rebuttal: Habermas's critique, he says, is thorough and sophisticated but far too dichotomized into pro- and anti-establishment perspectives to do justice to the complexity of modern social and administrative problems. The kind of democratic discourse among rational individuals on which Habermas would like to base legitimacy and even truth is a myth, says Luhmann, and bears little resem-

blance to what people in modern society actually engage in when they speak with each other.27 Habermas's whole approach to legitimacy, in fact, stems "from a time which was not yet familiar with the problem" of communication in complex modern society (p. 355), presumably the Enlightenment or at the latest, the nineteenth century. Luhmann does not mean to question the historic quest for individual emancipation and democratization so much as their traditional link to individual rationality or the triumph of reason. Functional-structural systems theory, the author hopes, is beyond these traditional points of reference and beyond the confrontations of establishment and opposition. The "great debate," however, has remained close to these important questions of modern political theory in the years it has continued at German universities.

The Neo-Marxist Challenge

The rapidly changing directions of left-wing opposition at German universities produced a fine vintage of neo-Marxist theories after most of the activists had wandered off into the partisan Young Socialists (Jusos) or Young Democrats(Judos).²⁸ Any discussion of neo-Marxist thought in Germany requires an explication of Spaetkapitalismus (advanced capitalism), as well as of the theory of "state monopoly capitalism" (Stamokap) and other criticisms of advanced capitalism.

The label Spaetkapitalismus for organized, interventionist, or advanced capitalism is problematic in suggesting an all-too-familiar, transparent kind of word manipulation: "capitalism in its late phase" simply asserts its imminent demise, instead of demonstrating the necessity of such a development. Translating the bad label as "late capitalism"²⁹ compounds the problem to ludicrous proportions, for the mission of all these neo-Marxists is precisely to explain why the patient, in spite of recurrent crises and afflictions, is still as far from being "the late" capitalism as ever. A second key concept is that of "crisis," which occurs in

²⁵Luhmann proposed this in Zweckbegriff und Systemrationalitaet, Ueber die Funktion von Zwecken in sozialen Systemen (Tuebingen: Mohr, 1968), p. 224. Theories of crisis are particularly important to critical analytic theory as will be seen below.

²⁶Luhmann, Legitimation durch Verfahren, p. 32. Earlier in the same book, the author speaks of a generalized willingness to accept even decisions of undetermined content within certain margins of tolerance." *Ibid.*, p. 28.

²⁷Here Luhmann cannot pass up the temptation of describing in an aside the dogmatic and contentious nature of discussions in "supposedly free, democratic groups dedicated to the critique and reform of social!s." Theorie der Gesellschaft oder Sozialtechnologie, p. 340; see also pp. 391–392 and 399–401.

²⁸On these young groups see below, and the sources cited in footnote 45.

²⁹See Habermas's note "What Does a Crisis Mean Today? Legitimation Problems in Late Capitalism," in Social Research (reprint), no date.

many titles of books and articles of this genre today. A much-quoted collection by Martin Jaenicke³⁰ even purported to present a new area of comparative "crisis research," by roaming far and wide among explanations of the "crisis" of political decision systems (Deutsch), developing areas (Rittberger), international systems (Hermann), and capitalist systems (Offe, Altvater), with predictable analytical vagueness. It is particularly disappointing to see the carelessness with which a powerful tool of comparative analysis, such as Marxist theory, is used by today's neo-Marxists, not to mention their seeming unwillingness to compare in detail the striking differences among various advanced capitalist systems and the effect of these differences on system legitimacy.31

The various neo-Marxist schools differ also in how they visualize the crisis of Spaetkapitalismus which, after all, is the source of their vitality, of their legitimacy, and of their own theory-making. The chief differences relate to the relationship between the economic and the political system. Orthodox and neo-orthodox Marxism and certain Stamokap (state monopoly capitalism) theorists continue to expect the great crisis chiefly in the economic realm,32 for they view the state as merely the political manifestation of the economic contradictions and the instrument of individual capitalist interests. At the other end of the scale are those such as Claus Offe and Habermas who foresee the coming crisis as a political breakdown, a critical loss of legitimacy of the state and the dysfunctional politicization of the masses, led by revolutionized students and intellectuals rather than by the leaders of the proletariat.33 The intimate interpenetration of

³⁰Jaenicke, ed., *Herrschaft und Krise* (Frankfurt: Suhrkamp, 1972). This book is the outcome of a 1972 conference on "crises" in Berlin.

³¹Habermas's use of analogy between the crisis of Spaetkapitalismus and the body's crisis or the "crisis" in Greek tragedy is hardly a tool of social science analysis.

32A good guide through the current German literature is Bernd Guggenberger, "Oekonomie und Politik — die neomarxistische Staatsfunktionenlehre," Neue Politische Literatur (1974), 20, No. 4, pp. 425-471 whose work is discussed more fully below. See also von Beyme, Die politischen Theorien, pp. 64-87, and the sources cited there. Examples of this literature are Claudia von Braunmuehl, ed., Probleme einer materialistischen Staatstheorie (Frankfurt: Suhrkamp, 1973); Wolfgang Muller and Christel Neusuess, "Die Sozialstaetsillusion und der Widerspruch von Lohnarbeit und Kapital," Sozialistische Politik II, (June, 1970), no. 6/7, pp. 4-67; and Ernest Mandel, Der Spaetkapitalismus (Frankfurt: Suhrkamp, 1972).

33Guggenberger also suggests, as a third, in-between group, writers such as R. Miliband, N. Poulanzas, A. Gorz, P. Sweezy, and P. Baran. economics and politics no longer permits the neo-Marxists to deny the importance of the state or its relative autonomy as a factor that at least temporarily has been able to save capitalism from its long-predicted collapse. On the other hand, some neo-Marxist writers exhibit at times a curious nostalgia for the simple days of nineteenth-century capitalism when Marxist analysis was not yet bedeviled by an interventionistic state.

Bernd Guggenberger, a very perceptive critic of the neo-Marxist literature, directs attention to the striking differences among the prescriptions of the various schools. Even where these schools agree, say, on the role of the financial oligarchy and the great monopolies in directing the state, their proposed solutions differ in telling ways. Guggenberger has little patience with the East German school, 34 which, according to him, merely proposes to replace the oligarchic, parasitic elite there with another more benevolent, even vaguely democratic technocratic elite.35 He thinks more highly of the French Communist theoreticians who would follow up the takeover with a decentralization of economic decisions to producers' organizations and with the democratization of planning, a program similar to the ideas of industrial democracy achieved through workers' councils. In the prolonged discussions among Stamokap theorists and various gauchiste groups of the German Young Socialists (Jusos), 36 all the participants have striven in their own ways to "turn around" the West German SPD while it is still in control. Some prominent Jusos have questioned especially the simplistic "undialectical" assumption that a complex social organization such as the state can ever be a mere instrument in the hands of anybody.37 Hence the Jusos's "double strategy" combined

³⁴Guggenberger points out that their theories of West German imperialism and capitalism originated among the members of the Institute of Social Sciences of the Central Committee of the East German Communist Party (SED) and that their advocacy by the Soviets, the SED, and the West German Communists (DKP) is strongly colored by the current popular front strategy.

³⁵The avoidance of true democratization, of course, corresponds to the bureaucratic-authoritarian model of Soviet and East German socialism. See also Margaret Wirth, *Kapitalismustheorie in der DDR* (Frankfurt: Suhrkamp, 1972).

³⁶Guggenberger covers them well in his Wem Nuetzt der Staat? Kritik der neomarxistischen Staatstheorie (Stuttgart: Kohlhammer, 1974), pp. 67-75.

37As the author points out, there are significant links here with the theories of the state of Hermann Heller of the Weimar Republic whose writings have recently been reissued. Guggenberger, Wem neutzt der Staat?, pp. 135-136, fn. 210 and 213.

the political mobilization of the public "at the base" with attempts to influence policies and the distribution of power at the top. The inevitable linkage of these theories to the political strategies of the day make these perspectives more perishable than the reinterpretations made by Offe and Habermas, who refrain from proposals of strategy and action.

Claus Offe's Strukturprobleme des kapitalistischen Staates³⁸ is a highly original political sociology of Spaetkapitalismus which consists of several separate essays on various aspects of the relationship between the political-administrative system and economic and social policy. It has filled a major gap between the literature on planning and administration on the one hand and the insufficiently state-oriented neo-Marxist literature on the other. Offe's thorough acquaintance with American social science and systems theory and his critical-analytical training make him a potent analyst of critical problems of legitimacy, technocracy, and public policy in advanced capitalism.

The strength of Offe's and Habermas's "revisionism" (as compared to the Stamokap and neo-orthodox Marxists) lies in its sophisticated analysis of the weaknesses and incongruities of the "political-administrative system" of contemporary democracies. Its weaknesses, on the other hand, owe much to the cumbersome imprecisions of the Master - Marx himself and to the circuitous logic that sometimes characterizes his disciples. Thus they may at once open themselves to accusations of taking liberties or being selective with the writings of Marx and Engels, and of taking them too seriously as a point of departure of actual historical development.³⁹ The reader, for example, may find Offe's "dynamic interpretation" of Marx unacceptable, or may balk at Offe's treating the "deviations" of actual capitalist development (such as the rising role of nonproductive labor) from the original nineteenth-century model as further "contradictions" of capitalism. But thereby hangs Offe's tale of how the contradictions in the structures

³⁸Offe, Strukturprobleme (Frankfurt: Suhrkamp, 1972).

³⁹The extraordinary spell which Marxism has cast over many young German intellectuals can be illustrated by an incident during a conference on comparative fascist movements in Bergen, Norway, in 1974. There was great puzzlement over the fact that in many fascist movements transport workers were among the few proletarians to join when a young German participant interjected: "I have a plausible explanation. These transport workers were at the intersection between the process of production and the process of circulation. Da musste doch etwas schiefgehen! (So something was bound to go wrong.)"

of evolving Spaetkapitalismus will in time exceed the capacity for "social integration" of a society.

By combining the integrative-system approach with the neo-Marxist theory of structural conflict, Offe is able to deal with the resistance of pluralistic systems to structuralist analysis. The legitimacy of the early capitalist state, according to Offe, required none of the current efforts of the regimes under Spaetkapitalismus to ingratiate themselves with the public. Even the market mechanism and the relation between capital and labor provided a kind of imminent social justice which today has been usurped by the administered prices and arbitrary interventions of the state. The state of Spaetkapitalismus thus has all of the old social conflicts and many new ones lying undigested in its greatly extended belly. Its ability to steer society knows few limits, and yet its need for legitimizing its steering power (and specific policies) was never greater. Technocracy and planning, Offe argues along Habermasian lines, have destroyed the bourgeois institutions of public consensus and thereby their own source of legitimacy just when they needed it the most. Their new forms of legitimizing strategies are short-run, and the class or other bias of the policies easily seen through. Spaetkapitalismus can give the masses bigger handouts and better living conditions and yet forever falls behind their rising demands. Qualitative emancipation rather than the "reproduction of labor" or proletarian impoverishment is the well-springs of public discontent.

Habermas in his Legitimationskrisen im Spaetkapitaiismus⁴⁰ goes beyond analyzing the level of technological and functional mastery to discuss the crises of meaning in modern industrial life, including what Daniel Bell and Herbert Marcuse have called the "cultural crisis" of capitalism. Thus he also goes beyond Marx and the orthodox premises of Marxism toward additional definitions of "contradictions" and "crises": crises of the ecological balance, of human alienation, of the international system, and most of all, of the legitimacy of the political-administrative system. The state has a hard time meeting all the demands, he

40See also the critical remarks of Wolfgang Fach about Habermas's "legitimation logic" in Zeitschrift für Soziologie. 3 (1974), 221ff. Another one of the landmark debates involving Habermas and the late Theodor Adorno was the discussion over the role of "positivism" in sociological method which pitted Karl Popper and Hans Albert against the Frankfurt School. See also Hans Albert and Ernst Topitsch, eds., Werturteilsstreit (Darmstadt: Wissenschaftliche Buchgesellschaft, 1971).

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argues, since it has to act as the "ideal capitalist" representing many particular and frequently conflicting capitalist interests, as the state capitalist with enterprises and responsibilities of its own, and as the representative of public interests and demands readily backed up at the ballot box. Short of the "administrative manufacture of meaning," or deceit by manipulation of opinion, the state will find the cultural sphere rather resistant to its quest for legitimacy. It is even less able to safeguard the values of bourgeois culture which once upon a time were the values behind its own smooth operation. The crisis of bourgeois culture, exemplified by fascist regressions, scientism, and modern art, according to Habermas, also erodes the cultural basis of capitalism, leaving the field to countercultural revolt.

The reader must not imagine that the wave of West German prophets of the New Left went unchallenged. Countless debates and countermanifestoes, as well as personal struggles in the SPD, at the universities and schools, and among the bureaucrats have been waged in order to "excommunicate the excommunicators." The New Left has mounted campaigns against SPD politicians and other prominent figures of the establishment, and now the courts, state government administrators, and educators are conspiring to bar their left-wing enemies from positions in the system.

The Conservative Rejoinder

The last word on the intellectual ferment of our time in Germany, at least for the time being, fell to Schelsky, book with the revealing title Leave the Work to the Others: The Class Struggle and Priestly Rule of the Intellectuals.41 To Schelsky, anyone engaged in "ideological critique" today is really trying to shift our attention from the problems of social change and group antagonism to questions of faith or meaning which ultimately tend to promote rule by the "ideology-producing class" - the intellectual high priests - over those who produce goods and services. With a bow to Max Weber and Georges Sorel, the author draws contemporary parallels to hierocratic rule. The expectations of salvation and the perception of current miseries preached by the "intellectu-

⁴¹Schelsky, Die Arbeit tun die anderen: Klassenkampf und Priesterherrschaft der Intellektuellen (Opladen: Westdeutscher Verlag, 1975). The author, among other things, accuses German political science and sociology of a one-sided emphasis on power and conflict research (especially peace research) rather than on democratic legitimacy, pp. 25–26.

als"⁴² are capable, he says, of destroying all dedication to working in the here and now of the industrial societies or the Third World. Instead they mobilize the masses against their alleged oppressors and in pursuit of an elusive millennial goal which is strictly of their own making. The current miseries are mostly "borrowed" from faraway situations such as "death in Vietnam," repression in Spain, or hunger in Africa, and transferred emotionally to the tender sensibilities of the upper-middle classes to motivate the intellectual grab for power.

Schelsky treats the rationalistic critical method of the Frankfurt School as harshly as he does any political or university reform movement. He describes them all as a species of Anabaptist revivalism generating new meanings and salvational goals, promising to free people from the daily "pressure to achieve," claiming an absolute monopoly on truth, and attributing a "false consciousness" to anyone who does not agree with them. Since the producers of ideology, or meaning, are not subject to the same instant refutation as are people involved in production and marketing, there is no natural corrective for producing bad ideology and, at best, only the competition of different ideologies which may be no better. The ideological goal may be an elusively formulated "democratization" or the battle of "intellectualized women" over West German abortion laws (Criminal Code Paragraph 218). It may involve seeing the enemy in science and technology, in the governing elites, or in other established authorities. But it is always based on the postulation of an enemy and on a claim to leadership and power. Schelsky particularly deplores the defamation of the achievement motive among the German intellectuals (following Herbert Marcuse). This German "intellectual class," says Schelsky, seeks to control the leisure-time activities of society. Its strength is based on its practical monopoly of socialization (education) and information, on its power over words, and over the language. It attempts to conquer society by the "transvaluation of words," by changing the commonsense meaning of such terms as "democracy" or "public opinion."

Schelsky's rejoinder ends with a bitter diatribe against the "treason of the intellectuals" who have tied their services to society to a bid for power and respond with a "great refusal"

⁴²Schelsky devotes considerable space in his book to a systematic description of seven categories of intellectuals, ranging from the technical and administrative intelligentsia, artists, social scientists, teachers, journalists, and public relations people to the priests of established religion, pp. 98–117.

when this bid is not readily granted. His major target is the sociologists and political scientists (or, in German nomenclature, the political sociologists), in other words, his own profession. His criticism is a systematic assault on the foundations of the discipline itself, an "Antisoziologie" aimed at "sociological enlightenment." He also comments searingly on the "enlighteners" in education, among the purveyors of the social gospel, and on engage writers and journalists. In Schelsky's angry vision, the news magazine Der Spiegel becomes a "class struggle sheet" and Nobel prize-winning author Heinrich Böll the "cardinal and martyr" of social salvation.

Studies of Institutional Legitimacy

The challenges of neo-Marxism and "democratization" to the legitimacy of West German institutions, and Schelsky's doubts may suggest that the system is on the verge of collapse, but this is not the case, nor has there been any reduction in the quantity and quality of institutional studies. On the contrary, as a few selected examples will show, the current West German literature on political parties and institutions can easily stand comparison with the flowering of political thought and of the behavioral school in Germany. As the editors of a major symposium on the West German party system put it, the core problem for investigation is "how far the democratic challenge of the extraparliamentary opposition (of 1966), the student movement (of 1967/1970) ... and especially of the youth organizations of the major parties ... has been taken up and translated politically by the party system."43 Since the major parties of the West German "party state" wield a monopoly of power, the literature on German parties44 had already tended to raise questions of internal party democracy in connection with the debates of the early 'sixties over the law implementing the constitutional mandate of political parties, (art. 21) the electoral reform, and public campaign financing.

When the crises of the late 'sixties began to pile up, therefore, students of German parties were already deeply involved in probing relevant aspects of the internal life of the parties —

their social composition, the activities and attitudes of their members, and the career paths of party politicians. These earlier projects are well-represented in the Dittberner and Ebbighausen symposium, as are studies of interest groups within the two largest parties, e.g., labor in both the CDU and SPD, and the Economic Council (business) of the CDU. They are, moreover, integrated with a searching and fairly exhaustive analysis of the Young Socialists (Jusos), Young Democrats (FDP), and the Young Union (CDU),45 as well as the newly (1968) admitted Communist party (DKP). All party studies, of course, are quickly out of date. Within a few years, much of the "systemchanging" potential of the Jusos, and of the DKP and more recent groups, appears to have spent itself, even though the wave of politicization they represent is still felt in local mass actions and "citizens initiatives." Juergen Dittberner's cautiously expressed judgment that the West German party system has shown astounding stability throughout the upheaval of the late 'sixties and early 'seventies is likely to endure, even if recent elections did not usher in a period of SPD dominance comparable to Adenauer's dominance in the late 'fifties. In this respect, then, stability under fire spells legitimacy, because it demonstrates that the procedures of formal democracy have indeed taken the place of the traditional sources of legitimation in Germany. The pluralism of groups and interests which the Weimar Republic was unable to absorb is now well within the integrative capacity of the system. Even the vulnerability to economic crisis and political extremism which has so often characterized West German democracy has not so far imperilled the survival of constitutional democracy there.46

45The activities of the Young Socialists, as the paper about them points out, generally remained well within the traditional range of issues in the SPD, but strengthened the Old Left elements at the expense of the dominant center-right coalition which has supported the SPD leadership. On these young partisan organizations, see also Helmut Bilstein et al., Jungsozialisten, Junge Union, Jungdemokraten (Opladen: Leske Verlag, 1971). Attention should also be drawn to the excellent collection of materials on West German parties by Ossip K. Flechtheim, ed., Die Parteien in der Bundesrepublik (Hamburg: Hoffman & Campe, 1973).

46Dittberner and Ebbighausen, Parteiensystem, pp. 469-475. Dittberner, however, still attributes a considerable share of the integrative capacity to the economic prowess of the system to generate and distribute prosperity, which implies that a more massive economic failure than has been experienced so far in West Germany could be fatal. On the other hand, the author leaves little doubt about his low estimation of the neo-Marxist analysis of the structural potential for crisis, pp. 481-488.

⁴³ Parteiensystem in der Legitimationskrise, ed. Juergen Dittberner and Rold Ebbighausen (Opladen: Westdeutscher Verlag, 1973), p. 31.

⁴⁴The introduction by Ebbighausen carefully puts the state of German party studies into the appropriate context of German postwar history and international comparative party studies, including the contributions of American scholars.

The faith in the institutions of West Germany also shines through in varying degrees in works such as the studies of German federalism of Heinz Laufer, who seems less concerned that a political threat will arise than that West Germans may not understand and appreciate their federal institutions. He would like to see more information about the federal-state relations taught in the schools and transmitted by the media. His work on the federalism of the Federal Republic exemplifies the research of many well-trained young German political scientists and some older ones who share a firm grounding in constitutional law and administrative practice.47 Laufer has also published research about other little-known institutions. such as the important, semi-official Laender offices in Bonn, which constitute a link between the Bundesrat delegations, the Land governments, and the federal government.48 This particular study skillfully combines institutional knowledge, constitutional history, and interviews to penetrate the bureaucratic mysteries of the Bundesrat type of federalism, in which lobbying practices and administrative relations are inextricably intertwined. Another example of such institutional studies is The Head of State in Parliamentary Democracies 49 by Werner Kaltefleiter, who is better known from his election studies. A classical comparative study, The Head of State compares the parliamentary systems of Great Britain in the 1930s, Sweden, Denmark, Norway, Belgium, France under the Third and Fourth Republics, Italy, Finland, Austria, and Weimar Germany in order to create a functional model for the interpretation of the office in the Bonn Basic Law. The Federal Presidency of West Germany embodies dangerous contradictions of function which so far have not surfaced only because its first incumbent, Theodor Heuss, took an ex-

ceedingly narrow view of his role and because Konrad Adenauer, in 1959, decided not to run for the office. The author went the painstaking, classical route of checking all his comparative material with colleagues in the countries in question. This is a typical example of the practical, or if you will, policy relevant, purposes of institutional comparison.

These institutional studies show neither the critical ideological bent of the challengers nor the subtle subversions of the spirit of political science research alleged by Helmut Schelsky. Even freer of bias is the symposium The Second Republic edited by Richard Loewenthal and Hans-Peter Schwarz; 50 this tome ranges over a comprehensive list of topics, from the origins, institutions, and parties of the Federal Republic to social change and a variety of policy topics. The editors and most of the contributors emphatically maintain that the Federal Republic, after a rather unpromising start, has developed a "new legitimacy" and, to everyone's surprise, has turned into a "political miracle" (Loewenthal). This view differs from the uncertain assessments made only a decade ago, when many observers saw in the Federal Republic a "fair-weather democracy lacking responsible democrats."

The Second Republic presents a succession of tableaux and scenarios, beginning with an analysis of the situation of occupied Germany by Hans-Peter Schwarz and detailing the curious mixture of traditional, especially bureaucratic, elements and the makings of a conservative, post-totalitarian democracy, German-style. Schwarz, who has written a classic work on the origins of the Federal Republic,⁵¹ refutes much of the revisionist German literature and its claims that the conservative path of the Federal Republic was chosen in 1949 as a result of political intrigues and occupation pressures. It was the voters who opted against socialism and in favor of the West, thereby creating a new political entity and identity. The major party alignments actually antedated the republic, and their historical positions at the outset were undoubtedly crucial to the political development to come: the SPD in revulsion from Communist domination on the one hand and

⁴⁷Laufer, Das foederative System der Bundesrepublik Deutschland (Munich: Bayerische Landeszentrale fuer politische Bildung, 1973) is noteworthy also for its exhaustive coverage of the financial aspects and cooperative practices of West German intergovernmental relations.

⁴⁸Laufer and Jutta Wirth, *Die Landesvertretungen* in der Bundesrepublik Deutschland (Munich: Goldmann Verlag, 1974). He also wrote books on the parliamentary state secretary and on the constitutional Court.

 ⁵⁰ Loewenthal and Schwarz, Die Zweite Republic.
 25 Jahre Bundesrepublik Deutschland, eine Bilanz

from German rearmament in NATO on the other; the CDU/CSU between the devil of the Nazi past of the national bourgeoisie and the deep blue sea of unprecedented European and trans-Atlantic entanglements. As prosperity grew and the cold war waned in the 'sixties, social and generational change gave birth to the new pressures that erupted into new forms of political extremism of the right and the left with the economic and political crisis of 1966-1967. The new ferment, including the "sociological enlightenment" of rebellious youth, tended to benefit the SPD at the expense of the CDU, and was probably accelerated by the change at the helm of the state.

Where will the Federal Republic go from here, for its second quarter of a century? Loewenthal emphatically discounts the Cassandra cries of polarization and impending civil war from today's right-wing observers, just as he discounted similar left-wing warnings of a protofascist development of the Grand Coalition of a decade ago. Like his co-editor, he is confident of the democratic consensus among the major parties, and he feels this consensus is the guarantee of future legitimacy regardless of which party happens to have the popular mandate to carry out its choice of policies. He also hopes for "democratization" so long as it does not induce institutions to get out of the control of the democratic society, or even to be hostile to it. Schwarz, on the other hand, sees the key to future legitimacy in the continuation of the successful marriage between political democracy and a thriving industrial economy. He also expects the aversive experiences of Nazi and Stalinist totalitarianism to benefit West German dedication to democracy for at least another decade until generations take over who never witnessed either form of tyranny. Strong liberal democracies, he feels, can put up with a certain amount of anarchy and violence. Schwarz does not discount the crisis potential of the German division (or related aspects) or of a neutralization of Central Europe heralded by the European Conference on Security and Cooperation, nor does he make light of the dangers of a domestic political crisis, to be sure. In fact, he sketches several scenarios of a rightor leftward swing, and especially of a popular front of East German Communists and their West German allies among the DKP and its Young Socialist and New Left friends. But. aside from a Swedish type of development based on a moderate SPD majority in West Germany, none of the scenarios appears to be in itself capable of success without strong countervailing forces that would right the balance. It remains a matter of judgment whether the Swedish road would be a "road to serfdom." And so he ends the book on an upbeat of hope, as long as West German internal affairs remain undisturbed by worldwide crises.

Any attempt to assess a whole profession in a single review article is bound to give at best a partial view, no matter how wide the net is cast. In this case, the limited coverage and the state of war within the discipline cannot but leave a distorted and superficial image. The different schools of thought, the great debates over democratization and systematic approaches, and most of all the burning concern on all sides with some of the most basic issues of politics and political science - the kind that hardly ever get raised in our own compartmentalized discipline - make German political science appear vibrant and dynamic. It may not be logically satisfying to see the obvious noncongruence of the questions asked and the answers given by different schools, even though their methods have obviously become more sophisticated. The heated confrontations frequently generate more noise than real communication. Nevertheless, the sympathetic observer finds himself wishing that the years of upheaval in American political science had left more of this real concern with the vital issues of politics.

On Social Psychological Handy Work: An Interpretive Review of *The Handbook of Social Psychology*, Second Edition*

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How should one evaluate a collection of forty-five long articles on every aspect of social psychology from the behavior of "infra-human animals" to "laughter, humor, and play," from collective behavior to aesthetics? The reviewer cannot help but blanch at the variety of material available in these five volumes. An additional impediment is the passage of time. Although this new edition of the Handbook appeared in 1968-69, most of the articles were written years earlier, and most of these articles draw on other research conducted in the 1950s and early 60s. One is therefore hard put to know whether the "new Handbook" is really new at all - or at the very least how representative it is of social psychology today. In any event, the discerning contemporary reader will notice what appear to be significant gaps in the Handbook.

It should also be admitted that no human being can read 4100 pages of demanding text and the results of literally thousands of studies with unflagging attention, particularly when (as in the case of cognitive dissonance theory), he encounters many of the same considerations in overlapping contexts. Eventually one finds oneself wondering as much about one's own health as about what is being reviewed. Obviously, one urgently requires some guidelines.

Luckily, such guidelines become easier to devise once we realize that social psychology is hardly an esoteric field to most political scientists. Knowingly or unknowingly, every political scientist incorporates social-psychological assumptions into his research. The student of election campaigns applies social-psychological assumptions about attitude change and formulation; the student of legislative coalitions applies social-psychological assumptions about

leadership behavior in small groups; the student of comparative politics applies social-psychological assumptions about the interplay of culture and personality. And so on, ad infinitum. The fact is that social psychology is the very skeleton of political science, the scaffolding upon which we hang our more specialized investigations. Because it seems reasonable to assume that the corpus of political science can be only as strong as its socialpsychological skeleton, most political scientists can benefit from greater familiarity with social psychology; the new Handbook seems an obvious place to begin. Specifically, I will attempt to discover how well the Handbook familiarizes the political scientist with social-psychological ideas that might be of service to him or her in research and teaching.

The organization of the new Handbook lends itself to the sort of review I have outlined. Volume I explicates leading social-psychological theories, many of which underlie the various subfields of political science. Volume II deals with socio-psychological methodologies, many of which we have borrowed from social psychologists. Volumes III to V deal with the major substantive fields and findings of social psychology, many of which overlap the interests of political scientists. Therefore, I shall first discuss theory, then methodology, and finally, the substance of social psychology.

Since there is no one "best way" to evaluate a project as massive as the Handbook, my review strategy will be to extract as many examples of the Handbook's uses as I can. I believe this to be the most fruitful way for political scientists and their graduate students to employ the Handbook in research and teaching.

*The Handbook of Social Psychology, 2nd edition, eds., Gardner Lindzey and Elliot Aronson (Reading, Mass.: Addison-Wesley, 1968-69, 5 volumes. Price: \$16.95 each volume.)

Theory in Social Psychology

The current hierarchy of social-psychological theories is fairly accurately reflected by the relative quality of the articles contained in Volume I of the *Handbook*. Specifically, the best chapters in Volume One are those on cognitive theories, reflecting perhaps the dominance of these theories over those of the S-R learning theorists and of the Freudians.

Zajonc's chapter on "Cognitive Theories in Social Psychology" and Sarbin and Allen's piece on "Role Theory" are the pick of the lot in Volume I. These two chapters well display the strengths of cognitive theory, of which perhaps the greatest is its middle-of-the-road stance between the environmentalism of classical learning theory and the residual instinctualism of the Freudians. Cognitive theorists have managed to turn the tension between person and context to their own theoretical advantage.

But there are two other advantages of cognitive theory which also stand out. First, as Zajonc shows, the cognitive approach has generated interesting, nonobvious propositions about recognizable people. Second, cognitive theorists have invented ingenious ways of subjecting their propositions to test. The result has been a closer blend of theory and data than that which has been achieved by either the learning theorists or the Freudians.

For comparison, consider the Freudian approach. At least as it is described by Hall and Lindzey,³ psychoanalytic theory remains far too closely bound to the interplay between ego, superego, and id as they vie for supremacy within the mental economy. Hall and Lindzey's exposition is depressingly archaic and quite irrelevant to contemporary research in social psychology. And, of course, there is an additional issue of evidence, which constitutes a second basic problem with Freudian theory. As the Hall and Lindzey chapter suggests, psychoanalytic theorists have generally been unsympathetic to theory testing, a position which, rightly or wrongly, has not stood them in good stead with most social psychologists.4

In fairness, it should be said that there is more to psychoanalytic theory in social psy-

chology than is hinted at by Hall and Lindzev. Their article poorly represents the place of psychoanalytic theorizing in contemporary social science. Only scant attention is paid to ego psychology in its many contemporary forms.⁵ no attention at all is paid to Maslowian and other "third force" theorists,6 and even Erikson's work is underplayed. 7 Strangely, Freud's own applications of psychoanalytic insights to sociopolitical phenomena are also generally overlooked.8 Contemporary research into psychohistory suggests resurgence of Freudian theory in the social sciences.9 Unfortunately, the inquisitive political scientist will find himself wondering how this could have occurred if he relies solely on the Hall and Lindzey discussion for illumination. Indeed, the Hall-Lindzey chapter is in some respects an illustration of how much psychoanalytic theory has had to overcome in order to attain its newly respectable position.

The problems of classical learning theory are very different from those of Freudian analysis. Unlike the Freudians, the early learning theorists positively reveled in applying their hypotheses to experimental test. But, again unlike the Freudians, the inventors of S-R theory found it difficult to generate ideas that would move their paradigm away from simple dyadic relationships and into the heart of group phenomena. Berger and Lambert's chapter represents a brave attempt to take S-R theory beyond the dyad. ¹⁰ Despite their attempts, however, they ignore much that is politically interesting in S-R theory.

We can remedy this deficiency, however. Let us briefly consider the following findings from

⁵One of the most influential forms is that outlined by Jane Loevinger, "The Meaning and Measurement of Ego Development," *American Psychologist*, 21 (1966), 195–206.

⁶For influential applications of Maslowian theory to political science see Jeanne M. Knutson, *The Human Basis of the Polity* (Chicago: Aldine-Atherton, 1972); and Stanley A. Renshon, *Psychological Needs and Political Behavior: A Theory of Personality and Political Efficacy* (New York: The Free Press, 1974).

⁷Erik Erikson's seminal work is, of course, *Childhood and Society* (New York: Norton, 1958).

⁸For a good treatment of this aspect of Freud's work, see Paul Roazen, Freud: Political and Social Thought (New York: Knopf, 1968).

⁹For a stimulating recent application, see Michael Paul Rogin, Fathers and Children: Andrew Jackson and the Subjugation of the American Indian (New York: Knopf, 1975); a useful overview is Robert Jay Lifton, ed. Explorations in Psychohistory: The Wellfleet Papers (New York: Simon and Schuster, 1975).

10Seymour M. Berger and William W. Lambert, "Stimulus-Response Theory in Contemporary Social Psychology," I, 81-178.

¹Robert B. Zajonc, "Cognitive Theories in Social Psychology," I, 320–411. All references to articles in the new *Handbook* will refer to volume and page number, unless special circumstances arise.

²Theodore R. Sarbin and Vernon L. Allen, "Role Theory," I, 488-567.

³Calvin S. Hall and Gardner Lindzey, "The Relevance of Freudian Psychology and Related Viewpoints for the Social Sciences," I, 245-319.

⁴But, as Kline's recent study suggests, psychoanalytic theory is actually not as resistant to verification as one might assume. See Paul Kline, *Fact and Fantasy in Freudian Theory* (London: Methuen, 1972).

the Berger-Lambert chapter. First, rewards are usually more effective than punishments as a means of inducing learning. 11 (An explanation for this phenomenon may be that punishment creates anxiety, and anxiety, in its turn, disorients behavior.)¹² Second, intermittent rewards are usually more effective than unfailing rewards.13 Third, behavior that has been learned through mixtures of reward and frustrations (in the form of delays) resists eradication more stubbornly than behavior learned in other ways. 14 To summarize colloquially: the carrot is more powerful than the stick; a few carrots are better than a great many; difficulties in obtaining carrots make people value them highly.

These findings have particular relevance to the study of political leadership. For example, leaders may be unable to secure compliance and support indefinitely because they cannot apply these principles consistently. Especially is it difficult for leaders to hit upon the optimal blend of rewards and punishments in securing compliance. Temperament alone may bias a leader in favor of either indulgences or deprivations. Berger and Lambert are unable to extend their findings to these sorts of questions, partly because they are still victimized by the dyadic strictures of S-R theory. Yet, inadvertently, they provide us with the rudiments of a useful political model.

That Berger and Lambert may have unintentionally sold S-R theory short becomes more evident when we realize that, in recent years, S-R theory – like Freudianism – has staged something of a comeback. Exchange theory in political economy and sociology, 15 Skinner's applications of learning theory to politics, 16 behavior modification within psychology itself 17 – all testify to the vigor of the S-R tradition. Berger and Lambert's treatment gives little presentiment of these recent developments. Their discussion remains locked for the

most part within the confines of the dyad, confines which S-R theory in its current resurgence has finally begun transcending.

These considerations should clarify the basis of cognitive theory's current preeminence over S-R theory and Freudianism in social psychology. Cognitive theory has succeeded because it has managed to combine the creativity of the Freudians with the experimental rigor of the S-R theorists, thus producing nonobvious, testable propositions about recognizable social actors. No other theoretical tradition in social psychology can claim as much. Yet who would deny the suggestiveness of such propositions as "... the more the individual undertakes unrewarding tasks the greater will be his reluctance to give such tasks up"?18 And, further, who can dispute the richness of the experimental tradition that runs from Acsh's elegant manipulations of social judgments to Schachter's more formidable elaborations of attribution theory.

Zajonc's chapter on cognitive theories in social psychology does full justice to this now-dominant tradition. His review bristles with findings and propositions that many political scientists could find rewarding. Moreover, he is careful to indicate — perhaps too briefly — that cognitive theories include more than just cognitive dynamics. ¹⁹ Thus he at least leaves the door open to Piaget, as well as to the usual spokesmen of cognitive theory. I might have chosen many different parts of Zajonc's chapter to illustrate its utility for the political scientist, but let me concentrate on three findings.

First, Zajonc demonstrates that the cognition of influence structures is both ubiquitous and biased toward closure. We attempt to locate transitivity and asymmetry in influence structures, and when we confront ambiguous influence formations — such as families — we *impose* leader-follower definitions on them.²⁰ Surely it is not accidental that we always arrange silhouettes of family members with fathers at the extreme left of the group, where they dominate the family scene.²¹ In short, we import cognitive biases involving power into many different situations. Political scientists interested in the vicissitudes of power should at least be aware of these cognitive propensities.

¹¹*Ibid.*, pp. 88–89.

¹²Ibid., p. 89.

¹³*Ibid.*, p. 91.

¹⁴Ibid., p. 95.

¹⁵For an attempt at synthesis, see Sidney Waldman, Foundations of Political Action: An Exchange Theory of Politics (Boston: Little, Brown, 1972).

¹⁶B. F. Skinner, Beyond Freedom and Dignity (New York: Knopf, 1971). See the critique of Skinner by Meredith Watts, "B. F. Skinner and the Technological Control of Social Behavior," American Political Science Review, 69 (March, 1975), 214-227; Skinner responds in the same issue, pp. 228-229.

¹⁷The major impetus here was provided by Albert Bandura, *Principles of Behavior Modification* (New York: Holt, Rinehart, and Winston, 1969).

¹⁸This is the sort of counterintuitive proposition that runs through much work in cognitive dissonance. For an application of the proposition, see Leon Festinger, Henry Riecken, and Stanley Schachter, When Prophecy Fails (Minneapolis: University of Minnesota Press, 1956).

¹⁹Zajonc, I, 332–335.

²⁰Ibid., p. 336.

²¹*Ibid.*, p. 337.

But there is a second cognitive bias that should interest the political scientist. According to Zajonc, studies based on Heider's balance theory concur in concluding that people find it easier to balance positive than negative relationships. Relationships of hostility, repulsion, and suspicion apparently create cognitive disruptions which prevent the development of balanced structures. Could it be that the cognitive rules governing political hostility are different from those governing agreement? If the "rational man" maps political cooperation and political hostility differently, can he be said to be comparably "rational" in both cases?

Finally, as Zajonc notes, the theory of cognitive dissonance yields interesting findings about disconfirmed expectancies. People who feel that they have been rewarded beyond their just deserts apparently strive extra-hard in order to match their achievements to their rewards.²³ Findings of this sort have relevance not only to theories of political ambition and career pursuit, but also to theories of justice. Perhaps reciprocity and fairness are intrinsically attractive to people, leading them toward "just" distributions of rewards. If so, we need not discount conscience as a guide to political action.

Yet cognitive theory is limited in several important ways. For one thing, the cognitivist's experiments are sometimes excessively sophisticated and artificial. This criticism extends particularly to experiments in the dissonance tradition.²⁴ Second, the dynamics of cognition remain every bit as much a matter of inference and assumption as is the psychoanalytic dance between id, ego, and superego. Dissonance, for example, is inferred from its presumed effects, not from direct measurement. Third, cognitive theory, like S-R theory and most varieties of Freudianism, essentially hypothesizes a tension-reduction system of behavior. There is little room in such theories for genuine innovativeness or cognitive restructuring of the political sort described by, among others, Peter Sperlich.²⁵ Fourth, and finally, cognitive theory is usually a bit uncertain about the psychological generation of social norms.

Indeed, a realistic account of the way social roles emerge is a problem for most of the

theories reviewed in Volume I of the Handbook. Role theory, as described by Sarbin and Allen,26 perhaps comes closest to capturing the intricacies of person-environment interaction in the real world. But, role theory is still preoccupied with descriptive typologies and role dimensions.²⁷ True, role norms have been generated under some experimental conditions, but such experiments usually exaggerate the flexibility of the environment, a fact which Patrick Heine implies when he notes, "... to conform or not to conform is scarcely ever a question in real life, but a condition of it. Thus, whether we do or do not ally ourselves with the majority in any given situation illuminates neither character nor conformity generally."28 Perhaps a judicious blending of symbolic interactionism, Piagetian psychology, and attribution theory could provide a fresh viewpoint on the problem of how role norms are generated in the real world. But this task represents a continuing problem for social psychology.

Methods in Social Psychology

Volume II of the *Handbook* cannot help but impress the reader with the variety of social-psychological methodologies. Interestingly, it is the chapters on techniques little used by political scientists that are most engaging. The articles on techniques that already form part of the political scientist's armory seem dull and stolid by comparison. Indeed, in at least a couple of cases, these latter chapters are sadly incomplete and outdated.

Consider, for example, Mosteller and Tukey's piece on data analysis.²⁹ The authors concentrate on some extraordinarily arcane and complex procedures for establishing the reliability of sample means. The techniques described (including binomial probability paper, Bayes Theorem, the "jack knife") seem far too complex for even statistically advanced political scientists;³⁰ worse, Mosteller and Tukey never really convince us that it is necessary to adopt the techniques they de-

²²Ibid., p. 351.

²³Ibid., p. 378.

²⁴The original criticism was made by N. P. Chapanis and A. Chapanis, "Cognitive Dissonance: Five Years Later," *Psychological Bulletin*, 61 (1964), 1-22.

²⁵Peter Sperlich, Conflict and Harmony in Human Affairs (Chicago: Rand-McNally, 1971).

²⁶Sarbin and Allen, "Role Theory," I, 488-568.

²⁷This is indicated by the large proportion of the Sarbin and Allen chapter given over to these topics and the small proportion devoted to actual propositions about role behavior in the real world.

²⁸Patricke J. Heine, *Personality in Social Theory* (Chicago: Aldine, 1971), 33.

²⁹Frederick Mosteller and John W. Tukey, "Data Analysis, Including Statistics," II, 80-203.

³⁰I have found these techniques used specifically by just one political scientist. See Edward R. Tufte, "Determinants of the Outcomes of Midterm Congressional Elections," *American Political Science Review*, 69 (September, 1975), 812–826, 819 n.

scribe. At the same time, techniques that are at the heart of contemporary political science linear regression, causal modeling, factor analysis, smallest space analysis - simply do not appear. A clue to the emptiness of the chapter is the fact that the authors' examples are as arcane as their techniques. Mosteller and Tukey's techniques may be useful in helping researchers decide whether Hamilton or Madison wrote disputed parts of the Federalist papers, but neither political scientists nor social psychologists usually pursue this sort of problem in their research. Political scientists interested in a relevant and comprehensive discussion of contemporary data manipulation had best look elsewhere.

Other discussions of methodologies will be more useful for political scientists. For example, William Scott's piece intelligently advocates a multimethod approach to attitude measurement as a means of coping with problems of reliability and validity.³¹ Scott also does a good job of exposing the assumptions underlying cumulative versus additive scaling techniques. But even so there are problems. Scott has little to say about the relationship of attitudes to behavior, even though contemporary research demonstrates convincingly that many attitudinal constructs are heavily domain-specific behaviorally.32 In addition, Scott limits his discussion of response biases to extremity sets, acquiescence sets, and "faking good" sets. But subject sensitivity to attitude measures these days includes a good deal more than these biases (e.g., racial, ethnic, and class cues). Finally, Scott gives us little reason to foresee the emergence of the "nonattitude" debate that is currently dividing political scientists.33 In sum, Scott's discussion reflects a perhaps overly optimistic view of attitude measures in social science.34

A similar judgment can be entered about

Cannell and Kahn's treatment of interviewing.³⁵ Again the chapter as a whole is quite straightforward and clear, with many helpful hints thrown in along the way. Especially useful is Cannel and Kahn's review of studies which focus on memory failures in the interview situation.³⁶ Not surprisingly, the more threatening a bit of information is to the respondent, the less accurate is his report. Political scientists often are forced to guess about the accuracy of their respondents' memories; they need not do so quite so blindly if they read Cannell and Kahn.

But again Panglossianism looms. Cannell and Kahn blithely counsel the would-be interviewer to teach the respondent his "role" and to "motivate" him to take part in the interview.³⁷ One can only wonder if respondents who require such urgings contribute much to social research in the long run. Surely tactics of the sort advocated by Cannell and Kahn must inflate the large amounts of error commonly found in interview studies. Social scientists who condemn the "artificiality" of the psychological experiment might ask themselves about the "realism" of the interview techniques advocated by Cannell and Kahn.

Among the outright omissions in these chapters is any discussion of how one identifies populations for surveys, for interviewing, or for attitude measurement. Nor is the logic of sampling described. It is almost as if these subjects — each of which occupies the margins of data analysis, interviewing, and attitude measurement — fell between the cracks.

Finally, significant lacunae appear in another area of social-psychological methodology familiar to political scientists - cross-cultural research. Because John Whiting comes at this topic very much from the perspective of a social anthropologist, most of his examples will have little relevance for the comparativist in political science.38 Still, the chapter is redeemed by Whitings's brilliant discussion of one of the chief theoretical difficulties underlying all comparative research - namely, the problem of inferring whether common processes in two cultural units are the result of diffusion from one culture to the other or the result of the sharing of a common historical cultural source. This is Galton's problem, first outlined in

³¹William A. Scott, "Attitude Measurement," II, 204-356.

³²For a review, see Steven Jay Gross and C. Michael Niman, "Attitude-Behavior Consistency: A Review," Public Opinion Quarterly, 39, Fall, 1975, 358-369.

³³See John C. Pierce and Douglas Rose, "Nonattitudes and American Public Opinion," *American Political Science Review*, 68 (June, 1974), 626–649, with subsequent comments and rejoinder by Philip Converse and Pierce and Rose, *Ibid.*, 650–666.

³⁴For a hard-hitting, if somewhat erratic, critique of social psychology in these terms, see Serge Moscovici, "Society and Theory in Social Psychology," in *The Context of Social Psychology: A Critical Assessment*, ed. Joachim Israel and Henri Tajfel, European Monographs in Social Psychology, 2 (London and New York: Academic Press, 1972).

³⁵Charles F. Cannell and Robert L. Kahn, "Interviewing," II, 526-595.

³⁶Ibid., p. 546-548.

³⁷*Ibid.*, p. 576, ff.

³⁸ John W. M. Whiting, "Methods and Problems in Cross-Cultural Research," II, 693-728.

1889.³⁹ The relevance of this issue in current studies of political diffusion is obvious. For example, does the fact that adjoining American states share legislative innovations indicate structural-functional similarity, diffusion, or the effect of common historical origins? And, if the last of these three alternatives is the case, what is the source culture? Need culture be "real" in the anthropological sense or might it be a common storehouse of memories, perhaps even a common imagined past (such as the myth of a master race)? To my mind no political scientist interested in diffusion has firmly grasped the ramifications of Galton's dilemma.40 Whiting's chapter would be a good place to begin.

The real strength of Volume II, however, lies in its discussion of methodologies which are comparatively unfamiliar to political scientists. Three methods in particular bear comment: experimentation, observational analysis, and measures of social choice and attraction. As we shall see, together these three chapters shed interesting new light on the current state of political science.

The most impressive single methodological contribution to the *Handbook* is Aronson and Carlsmith's piece on experimentation.⁴¹ It is hardly any secret that political scientists have generally shunned experimental formats, even though experimentation is the bedrock of social psychology, and particularly of cognitive theory. The political scientists' arguments against experimentation are too well known to require rehearsal. It is therefore all the more striking for us to consider Aronson and Carlsmith's precise discussion.

Aronson and Carlsmith's major contribution lies in their discussion of several rarely noted advantages of the experimental method. They do not slight the usual focus upon the experimenter's capacity to control and vary stimuli, the advantages of randomly sorting subjects, and the merits of a control group. But they go further. They argue that the apparently damaging artificiality of the experimental situation can actually be turned to good account. All depends on whether the experimenter uses artifice cleverly, thereby denuding the subject of his usual resistances and eliciting spontaneous, natural, but unusually intense be-

havior.⁴² Here Aronson and Carlsmith hit on the most obvious, but least remarked advantage of experimentation: a good experiment engrosses and exposes as real life rarely can. Therefore, a good experimenter can lay bare what may otherwise remain hidden.

It is all very well for the political scientist to dismiss the possibility that the political world can be reduced to credible experimental terms. But this anti-experimental bias ignores three salient factors: First, experimentation is the only method currently available that permits straightforward causal inference in the social sciences. Therefore, unless political science adds experimental methods to its standard repertoire, it will lack the scientific power to which it aspires. Second, experimentation permits careful examination of the conditions, boundaries, and limits of social relationships. This feature gives it a degree of precision unmatched by other methods. Finally, experimentation permits the observation and calibration of actual behavior. "So what," we may ask, but only until we realize that most "behavioral" research in political science is not composed of behavioral observations at all. The bulk of such research either relies upon verbal reports (in sample surveys, for sample) or upon indicators (roll-call voting, for example).

Yet experimentation rarely permits the observation of behavior in the real world. Careful observation outside an experimental format may appear hopelessly beyond the social scientist's grasp; such pessimism, however, is not justified. Weick's chapter on "Systematic Observational Methods" describes a number of standardized coding systems for the description of social interaction.43 The multiplicity of such systems will perhaps come as a surprise to political scientists, most of whom believe that the Bales Interaction Recorder marks the high point in observational techniques. But there now exist standard methods for recording not only individual data, such as body movements, facial expressions, and language quirks, but also much subtle phenomena of group interaction. It is true that many political decisions are inaccessible to the observer, but at least some decision making takes place in public forums. Inferences about power and influence under these conditions underlie many theories in political science. Most measures of power which have been applied to these situations - indices developed by game theorists or legislative ana-

³⁹Ibid., p. 703.

⁴⁰But see the at least implicit realization of the problem in Virginia Gray, "Innovation in the States: A Diffusion Study," American Political Science Review, 67 (December, 1973), 1174–1185, at 1180.

⁴¹Elliot Aronson and J. Merrill Carlsmith, "Experimentation in Social Psychology," II, 1–79.

⁴²*Ibid.*, pp. 43, 56.

⁴³Karl E. Weick, "Systematic Observational Methods," II, 357-451.

⁴⁴*Ibid.*, pp. 373-376.

lysts, for example — rest upon untested assumptions about how participants in decisions interpret their own behavior as well as the behavior of others. Observational techniques of the sort described by Weick can be used to flesh out these assumptions. Close observation of how people react to each other when power and influence are being exercised could add a subjective side to our more formal measures of power.

But shouldn't we worry about the validity of such observational measurés? Aren't they unbearably intrusive? Weick suggests ways by which this problem can be managed. 44 More important, he makes us realize that observations of behaviors which actors themselves consider consequential promise to be at least as valid as interpretations of behaviors, such as responses to survey instruments, which have little real payoff for actors.

Lindzey and Byrne's comprehensive discussion of sociometric measures provides a filip to these considerations. The advantage of sociometric techniques is that they permit the mapping of a set of social relationships as seen from the perspective of participants themselves, and not from the perspective of an outsider. Surely it would be useful for the political scientist to have such a map at his or her disposal in attempting to infer the meaning and consequences of a political act in an organized setting. Unfortunately, for most political scientists sociometry is terra incognita. The Lindzey-Byrne piece is a useful corrective.

The common — and refreshing — element of these three chapters is that they converge on the microanalytic side of social behavior. For ours is a science that has been preoccupied with the macro side of behavior — not its microcosmic variations and meanings. We skim the surface of social behavior; we rarely look beneath. Of course, it is hard to produce a theoretically satisfying match between the micro and the macro. But that isn't really our problem. The fact is that we have yet to describe the micro sufficiently well even to grasp the contours of the match problem. Our immediate task might be to remedy this situation with the help of the social psychologist.

The Substance of Social Psychology

Volumes III and IV of the *Handbook* treat the major substantive fields of social psychology, and will be of greater interest to political scientists than will Volume V (describing such applied areas of social psychology as mental health, religion, etc.). Focusing on Volumes III and IV, we see again the appeal of social psychological ideas that are comparatively unfamiliar to political scientists. This is true not only because familiarity blunts our response, but also because several of the articles on subjects familiar to political scientists are flawed by incompleteness, by normative bias, or by a lack of attention to important structural and institutional influences on behavior.

Consider, for example, the articles on socialization (by Zigler and Child), 46 on national character (by Inkeles and Levinson), 47 and on leadership (by Cecil Gibb). 48 Each of these three literatures has found its way into political science — socialization by way of research into political learning, national character by way of comparative politics, and leadership by way of research into political recruitment, decision making, and empirical democratic theory. Yet each of these chapters is flawed by at least one of the problems mentioned above.

Zigler and Child's piece suffers mainly from incompleteness and misplaced emphasis. Heavily devoted to a regurgitation of the voluminous literature on child-rearing and character structure, it virtually ignores important topics like extrafamilial socialization and post-childhood learning. The approach is anomalous, not least because the child-rearing literature itself is so inconclusive. ⁴⁹ The fact is that very few reliable or interesting conclusions can be drawn about the effects of specific child-rearing techniques. Therefore, why Zigler and Child should have pursued this line so singlemindedly is something of a mystery.

By contrast, Inkeles and Levinson's consideration of national character suffers from a failure to confront structural problems that command the political scientist's concern. The problem revolves around the authors' definition of national character as "relatively enduring personality characteristics and patterns that are modal among the adult members of the society." 50 The authors adopt this deliberately narrow definition in order to avoid imputations about the links (if any) between national

⁴⁵Gardner Lindzey and Donn Byrne, "Measurement of Social Choice and Interpersonal Attractiveness," II, 452–525.

 $^{^{46}}$ Edward Zigler and Irvin L. Child, "Socialization," III, 450 – 589 .

⁴⁷Alex Inkeles and Daniel J. Levinson, "National Character: Modal Personality and Socio-Cultural Systems," IV, 418-506.

⁴⁸Cecil A. Gibb, "Leadership," IV, 205-282.

 $^{^{49}}$ Zigler and Child virtually admit this. See pp. 508, 549, 555.

⁵⁰Inkeles and Levinson, IV, 428.

character and specific political institutions. Understandably, they wish to make such linkages a matter of empirical investigation rather than definitional flat. The approach thus avoids the pitfalls of the early national character literature, which often asserted rather glibly that a particular sort of "modal personality" was "functional" for a particular set of institutions.51 So far, so good. The problem is that, once having made this necessary distinction, Inkeles and Levinson never really get around to the relationship between national character and political institutions. Instead, they confine themselves to a discussion of recent methods for uncovering modal personality. And, occasionally, they fall back into the morass from which they have attempted to extricate themselves. Their conclusion that traditional Chinese culture generated "ideal congruence" between personality and institutions 52 does little to explain the triumph of Maoism, which, in many respects, contradicted such "congruent" traditional values as filial devotion, ancestor worship, and relativistic morality. Either the congruence of which Inkeles and Levinson speak was imaginary or else modal personality is more labile than the authors would care to admit.

Finally, Gibb's discussion of leadership is flawed by normative bias. Gibb argues that "the term leadership applies only when this [influencel is voluntarily accepted or when it is in a 'shared direction.' The relation between master and slave, teacher and pupil, and frequently that between officer and men is characterized by a type of unidirectional influence which few people would want to call leadership."53 While there is obviously warrant for distinguishing empirically between positional and unidirectional components of leadership on the one hand and reciprocal and affectual components on the other, we would not want to rule the former two elements out by definition. Worse, Gibb's view encourages us to think that leadership is always consensual and legitimate. But this is not so. Often leaders find themselves forced to combat dissension harshly. Moreover, Gibb's characterization deemphasizes the dynamism of the leadership process. After all, if leadership is always voluntarily accepted, it is hard to know why leaders ever face real challenges at all, much less actually fall from power.

Ultimately, however, Volumes III and IV of the *Handbook* can best be used not by focusing on single chapters, but by extracting bits and pieces from several articles in order to shed new light on processes of interest to political scientists. Let me show how this strategy can apply to the subjects of political reasoning, political compliance, and political decision making.

The Study of Political Reasoning. For a long time the study of political reasoning was dominated by descriptions of the limited amounts of political information, the cognitive and affective inflexibility, and the lack of settled political ideologies which allegedly characterized the mass public. This research tack was perhaps epitomized by the syndrome of working class authoritarianism, explicated in the 1950s. During this period political man – as cognizer – cut a sorry figure, indeed. 54

Unfortunately, scarcely anyone noticed that these conclusions rested upon survey techniques which rarely probed the reasoning process deeply and upon theory which rarely social-psychological variables. Of course, recent research in mass political behavior has provided us with somewhat more optimistic assessments of man's political reasoning abilities.55 But this research too is comparatively uninformed by social-psychological insights. Our debates on political reasoning, therefore, display a certain incompleteness. And the problem is only compounded by discussions of "rationality" - a concept that seems almost impossible to handle from the social-psychological point of view.

Interestingly, research in social psychology as far back as the 1950s provides us with a far more complex view of reasoning than that which has informed the work of most political scientists. For one thing, we seem consistently to have overstated the importance of personality appeals, cultural stereotypes, and source credibility in information processing. The Handbook demonstrates that people can do a better job of evaluating political arguments on their merits than many of us have previously assumed. An example comes from the work of

⁵¹For a recent criticism of this view, with specific reference to colonial America, see James A. Henretta, "The Morphology of New England Society in the Colonial Period," in *The Family in History*, ed. Theodore Rabb and Robert Rotberg (Cambridge: MIT Press, 1971), 191–211.

⁵²Inkeles and Levinson, IV, 481-482.

⁵³Gibb, IV, 212-213.

⁵⁴For a useful recent discussion, see Nevitt Sanford, "Authoritarian Personality in Contemporary Perspective," in *Handbook of Political Psychology*, ed. Jeanne M. Knutson (San Francisco: Jossey-Bass, 1973), 139-170.

⁵⁵Perhaps the best single source is Norman Nie, with Kristi Anderson, "Mass Political Belief Systems Revisited: Political Change and Attitude Structure," Journal of Politics, 36, (August, 1974), 540-591.

Rokeach and Triandis, who discovered that when blacks and whites find themselves together, whites are more willing to associate with blacks who share their views on segregation than with other whites whose views on segregation they oppose. ⁵⁶ Now, of course, blacks and whites in the real world are rarely thrown together in quite the way Rokeach and Triandis arrange; yet, the Rokeach-Triandis research suggests that ideological communities may have the capacity to challenge ascriptive communities. The power of ideas should not be underestimated.

Of course, merely demonstrating the appeal of ideas per se tells us nothing about the way ideas form, nor about how ideas are connected to each other. Again, however, social-psychological research questions conventional wisdom among political scientists. For one thing, there is no convincing evidence that selective perception dominates the communication process; on the contrary, as Sears and Freedman pointed out back in 1965, people are usually willing to seek out novel political arguments.⁵⁷ Even cognitive dissonance does not severely restrict flexible information seeking, and anxiety usually seen as an inhibitor of communication - can occasionally open up the informationseeking process.⁵⁸ Thus, there appears to be both scope and opportunity for political intelligence to operate.

Part of our misapprehension in these respects seems to spring from an assumption that logical and emotive appeals represent poles on a single continuum. Such an assumption is obviously convenient in allowing us to label people as "rational" or "irrational." Unfortunately, however, the continuum itself appears not to exist. According to McGuire, "... as regards psychological judgment, logical and emotional are not at opposite ends of a single continuum but tend rather to be separate ... dimensions." ⁵⁹ If this is so, political judgment would appear to be far more complex than most political scientists have so far appreciated.

Nor are people easily manipulated by the sequence in which they receive information. Instead, the literature shows that people average out positive and negative elements in arguments, thus resisting the distortion of halo effects. When push comes to shove, people stick

stubbornly to the overall weight of the evidence.⁶⁰

Finally, work in the areas of conceptual development and socialization reveals a normal growth process of synthetic capacity operating throughout the life span. Piaget's research in this regard is too well known to bear describing.61 Less well known, though perhaps equally promising, is the work of Harvey, Hunt, and Schroeder, who have outlined four developmental stages, each of which includes both affective and cognitive elements. Each stage is characterized by a distinctive conceptual and personality style, ranging from intolerance of emotional and cognitive ambiguity in the early stages to a balance between affiliative and independent tendencies in Stage IV. Although people in the early stages seem to be vulnerable to external manipulation, those at levels III and IV probe autonomously for information, while resisting coercive or emotive appeals.62 Research corroborating the Harvey, Hunt, and Schroeder scheme now exists in several areas.63 Could it be that the absence of well-integrated political views, not their presence, constitutes a sort of developmental aberration?

In sum, social-psychological research reveals political thinking to be significantly influenced by the *content* of ideas, to be comparatively free of crippling perceptual biases, to be a complex mix of cognitive and emotive elements, to be informed by a logical weighting system, and to be driven by a need for synthesis and flexibility. Obviously there is ample room for individual variation within this characterization, but the nature of the general model differs considerably from that which has been commonly employed by political scientists.

The Study of Political Compliance. Whatever its relevance on social contract theory and the notion of consent, the liberal theory of politics also posits a willingness on the part of ordinary citizens to be suspicious of and even occasionally hostile toward their leaders. After all, liberal theory originally served as a rationale for challenges to royal authority. The liberal believes that vigilance is an indispensable means of preventing leaders from taking undue advantage of their followers. Moreover, liberal theory also

⁵⁶William J. McGuire, "The Nature of Attitudes and Attitude Change," III, 136-314, 188.

⁵⁷Leonard Berkowitz, "Social Motivation," III, 50-135, 98.

⁵⁸*Ibid.*, pp. 88-89.

⁵⁹McGuire, III, 202.

⁶⁰ Ibid., p. 154; see also the more exhaustive review in Seymour Rosenberg, "Mathematical Models of Social Behavior," I, 179-244, 187-193.

⁶¹The fullest treatment is John Flavell, The Developmental Psychology of Jean Piaget (Princeton: Van Nostrand, 1963).

⁶² Zajonc, I, 334-335.

⁶³ Ibid.

includes the hope that leaders will be representative of their followers; otherwise it is unlikely that followers will feel themselves equal to the task of judging and controlling their leaders, and leaders may well become contemptuous of their followers. Controlled hostility within a framework of representation is a vital part of liberal democratic thought.

How does liberal theory construed in this way fare in the face of social-psychological research? The answer is, "Not too well." If anything definitive can be said, it is that even under very dubious conditions people appear altogether too deferential to authority. What we might call a "presumption of acceptability" displays itself across many different areas of social psychology. A case in point is the fact that people seem willing to endure "unbalanced" cognitive fields in order to remain friendly to each other. Being disagreeable is perhaps more costly psychologically than many people can bear.

Sometimes the presumption of acceptability reaches absurd proportions. The best example, of course, is Milgram's demonstration of the ease with which experiments can elicit counternormative and even proscribed behavior from subjects. 6.5 But there are other examples. As early as 1944 Frank demonstrated that experimental subjects would willingly consume great amounts of soda crackers under the most transparently ridiculous pretexts. More recently, Orne succeeded in getting his respondents to engage in such noxious activities as filling papers with random digits, grasping a dangerous reptile, plunging their hands into acid, and even throwing acid at an assistant.66 It would be easier to dismiss these unpleasant findings as the freakish offspring of artificial experimental situations if it were known that the experimenter had been perceived to be wholly benevolent in these situations. But, alas, anyone familiar with social experiments knows that, if anything, the opposite is the case. My guess is that people are more suspicious of experimenters than they are of most "authorities."

Even this evidence of quiescence would not be wholly damaging to liberal theory were it not for the characteristics of leaders themselves. The problem is that leaders are usually more flexible, less neurotic, and more self-confident than their followers.⁶⁷ Undoubtedly it is partly for these reasons that leaders become leaders in the first place. Hence, leaders begin with a two-fold advantage: they can count on both the docility of their followers, and, their own capacity to take advantage of the situation. It is all very well to argue that self-confidence, flexibility, and healthy psychological functioning conduce to democratic leadership, but this is small consolation.⁶⁸ There is ample opportunity for the processes of leadership to be perverted.

Nor is the presumption of acceptability confined to the laboratory. Deference to leadership is even embodied in language. As Miller and McNeill indicate in their chapter on psycholinguistics, diminutives and familiar nouns almost always signify the legitimate presence of hierarchical relationships. And, as these authors point out, "students make appointments with professors, professors do not make appointments with students..."70

It thus appears that the liberal democratic experiment has much to overcome. Without specific barriers against the bias toward hierarchy and deference, democracies would seem constantly at risk.

How can one square the optimism generated by our discussion of political reasoning with the pessimism of our view of compliance? The answer lies in the fact that forces conducive to compliance are absent from the research on reasoning we have cited. The moral seems to be that, left more or less to themselves, people display an admiral flexibility in political reasoning. When, however, they find themselves stuck in situations where authoritative cues prevail, their discrimination flees. In sum, making ordinary people politically "comfortable" would appear to be a priority item for liberal democratic polities.

The Study of Decision Making. Much political decision making occurs in groups. This is unavoidable in a world of specialized information. Yet, as Irving Janis points out, group decision making has its dangers.⁷¹ Particularly in light of our Vietnam misadventure, it would

⁶⁴*Ibid.*, p. 347.

⁶⁵Barry E. Collins and Bertram H. Raven, "Group Structure: Attraction, Coalitions, Communication, and Power," IV, 102–204, 179. See Milgram's own account in Stanley Milgram, Obedience to Authority (New York: Harper and Row, 1974).

⁶⁶Collins and Raven, IV, 179.

⁶⁷Gibb, IV, 218-228.

⁶⁸A related argument may be found in Paul M. Sniderman, *Personality and Democratic Politics* (Berkeley: University of California Press, 1975), 313-315.

⁶⁹George A. Miller and David McNeill, "Psycholinguistics," III, 666-794, 755-756.

⁷⁰Ibid., p. 757-758.

 $^{^{71}}$ Irving I. Janis, *Victims of Groupthink* (Boston: Houghton-Mifflin, 1972), chap. 1.

appear that both policy makers and political scientists need to know more about the forces that can maximize the strengths of group decision making as well as about the pressures that occasionally turn groups into political monstrosities. Contemporary research in social psychology is useful in this regard. Let me sketch some key factors.

Beginning with George Homans, students of social interaction have revealed over and over again that group members strive to attain a balance between their feelings and the demands of the situation. As a result, people do tend to like those with whom they must interact, and to interact more with those they like.⁷² Indeed, were this process wholly unchecked one could visualize a gradual polarization of groups across society and a hardening of group boundaries.

The picture would not be a pretty one; after all, groups are hard on deviants. Group members seek reassurance from others and demand conformity to decisions once made. Many therefore punish deviants, even where decision rules do not require unanimity and where the deviant in fact possesses the correct answer to a problem.⁷³ Nor are group characteristics easily changed; group culture can persist over several "generations" of members.⁷⁴

The forces which push groups toward consensus and conformity would be less worrisome if it were not for the "risky shift" phenomenon — the frequent tendency of groups to adopt more dangerous and chancy political alternatives than group members individually prefer, when polled outside the group setting. There are no persuasive explanations for this phenomenon. Indeed, it may even have physiological underpinnings. As Shapiro and Crider point out, people in competitive relationships within groups display levels of physiological arousal that are far in excess of those they exhibit when making similar decisions under noncompetitive, extragroup condi-

tions.⁷⁶ Perhaps when the politician plays out his role within group settings his need to gain and hold power may so arouse him physically that he becomes willing to take chances he would otherwise find unacceptable.

But groups can perform more effectively than individuals, especially when group members are complementarily deficient (i.e., when they suffer from different incapacities.)⁷⁷ We also know, however, that group decisions usually take longer than individual decisions.⁷⁸ This and much other readily available information could be of use in formulating new models of group decision making in politics.

Studies of information diffusion and communication nets also provide useful perspectives on group decision making. As Collins and Raven show, the main contrasts are between wheel shaped, circle shaped, and all-channel communication nets, each of which has fairly regular patterns of speed and accuracy in decision making. This literature should at least sensitize the political scientist to the spatial and communication properties of the groups he is investigating. While many political decisions are not made by spatially static groups, some are; and all groups have some regular communication properties. The challenge thus seems how to utilize the social-psychological literature flexibly.

Social Psychology and Its Policy Implications

Volume V of the *Handbook*, which focuses on applications of social psychology to various policy fields, is somewhat less useful than are Volumes III and IV for two major reasons. The first is the variability of fit between social psychology and the policy field in question; the second is the simple passage of time, which takes a particularly heavy tool on areas of policy application. Illustrations will again be helpful.

Consider, for example, Etzioni's piece on the social psychology of international relations. 80 The article admirably treats threat and escalation in international confrontations. Not surprisingly, Etzioni comes down on the side of those who feel that gradual de-escalation

⁷²Berkowitz, III, 94.

 $^{^{73}}$ Harold H. Kelley and John W. Thibaut, "Group Problem Solving," IV, 1-101, 71-72.

⁷⁴Henri Tajfel, "Social and Cultural Factors in Perception," III, 315-394, 356.

⁷⁵Kelley and Thibaut, IV, 78-84. A recent reconsideration of the risky shift phenomenon, with a strong support of its importance, is given by Robert Steven Baron, Gard Royce, and Penny H. Baron in "Group Discussion and the Stingy Shift," *Journal of Personality and Social Psychology*, 30 (October, 1974), 538-545, esp. 538.

⁷⁶David Shpairo and Andrew Crider, "Psychophysiological Approaches in Social Psychology," III, 1-49, 16.

⁷⁷Kelley and Thibaut, IV, 66.

⁷⁸*Ibid.*, pp. 73–74.

⁷⁹Collins and Raven, IV, 137-155.

⁸⁰Amitai Etzioni, "Social-Psychological Aspects of International Relations," V, 538-601.

through unilateral initiatives is feasible and desirable. Yet, despite his best efforts, he does not really succeed in making social psychology relevant to his argument. His piece succeeds not because of but regardless of its social-psychological origins. Here is a case in which the distance between social psychology and the field of application does not seem bridgeable.

Furthermore, a few of the articles are somewhat dated. An article on prejudice and ethnic relations without data on ghetto violence, 81 an article on the mass media without a consideration of the Surgeon-General's Report on Media Violence, 82 an article on education without reference to the Coleman Report 83—these papers lose much of their contemporary relevance. Sears's piece on political behavior also suffers in this respect. 84 It cannot consider recent data on "issue voting" and the rational calculus of voting. Nor does it hint at the increased volume of socialization research which, according to Sears himself in a recent publication, has "increased at a geometric rate"

 $^{81}{\rm John}$ Harding, Harold Proshansky, Bernard Kutner, and Isidor Chein, "Prejudice and Ethnic Relations," V, 1-76.

82 Walter Weiss, "Effects of the Mass Media on Communication," V, 77–195. For the Surgeon-General's report, see U.S. Surgeon General's Scientific Advisory Committee on Television and Social Behavior, Television and Growing Up: The Impact of Media Violence (Washington: Government Printing Office, 1972); recent corroboration of the impact of media violence in a very different setting is contained in Bradley S. Greenberg, "British Children and Televised Violence," Public Opinion Quarterly, 38 (Winter, 1974–75), 531–547.

 83 J. W. Getzels, "A Social Psychology of Education," V, 459-537.

84David O. Sears, "Political Behavior," V, 315-458.

from the time he wrote his Handbook chapter.85

Nevertheless, several of the articles bear reading by political scientists interested in the policy areas addressed. Two in particular deserve mention. The first is Freeman and Giovannoni's careful discussion of the medical, psychological, and social meanings of "mental health."86 The authors discuss all the issues including the political and legal - that surround this difficult concept. They also carefully review the weaknesses and strengths of the medical model of mental illness. In so doing they are able to chart the legitimate as opposed to the repressive uses of the medical characterization. For example, they report no association between a person's symptomatic state and his occupational performance,87 a finding which undercuts any who might like to use psychological diagnoses as rationalizations for essentially social and political purposes, such as the perpetuation of their own arbitrary hiring and firing practices.

Finally, Vroom's discussion of industrial relations provides strong support for worker participation in corporate decision making. Vroom elaborately demonstrates that worker motivation to execute decisions picks up with greater involvement in decision making, and that workers who feel responsible perform with greater efficiency. ⁸⁸ This is the sort of information that is indispensable for those who support power redistribution at the work place — or in the polity, for that matter.

⁸⁵Sears, "Political Socialization," in *Handbook of Political Science*, ed. Fred I. Greenstein and Nelson W. Polsby, II (Reading, Mass: Addison-Wesley, 1975), 93–153, 94.

 $^{86}\mbox{Howard}$ E. Freeman and Jeanne M. Giovannoni, "Social Psychology of Mental Health," V,660-719.

⁸⁷*Ibid.*, p. 665.

88Victor H. Vroom, "Industrial Social Psychology," V, 196-268, see pp. 213, 233.

BOOK REVIEWS

Documents on Political Thought in Modern India, Volume I. Edited by A. Appadorai. (New York: Oxford University Press, 1974. Pp. 547. \$13.75.)

The political thought of a nation is as much a commentary on the social and political context conditioning that thought as it is an exposition of fundamental assumptions and doctrines per se. In this first of two volumes of documents on modern Indian political thought, Professor Appadorai has provided a monumental collection to serve both these ends.

The temporal scope of the volume is a little more than a century from the British assumption of direct rule in 1857 to the death of Nehru in 1964. The 274 essays, articles, tracts, letters, and other selections include many familiar and essential names: Gandhi (150 items); Gokhale; Tilak; Pal; Banerjea; Ranade; Naoroji; Nehru; Bose; and others, but there are also several less familiar writers whose inclusion helps make this a much broader and more comprehensive collection of Indian political thought than any previous attempt.

The topical organization of the volume enhances its usefulness. Instead of focusing upon specific political thinkers or abstract concepts like "liberty" or "equality," Appadorai has classified his selections according to four broad themes. The first two of these ("A Dependent People's Conception of Imperial Rule" and "Self-Governance is Essential for a People") place the political thought in historical context and suggest cross-cultural comparisons with Jefferson, Payne, Marcus Garvey, Frantz Fanon, and others.

Treatment is balanced, with inclusion of materials in favor of British imperial rule as well as those opposing it. A great range of issues is considered — the right of the imperial power to legislate on social matters, the economic impact of British rule, political versus social reforms, and others. Unfortunately, the coverage is apparently limited to the political thought of British India. The nature of the imperial impact upon the indirectly ruled princely states which constituted roughly two-thirds of Indian territory, and the parallel response by rulers and subjects is largely if not entirely neglected.

"Political Methods" is the next major theme, comprising roughly a third of the volume. Constitutional agitation and terrorist methods are discussed only briefly, with much greater emphasis placed on the nonviolent techniques of protest pioneered by M. K. Gandhi. Though Gnadhi's thoughts on nonviolence are compiled elsewhere in less expensive volumes, Appadorai's thematic treatment maximizes the possibility of comparing and contrasting Gandhian thought with its predecessors and competitors.

The final section, "What is a Nation?," includes materials on communalism as well as secular nationalism. Here too, the organization and presentation of the materials points to fundamental political questions and helps to tie modern Indian political thought to that found in other places and periods.

One minor shortcoming of the work is the postponement of biographical notes on the authors until the appearance of the second volume. In general, the format and presentation of the material are of an exceptionally high quality. This volume is recommended to scholars of political thought as well as to South Asian specialists.

WILLIAM L. RICHTER

Kansas State University

Ideology and Participation. By Douglas E. Ashford. (Beverly Hills, Ca.: Sage Publications, 1972. Pp. 300. \$11.00, cloth; \$7.00, paper.)

The concept of ideology has been one of the major building blocks of political science. Douglas Ashford believes, however, that it is time to reexamine the conceptual underpinnings of the relationship of ideas to political behavior. He argues that most studies of ideology take one of two forms. First, existing empirical studies attempt to assess the influence of ideology on behavior. Often, the assumption is made that people perceive different political systems in similar enough terms to justify comparison across cultures. Such studies, Ashford maintains, do not consider the different circumstances and institutional structures in-

volved. An equally distasteful alternative is the assumption that the importance of ideology has waned (the so-called "end of ideology" perspective) as other, more important cleavages have arisen.

Ashford argues that there are a variety of perspectives on ideology which relate to behavior and which have not been fully explored. Consequently, he attempts to show that ideology does account for important variance at the individual and institutional levels, using a review of many studies and brief analyses of the political climates in a wide variety of countries in different geographical areas as a data base. His focus is so broad, however, that at times it appears to be more of a review of the major psychological approaches to the study of political behavior than a study of ideology per se.

The problem becomes clear in the introductory chapter when he searches for a definition of ideology. It becomes a catchall for "ideas" or even "attitudes and opinions" which are distinguished by a programmatic aspect, a worldview framework, and a simplification of reality to enable the individual to cope with the environment. In addition, ideologies have both cognitive and affective components. Within this broad definitional umbrella, he then proceeds to review a wide variety of studies and insights into the workings of institutional norms and the mind sets of individuals.

In subsequent chapters, Ashford focuses on two principal elements: the range of permissible variance' in (1) individual beliefs and (2) institutional norms when investigating the problem on a country-by-country basis (evaluated in a dichotomous fashion as "high" or "low"). This enables him to differentiate between such types as "pragmative modernizers" (who rely on individual initiative to change society) and "intensive modernizers" (who use ideology as the basis of social change).

One of the crucial issues faced is why ideologies arouse such intense reaction among followers. He ranges widely in his search for answers: psychoanalytic constructs; authoritarianism and ethnocentrism; and the institutionalization of dependency relationships as manifested by colonialism. He concludes that emotional insecurity and the inability of people to fulfill their psychological needs make them natural targets for mobilization through emotional appeals. Such hypotheses, however, are still unproven, and further research is required.

A similar analysis is provided for the basis of individual cognition. Among the perspectives reviewed are: need theory, learning theory, the achievment motive, the self-image perspective, and the belief systems approach of Rokeach

and Eysenck. He introduces a third dimension of ideology, the evaluative aspect, but this never receives the attention given to affect and cognition.

In assessing the role of institutional norms, Ashford asserts that there is great variation between ideological doctrine and "guidelines for hierarchical relationships" (p. 169) in most societies. Structural differentiation is introduced along with the problem of newly independent countries whose ideologies promise more than their institutions can deliver. The general psychological principles reviewed seem secondary, however, to the extended scope of the examples cited. "So many factors bear on the formation of a political system that it seems unlikely that any universal explanation is likely to be found" (p. 199).

Finally, the cultural underpinnings of an ideology are explored. In this context, Ashford relies most heavily on the work of cultural anthropologists. Considerable attention is given to smaller configurations or "subcultures," primarily the peasant subculture. The problem of coordinating a national-level ideology with rural beliefs is a problem facing most newly independent nations. The difficulty of making a "working ideology" viable, i.e., "providing content to national values at both the individual and collective levels" (p. 283), is a major problem yet to be faced by political scientists.

In summary, Ashford provides a valuable synthesis of current work on psychological approaches to politics in a variety of cultural settings. He also uses psychological variables to interpret political occurrences, past and present. His lack of precision in defining ideology, however, makes the study more an examination of attitudes and opinions than of ideology. He might have spent more time on the following questions which are crucial to his stated concerns: (1) given the pivotal role of affect in ideological attachment, why not investigate the symbols literature for possible answers (there were only a few fleeting references to it)?; and (2) given his concern with bridging the individual and collective levels, what are the methodological problems involved when dealing with variables that link perception and behavior at both levels?

ROGER W. COBB

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Knowledge and Belief in Politics: The Problem of Ideology. Edited by Robert Benewick, R. N. Berki, and Bhikhu Parekh. (New York: St. Martin's Press, 1973. Pp. 327. \$14.95.)

Since the late eighteenth century, social analysts have focused on two facets of ideology: the philosophical and action components. In the 1790s Destutt de Tracy formulated the term "ideology" to denote a science of ideas derived not from the discredited principles of faith and authority associated with the Catholic Church and the monarchical state but rather from more objective impressions gained from the physical senses. For de Tracy, ideology was essentially a scientific means for perceiving the world and also a guide to political action. During the next century, however, Karl Marx took a somewhat different position. He contrasted scientific socialism with unscientific ideologies based on false consciousness. In his view, ideology constituted both a rationalization for actions taken on nonideological grounds - the search for power, wealth, and status - and a highly subjective blinder which distorts an understanding of the social situation.

The fourteen authors whose essays appear in this volume examine the classic issues of the meaning and relevance of ideology. Originally presented at the University of Hull by professors teaching in Great Britain, these essays fall into two general categories. Six authors deal with the philosophical or more specifically the epistemological dimensions of ideology: How does the analyst come to know and understand the political world? How can we reason about the universe? Can our knowledge be objective and universally valid? In answering these questions, W. G. Runciman, Adrian Cunningham, and Bhikhu Parekh use linguistic analysis to interpret ideology. Since ideological perceptions derive from language, which constitutes a limiting cultural mechanism for making universally valid propositions about the world, ideas must necessarily reflect the social context in which the analyst finds himself. Probably Bhikhu Parekh, one of the editors, best states the theme of this set of papers when he describes the main "problem of ideology" (p. 61). Whereas the Greeks viewed reason in terms of pure ends, most contemporary philosophers believe that rationality dictates the means by which we attain our desires. If reason is thus located in the passions, desires, and interests that is, in a specific historical context - then how can the philosopher attain an objective, accurate, universally valid interpretation of events?

The second set of essays explores the relationships between ideology and political practice. Does ideology function as a guide to political actions; that is, do general ideas shape specific decisions? Or does ideology serve as a

rationalization for actions made on essentially nonideological grounds - specifically, to advance the power, wealth, and status of the group which the ideologue represents? Most of the authors take the latter view. For example, in the most comprehensive analysis of the functions performed by ideology and the possible relevance to political actions, Robert Dowse argues that in Nkrumah's Ghana (1961-1966) socialist ideology served mainly as a smokescreen for the pursuit of status, self-interest, and economic security by the Convention People's Party members, the cocoa traders, and the rural villagers. Although it stressed the need for hard work, discipline, and saving, Nkrumah's socialist ideology did not change behavior. By examining the social structure of Ghana during this period, Dowse suggests some convincing reasons behind the failure of the ideologues to secure a behavioral commitment to socialist ends. Taking a somewhat similar view, Harold Wolpe analyzes the relationship in the Republic of South Africa between the racist ideologies of segregation and apartheid, on the one hand, and the behavior of the state and capitalist economic firms, on the other. Although he undervalues the importance of the rural heritage of the Afrikaners, their Calvinist churches, and their schools and presents no information about class stratification within the white population, Wolpe does make a strong case for linking racial policies to class considerations. According to this Marxist perspective, both the pre-1948 segregationist and the post-1948 apartheid ideologies have served similar functions. By rationalizing the need for cheap African labor and subsistence wages, ideology masks the state's advancement of capitalism.

Although most of these fourteen articles present insightful interpretations of ideology in a variety of social contexts, the volume needs greater intellectual coherence. First, the authors share no common agreement on the meaning of ideology. Indeed, only four authors bother to define the concept. (See pp. 178, 229, 245, 257.) Even they conceive of ideology rather simply as a set of ideas, attitudes, and values which are more or less integrated, explicit, and persuasive. What is the relationship between ideology and belief system? How does a specific ideology differ from the more general belief system? The purpose should not be to formulate or discover the single correct definition of ideology but instead to clarify meanings and thereby establish boundaries for discourse. When belief systems as divergent as social science, pluralism, British Labourism, apartheid, and American utopianism in foreign

policy are all subsumed under the label "ideology," then the concept lacks a clear, precise interpretation.

Second, the themes stated throughout the volume remain implicit, rather than explicit. Since these essays derived from a conference held at the University of Hull, perhaps the editors could have prepared a position paper clarifying the various meanings attributed to ideology and articulating a series of explicit themes to explore at the conference. True, the English have become famous for the science of "muddling through" without any overall comprehensive plan, but the organization of papers around a few central themes would have provided greater theoretical unity.

Finally and most important, the volume needs a general introductory or concluding essay which integrates the fourteen diverse papers. A two-page preface does not suffice. By clarifying the meanings of ideology, showing the relationships among diverse interpretations, and explicating the central themes relevant to ideology, epistemology, and political decision making, an integrating essay would have added greater intellectual coherence to the volume.

CHARLES F. ANDRAIN

San Diego State University

The Morality of Consent. By Alexander M. Bickel. (New Haven, Conn.: Yale University Press, 1975. Pp. vii, 176. \$10.00.)

The Morality of Consent is based on the late Alexander M. Bickel's William C. DeVane lectures delivered at Yale. Like its author's life, the book is both noble and incomplete, yet through it we see more clearly than before who Bickel was and what he believed. Bickel, a law professor at Yale who died in 1974 at the age of 49, combined the roles of historian, lawyer, and social philosopher. The book confirms that the lawyer's role constituted for two reasons the center panel in his professional tryptich.

First, Bickel here describes a model of government based solidly on the legacies of Holmes, Brandeis, and Frankfurter. A respect for process, for the use of reason to discover the limits of reason, and for the precedents of history dominates the model. It eschews moral absolutes. The model is, to use some of Bickel's adjectives, Burkean, Whig, Lincolnian, skeptical, prudential, and complex. He carefully differentiates his perspective from that of Rawls and from modern liberalism, which he equates with the belief that we can and should translate moral imperatives directly into public policy. Bickel's elaboration of this model,

derived from our legal traditions, is the book's great strength.

This great strength, the reason why all students of democratic theory and American politics as well as of the legal process should read the book, arises not from originality as much as from the tremendous intellectual importance of the basic assertion: Moral absolutes have no place in public policy, and Dred Scott decisions and Vietnam wars happen when substantive morality intrudes. For public purposes we possess only Burke's "computing principle" by which policy accommodates the relative weights of tentative values and beliefs. Bickel writes: "We had better recognize how much is human activity a random confusion and that there is no final validity to be claimed for our truths" (p. 77). "There are no absolutes that a complex society can live with in its law" (p. 88). We have nothing lexically prior to society itself. Bickel illustrates best in a long discussion, in Chapter 2, of the absence in our law of a firm, philosophically grounded doctrine of citizenship. He warmly applauds this absence and prefers middle-level factual analysis of a sort that, for example, bases the eligibility of resident aliens to receive standard welfare benefits not on the abstract nature of citizenship but simply on the fact that we have subjected resident aliens to the draft.

The second reason for perceiving the centrality of law in Bickel's work is that he adopts his epistemology and methodology from law. Bickel orders his world by reasoning logically and by example in support of a clear conclusion and not through the precise formulation of plausible rival hypotheses. Here as always in his writing, Bickel is an advocate. While his model possesses great strength and nobility, his legal method of argument is the source of the book's significant, though hardly fatal, weaknesses, or at least its incompleteness. The book contains some "I get it all but the therefore" problems which will frustrate readers who, perceiving some differences between legal and scientific methods, prefer the latter.

Let me illustrate. Bickel insists that incremental, pluralistic, shortsighted politics most closely approximate his model. Courts must play a limited role in policy because they are removed by several orders of magnitude from a pragmatic political world that succeeds in making tentative, nonprincipled, "computing" decisions yet retains its legitimacy, i.e., our consent. Judicial legitimacy rests ultimately on its craftsmanship, its resolution of policy questions on the basis of principles that are fundamental if not absolute. Since such principles are rare, the courts' policy work must therefore be

rare, also. Bickel may be right, but plausible hypothetical alternatives consistent with his model do exist. Much in our judicial structure and common-law tradition, despite the linguistic trappings of certainty, makes the process tentative and changeable. Perhaps all consent depends primarily on the breadth and depth of public values that support the substantive results of government, so that courts may preserve legitimacy through imperfectly reasoned results, e.g., "one man, one vote," that the culture accepts.

I sense that the difficulty in Bickel's position on the judicial restraint question arises because Bickel never fully elaborated his view of the nature of reason itself. The lesson from law that all our assumptions are uncertain and that we must constantly reexamine our conclusions forms the heart of his model. But in his analyses of the Supreme Court's work, Bickel, who loved the skeptical, complex, exhaustively detailed reasoning of law, did not always practice what he preached. He criticizes the Court's First Amendment distinction between speech and conduct because "very little conduct that involves more than one person is possible without speech" (p. 63). He strongly disputes the correctness not of the Court's abortion policy but of the Court's making an abortion policy where "moral philosophy, logic, reason, or the materials of law can give no answer," no justification for its content.

But if Bickel's model correctly describes the nature of reasoning, then he is wrong here, for reasoning can give an answer. Perhaps the speech/conduct distinction is, as a general matter, wise because prosecutions for conduct will depend on more reliable testimony than prosecutions for speech, on observations of physical actions rather than perceptions of what was said. Perhaps the recognition that some arbitrary lines must be drawn in policy is itself a sufficiently reasoned justification for the abortion decision's line-drawing. Perhaps not. The point is that as an advocate, Bickel does not (and probably could not) entirely avoid moralizing about the evils of moralistic analysis.

On balance, however, the virtues here far outweigh the shortcomings. Bickel eliminated some earlier analytical weaknesses, e.g., the simplistic view of the role of electoral majorities in *The Least Dangerous Branch*. The holistically critical view of the Warren Court gives way to criticism of some cases and justices (particularly Justice Black) and praise for others. Students of Bickel himself will gratefully discover a complete bibliography of Bickel's published writings, especially the large

quantity of periodical pieces. More important, they will detect here a shift away from Brandeis, the subject of his first book, and toward Holmes, the subject of one of his last books. Most important the argument itself may take on historical value, for Bickel may have articulated a convincing philosophical center around which the reported current mass movement away from New Deal liberalism can and will revolve. At least in the attempt we are cruelly reminded of the unfairness of his loss.

LIEF H. CARTER

University of Georgia

Measures for Psychological Assessment: A Guide to 3,000 Original Sources and their Applications. By Ki-Taek Chun, Sidney Cobb, and John R. P. French, Jr. (Ann Arbor: Survey Research Center, Institute for Social Research, 1975. Pp. xxiv, 664. \$15.00.)

In the idealized view, empirical research in the social sciences is a continuous, cumulative, collective, and integrated process. However, the editors of Measures for Psychological Assessment present evidence suggesting that in sociology and psychology, at least, the empirical research endeavor is "fragmented, individualistic, discontinuous and wasteful" (p. x). For example, between 1954 and 1965, there were 2,080 different scales and measures used in the four leading sociology journals. Only 2.5 per cent of them were used more than six times, and 72 per cent were used only in one study. In psychology, the figures are equally dismal. A 20 per cent sample of all issues of the fifteen measurement-related psychology journals published between 1960 and 1969 revealed that about 2,500 different scales were in use, but only 3 per cent of them were used more than ten times and 63 per cent were used only once. It is my impression that the percentages for political science would be as bad or worse.

This situation would not be a source of concern if the tiny fraction of scales in repeated use were the highly valid and reliable scales while the little-used scales represented low quality chaff which the winds of empirical and theoretical rigor blew away. But Chun, Cobb, and French found that the most popular scales are no more reliable and valid than a sample of the stillborn scales. They argue that accessibility and ease of use rather than quality were the factors winnowing the popular from the unpopular. To correct this state of affairs, they have published this volume of references to

more than 3,000 measures of psychological assessment.

Because of the wide range of measures included, the thoroughness of the editor's research, and the manner in which the scales are indexed, this compilation should prove very helpful to many different types of political scientists, not just to those interested in political psychology. Although attitudes toward specific political actors and specific political issues are excluded, this volume references scales of authoritarianism, self-esteem, leadership ability and effectiveness, racial attitudes, need for power, alienation, and persuasibility, as well as political conservatism, liberalism, cynicism, interest, radicalism, ideology, and orientation.

These scales were indexed as the result of a very thorough search of the twenty-six-leading psychology and sociology journals in which scales, tests and measurements are published or used in research. Therefore, after referring to this volume (which covers material from 1960 to 1970), a political scientist in search of a properly validated and reliable scale needs only to consult the political and other social science journals published after 1970, before choosing a scale or deciding to create and validate his or her own scale.

The volume is relatively easy to use. There is an author index, a scale topics index, and two lists of sources. A "primary sources" list gives the location where the scale was first published, a summary of available validation information, and an index to subsequent uses of the scale. The second list includes all of the secondary sources, i.e., articles in which the various scales have been used and sometimes interrelated.

JOHN B. MCCONAHAY

Duke University

Prophets and Patrons: The French University and the Emergence of the Social Sciences. By Terry Nichols Clark. (Cambridge, Mass.: Harvard University Press, 1973. Pp. 282. \$12.00.)

There is nothing counterintuitive in the good common sense of Clark's "model": we must examine ideas in their institutional context, and, focusing on France, we must understand how a system of patronage — "clusters" of teachers and scholars under the protection of a Parisian professorial patron — acted critically either to institutionalize or hinder the institutionalization of a school of thought. Reduced to essentials, Clark's theory of "clusters" asserts that in such a highly centralized, hierarchical structure, with limited jobs, it is essential to

control or influence the center and to have the exercise of one's special intellectual talents considered a desideratum of the curriculum. "Prophets" cause a brief stir, but only "patrons" with institutional skills allow their "clusters" to survive. Genius and proper traditional credentials (à la Durkheim) help, but institutional arteriosclerosis (or, indeed, we are reminded from time to time, chance) can chill a nascent "patron-cluster" bloom before its roots are strong enough. In such a system, social science has taken a long time to come into its own.

Such a construct is more a good description of the French educational system than a serviceable model for isolating, let alone weighing, its elements and mechanisms, but Clark fails to tap even its full potential for illumination. He offers no comparison of the "institutionalization" of social science with that of a new natural science (or any other discipline, for that matter). He does not examine patiently, with attention to details, the functioning of "clusters." He does not study the critical debates and processes by means of which sociology was kept out of the lycées for so long. He neither identifies nor analyzes closely the French social-scientific community as it established itself in the universities and research centers after World War II. Instead, with no clear research design, Clark traces (with only cursory attention to contemporaneous intellectual dialogues and with little parallel focus) the fate of diverse groupings of social scientists: three early groups (Comtians, Le Playists, and the anthropologists around Broca); the "social statisticians," who "institutionalized" outside the walls of academe; the "international sociologists" (grouped around René Worms and Jeanne Weill), whose partisans failed to meet "university standards": and the Durkheimians, who penetrated but ultimately could not secure their survival within the structures of the educational system. The work concludes with a rapid survey of "continuities" and "discontinuities" since 1914, asserting (p. 201) that "no clear new alternatives had succeeded the old patterns," despite the dramatic shift of funding towards the social sciences since 1960.

Apart from its nondisconfirmable model, its looseness of categories, its lack of control groups from outside the social sciences, and its research gaps, what is wrong with this as history? Above all, Clark fails to distinguish between "institutionalization" of ideas in the persons of their original carriers and "institutionalization" of ideas by means of intellectual cooptation. Thus, he primarily traces the careers of figures who established societies, écoles,

and journals, but who did not succeed in acquiring university chairs, or new names for university chairs, or adequate curricular changes in terms of well-defined new disciplines. In chapter after chapter, however, we meet professors from the Sorbonne, the Collège de France, and the university system in general who are associating with the societies, *écoles*, and journals of the extramural social scientists. What were they doing there? How did this association alter their perceptions, their assignment of tasks to and evaluation of their students and later potential colleagues? If he had pursued such questions, Clark perhaps could have explained to his readers the real significance of the deus ex machina introduced briefly to account for the richesse of contemporary French social science: the VIème Section of the Ecole Pratique des Hautes Etudes, "Sciences Economiques et Sociales.

In fact, by the 1920s and 1930s, far from the attention of Clark's investigations, young historians such as Marc Bloch, Lucien Febvre, and their disciple, Francois Braudel already were profoundly impressed by the methods and accomplishments of "social science," viewing these as means by which the historians more systematically and cogently could study the past. These scholars created their own interdisciplinary institutions, transformed the practice of what they termed "the human sciences," and made the French educational system safe for social scientific thought. It was indeed this new breed of historians, grouped around the journal Annales, who more than any other figures "institutionalized" social science in France, and they are a group virtually ignored by Prophets and Patrons. The VIème Section, which links sociologists, economists, demographers, human geographers, and historians, has been directed since its founding in 1947 by two "patrons," both historians (Febvre and Braudel), and Clark simply failed to understand the importance of such a configuration. What, in fact, had taken place? The discipline of history, academically well entrenched, became animated and was soon led by scholars convinced that the "analytic" social sciences were vital to "historical synthesis." It was these historians who became the patrons of social scientists just as the wealth was beginning to expand. Intellectually, social science won the minds of French historians, and these historians became the agents of the institutional legitimation of the social scientific disciplines. If Clark had devised a valid plan of research, he would have had, if nothing else, an exciting tale of intellectual and institutional

change to tell .

ALAN CHARLES KORS

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Nietzsche's View of Socrates. By Werner J. Dannhauser. (Ithaca, N.Y.: Cornell University Press, 1974. Pp. 283, \$15.00.)

The subject matter of this book is of central significance for those who study political philosophy. Socrates is the primal figure for that uniquely western activity. More than any other modern thinker, Nietzsche placed Socrates at the center of Western history as the creator of rationalism, and claimed in his own thought to have overcome that rationalism. Therefore, in Nietzsche's view of Socrates we are near the center of thinking about the nature of political philosophy. For somebody from outside the U.S. (such as myself) it is a happiness to find that a professor of government at Cornell should devote his thought to so central a subject for our Western self-understanding.

I am even happier to say that Professor Dannhauser's book is very well done. He has read Nietzsche's writings carefully and comprehensively; he has also read the accounts of Socrates which are given us in Plato, Xenophon and Aristophanes. Because of the political and economic conflicts between the English-speaking and German peoples in the last generations, English-speakers have not paid sufficient attention to modern German philosophers - the greatest of whom is Nietzsche. Now that Nietzsche is becoming powerful in our society (particularly among the young) his influence often comes to us in the form of a quick and one-sided apperception of his teachings. Indeed, Nietzsche's enchanting (if terrible) rhetoric encourages that one-sidedness. Therefore, Dannhauser's care must be highly praised. He has obviously read Nietzsche over many years and knows the extensive and complex corpus intimately. This is a scholarly book in the best sense of that term. The form of this book is to trace carefully Nietzsche's view of Socrates from the early writings, when Nietzsche was taken up with positivism and modern science, to the last writings where his final encounter with Socrates is put before us. Dannhauser wisely leaves to a separate chapter what Nietzsche says by implication about Socrates in "Thus Spoke Zarathustra." He treats with proper caution the endless irony of the ambiguous speeches which largely make up that work.

One great quality of Dannhauser's book is that it is unpretentious. By that I mean that his personality does not get in the way of laying before us clearly and carefully what Nietzsche is saying. The book is concerned with Socrates and Nietzsche, not with himself. This is a pleasure these days, when so many books about politics that pass beyond the simply "objective" seem to be mostly an exposition of the author's personality and problems. If one wants to follow accurately the details of Nietzsche's changing view of Socrates, this book holds the subject together with accurate care. This is what the book is: a lucid exposition of a central (perhaps the central) issue of modern philosophy and therefore of political philosophy.

The present influence of Nietzsche in the U.S. owes much to Professor Walter Kaufmann of Princeton University, because of his extensive translations and his long book about Nietzsche's life and thought. This has been useful in the sense that Kaufmann has helped to free English-speakers from the picture of Nietzsche as a wild poet of nihilism, who politically was the precursor of the horrors of National Socialism. Kaufmann has destroyed amongst us, surely finally, the impression that Nietzsche somehow took part in the tradition of German anti-Semitism. He lays Nietzsche before us as the chief influence upon European existentialism and as a thinker who laid the foundations of what Freud was later to think. In that sense, the influence of Kaufmann's work has been salutory. On the other hand, Kaufmann has presented Nietzsche as if to make the latter's writings open and palatable to the social democratic world of American academia. He has done this by presenting Nietzsche first and foremost as the expositor of what is assumed in modern society, rather than as the thinker who wants to overcome what stands against human nobility in the modern world. Kaufmann treats vaguely what Nietzsche took as his "most abysmal" teaching, "the external recurrence of the identical." In short, Kaufmann does not take Nietzsche altogether seriously.

What makes Dannhauser's book welcome is that he takes Nietzsche's thought at the highest level of seriousness. For example, he confronts the fact that "the eternal recurrence" is claimed by Nietzsche to be at the center of what he teaches, and therefore cannot be put aside as some kind of ecstatic extra. This is to say that although Dannhauser is finally less persuaded by Nietzsche than is Kaufmann, he nevertheless treats Nietzsche as a greater thinker than does Kaufmann. It is indeed because Dannhauser (as he shows himself in his last pages) is a lover of Socrates, that he is able to treat Nietzsche deeply — namely as the most lucid of all teachers of historicism. As Socrates is the

supreme Western figure who taught that historicism can be transcendent, Nietzsche's account of Socrates must be understood, if one is to look at the problem of historicism clearly. As historicism appears to be the fundamental presupposition of the dominant North American social science, Dannhauser's book is important in our present intellectual situation. I mean by "historicism" the teaching that all thought is determined by belonging to a concrete dynamic context. If our contemporary academia is to face its main task, which is to try to understand the present state of our intellectual tradition. this must include the understanding of Nietzsche's writing as it is, in all its greatness, as the work of a thinker who not only assailed Western rationalism, but claimed to have overcome it. Because Dannhauser's book deals with these central issues in Nietzsche's view of Socrates, it takes us to a more philosophic stage of Nietzschean scholarship in the U.S. His work is on the level of the best writing about Nietzsche in Germany and France since 1945.

When one admires a book, it is likely to sound ungrateful to mention extra things one wishes the book had. Nevertheless there are three points I could wish had been developed further:

- (a) Dannhauser does not seem to discuss sufficiently what Nietzsche says about Socrates and Platonism in "Beyond Good and Evil." Dannhauser's chief acknowledgment is to the late Leo Strauss as a teacher. One of Strauss's last published writings was an article on how to read "Beyond Good and Evil." Perhaps it is Dannhauser's respect for that writing which has made him decide to give "Beyond Good and Evil" a less central place than in my opinion it deserves.
- (b) I wish Dannhauser had said more about Heidegger's work, Nietzsche. In my opinion this is the greatest book of commentary on a philosopher which has been produced in our age. Dannhauser refers to it several times, but does not deal with Heidegger's interpretation of Nietzsche at length. Perhaps this would have made his book too complex, and taken away from the fine clarity of his writing. It would have become a book not only on Socrates and Nietzsche, but on Heidegger, Socrates, and Nietzsche. Nevertheless one is aware from this book that Dannhauser is capable of writing about Heidegger's interpretation, and one wishes that he had. After all, Heidegger's historicism is the supreme contemporary understanding of what is implied in the acceptance of that teaching.
- (c) Most important. In the last pages of his book, where he makes clear that he is a

follower of Socrates and not of Nietzsche, Dannhauser writes: Nietzsche's "critique of dogmatisms and systems may hit Hegel, but it misses Socrates and Plato; and many of the attacks on reason, rationality, and rationalism may hit Descartes, but they miss Socrates and Plato" (p. 272). I wish Dannhauser had greatly expanded this passage, and said why this is so. However difficult such questions are, it is to be hoped that Dannhauser will turn to them in subsequent writings. In this book he presents us with plenty of evidence that he is capable of casting light on these extraordinarily complex issues.

GEORGE GRANT

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Marx and Mill: Two Views of Social Conflict and Social Harmony. By Graeme Duncan. (New York: Cambridge University Press, 1974. Pp. 386. \$15.50.)

"Poverty in itself" Hegel tells us, "does not make men into a rabble; a rabble is created only when there is joined to poverty a disposition of mind, an inner indignation against the rich, against society, against the government, etc" (Philosophy of Right, Par. 244 Addition). Professor Duncan compares Marx's and Mill's attitudes to this possible emergence of a "rabble." Marx hoped for a "rabble" which would have transformed itself into a class-conscious proletariat, the bearer of history's meaning; Professor Duncan indicates the large measure of faith underlying this hope, and its somewhat shaky foundations. Mill, on the other hand, feared the rise to power of an unreconstructed "rabble": Professor Duncan indicates the unacknowledged class interests and economic conditioning which set horizons to Mill's timid sincerity.

The long section on Marx, fruit of an impressively thorough reading of the whole corpus, is not only the main but also the best part of the book. Professor Duncan usefully brings out the extent to which certain problems about Marx's thinking and tactics - for example, violence versus peaceful change, revolution versus parliamentary pressure - can be settled only in concrete circumstances and not a priori (pp. 96, 180 ff.). He also stresses how revolution is to come about not through some deus ex machina but through the imminent, contradictory development of capitalism itself (p. 57). But, he argues forcefully, Marx does not prove that capitalism's contradictions are absolutely incapable of some kind of resolution, nor does he show how they will necessarily lead to the envisaged alternative society. His central problem about Marx is how a proletariat, conditioned as Marx asserts by the "muck of ages" (p. 176) and specifically by the muck of capitalism, can transcend its conditioning, and do so, moreover, in the direction of the communist ideal.

The final chapter on Marx equally raises central and thorny difficulties, this time about the quality of life and consciousness after the revolution. On what he himself calls a "rather pessimistic" theory, the author argues that class conflict is simply a crucial case of group conflict, which in turn is but a case of social conflict, and suggests that "inequality, division and conflict are not traceable to capitalism alone" (p. 202). These arguments have particular force in view of his earlier well-founded assertion that, for Marx, property divisions are neither the only nor the synoptic social ill: division of labor also is "a primary determinant of men's actual natures and capabilities" (p. 79). He draws together arguments, and adds some of his own, about why the abolition of class conflict will not necessarily purify all of man's passions, so that violence and domination may persist, may even indeed flourish. Here as elsewhere my main criticism is of Professor Duncan's silences. If he had narrowed his range and deepened his discussion (although he does clearly say that this is not his intention), he would have produced an excellent rather than a good book. For example, while his criticisms of Marx are sound and provocative so far as they go, the reader can hardly fail to be disappointed by a mere few pages in which we are treated to inferences from "the ruthless internal logic of revolutions" (p. 199).

In the shorter section on Mill, Professor Duncan argues against some misconceptions, such as that of Mill as an eclectic: Mill's enterprise of synthesis is shown as more solid and consistent than is often thought. A more important misconception is that of Mill as a liberal individualist par excellence; this image, we are told, "ignores some of his basic themes and concerns" (p. 272). While rejecting those views which would assimilate Mill to Marx, Professor Duncan shows that some elements of Mill's critique, as for example his discussion of cooperative enterprise, are as radical, at least in ideal terms, as much of Marx. But Mill had a fatal ambivalence toward the rabble: circumstances, he feared, had excluded them from the intelligence and objectivity which the ascendant middle classes had taken over from the now decadent aristocracy. The irony of liberal heartsearching is that it is an elite pondering the value of elite leadership; the "rabble" could not

have a fair trial. For all his sincerity, we are told, Mill could not see that what was "objectivity" to an enlightened bourgeois was itself a particular, conditioned ideology. "There is little perception of the interests which underlay its ideas, or of the illusory elements in them" (p. 223). Mill's politics is characterized as combining short-term defense against the potential destructiveness of mob rule with a long-term hope "that men could be liberated from class by reason" (p. 230). The long term was to be pretty long: however desirable universal suffrage may be, "it cannot be either good or practicable now" (quoted p. 231), a theme which ran throughout Mill's works. Professor Duncan himself comes near to Marx's judgment, in another context, on "those hypocritical friends who declare their agreement with the principle, but who are doubtful of its feasibility because the world is not yet ripe for it" (Neue Rheinische Zeitung, 2nd June 1848).

But be that as it may, the concluding chapter on "Marx, Mill and Modern Society" shows that a large part of the predictive honors go to Mill, whose notions on gradual moral and intellectual adaptation explain what Marx missed: the extraordinary persistence of an at times very shaky capitalist edifice. I would, however, have liked these comparisons to be more and deeper, at the expense, say, of the long "Preliminaries" (pp. 1-54). While Marx's historical materialism and Mill's opposed theory, which we might call "historical ideationalism" are neatly characterized and in some points contrasted, the confrontation could usefully have been sharper and more detailed. The intriguing clue of Mill's distinction between "constitutional" and "real" power might well have been set against Marx's notions of how, and why, political regimes lose their foundation. The book's central topic revolves around the Third Thesis on Feuerbach - about the need to educate the educators - yet, while it is mentioned in the section on Marx (p. 175), the discussion of this problem in Mill heroically avoids mentioning it (pp. 250-257). We then slide from the comparison of the two authors to a brief critical account of modern pluralist/ consensual theories of social integration. This discussion is long enough to state some important arguments, but it does not significantly further what is a fairly widespread debate in the political science community.

I must repeat that Professor Duncan disclaims the ambition of making original points about either Marx or Mill (although in the case of Marx he is far less than fair to his own contribution). But if he is to stick rigorously to that intention, we are entitled to ask for more

in the way of comparison and criticism. There are many strands of discussion — on Marx, on Mill, on revolutions, on pluralism and so forth — which are broached but, because of the wide aims of the book, do not get off the ground. I would have preferred a narrower but tauter book, but I found the book as written interesting throughout, and at times challenging and fresh.

JOHN MAGUIRE

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The Modern Law of Treaties. By T. O. Elias. (Dobbs Ferry, N.Y.: Oceana Publications, 1974. Pp. 272. \$19.50.)

The author of the volume under review is Chief Justice of Nigeria and a distinguished specialist in international public law. Because of his academic background (former Professor and Dean of Faculty of Law at the University of Lagos) and professional experience (member and consequently President of the United Nations International Law Commission), he was elected Chairman of the Committee of the Whole Conference on the Law of Treaties in Vienna. The reviewed book is a byproduct of the part Mr. Elias played in the making of the Vienna Convention.

The volume is divided into fifteen chapters dealing with various aspects of both the text and procedures of the Vienna Convention and Conference. The first two chapters entitled "Conclusion of Treaties" define some basic terms as well as the scope of the study. Since the Vienna Convention applies only to the treaties between states, Mr. Elias does not cover either agreements between states and international organizations or between other subjects of international law. Chapter III deals with observance and application of treaties, chapter IV with the effect of treaties upon third states. Chapter V is an account of rules and means of interpretation of treaties, Chapter VI discusses treaties' amendments and modifications. Chapters VII-XI cover various problems connected with termination and suspension of the operation of treaties. The four concluding chapters are devoted to the controversial Jus Cogens (XII), to procedures for judicial settlement, arbitrarion and conciliation (XIII), to consequences of invalidity of treaties on certain grounds (XIV), and to technicalities such as the function of depositaries, and to registration and publication of treaties (XV). At the end of the book, the full text of the Vienna Convention on the Law of Treaties with the Annex, bibliography, table of cases, table of conventions, and the index are reproduced.

The book is a brilliant legalistic treatise of the law of treaties. The author discusses all relevant legal aspects of the recent evolution of contemporary international law in a logically organized manner. His comments on controversial issues (or cases) are well argued. Mr. Elias is impartial in the best tradition of great international lawyers. This is why his study is a valuable contribution to public international law. But the author's strength - i.e., his rigorous legalistic approach to the subject - has caused certain problems. Take, for example, the question of conflicting ideologies for the interpretation of treaties: Mr. Elias rightly suggests that "there is a strong presumption that the terms used in each language version (of a treaty) are deemed to have been intended by the parties to have the same meaning throughout" (p. 86). This logical statement may not necessarily apply to such general terms as sovereignty, aggression, noninterference in internal affairs, etc. even if they are used in one language only. Here one may see the limits of the legal logic for the resolution of conflicts caused by doctrinaire approach to international law. Or to give another example, suppose one uses the formulation, "a treaty is invalid if its conclusion is procured by the threat or use of force in violation of the Charter of the United Nations." An impartial lawyer might suggest that such a formulation would make the Article applicable to member states of the United Nations only, whereas if one used the formulation, "a treaty is invalid if its conclusion is procured by the threat or use of force in violation of the principles of the Charter of the United Nations," it would be of general application today (p. 170-171). There might be still another distinction between the two formulations, however. The Charter of the United Nations is much more concrete than mere principles of the Charter. The Charter itself represents a definite text, while its principles are open to different ideological interpretations. I believe that most textual nuances in international law today have not only legal but also strong ideological implications. So long as this is true, modern international law literature ought to recognize this new aspect and build it into legalistic approaches.

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Utopias of the Classical World. By John Ferguson. (Ithaca, N.Y.: Cornell University Press, 1975. Pp. 228. \$11.50.)

The aim of John Ferguson's Utopias of the

Classical World is to "tell the tale of ancient Utopianism" without the psychological or sociological apparatus found in the works of such writers as Marcuse and Mannheim. Rather, Ferguson is eager to let "readers draw their own conclusions" (pp. 7-8). This aim may be noble in a field where the factual basis for such analyses is severely limited and at best speculative, but in his desire to allow the tale to speak for itself, Professor Ferguson has failed to give direction to the reader who is eager to know what the author means by "utopia" and "utopianism." what is unique (or not unique) about the utopias of antiquity, or even what a modern reader might learn from the study of these utopias of the past. Nowhere in the first hundred pages of this book are we given a definition of utopia any more specific than that it is an "enthusiasm" (p. 7). From the Preface we are led to believe that this could be an enthusiasm for a horseless carriage going twenty miles per hour just as well as an enthusiasm for social or sexual equality. As a result, throughout the book there is no explicit criterion for determining what will be discussed and what omitted. Thus, we have chapters devoted to Antisthenes and to Diogenes, but not even a passing reference to Aristophanes, who wrote the Birds and the Ecclesiazusae. For the most part, though, Ferguson errs on the side of inclusion rather than exclusion and offers a potpourri of visions of the desirable - be it a bountiful Nature whose sheep graze with their wool already dyed (Vergil, Ecologues), or philosopher kings, or the peace of St. Augustine's city of God.

Ferguson does not limit himself to the literary expressions of utopias. "Political theory which ignores the hard facts of material existence is in the last resort sterile, and wishful thinking no substitute for the working out of sound economic structure" (p. 176). His concern with "material reality" results in extensive treatments of the political history of Greece and Rome. Consequently, we find in less than two hundred pages of text a survey of Greek and Roman political history from the Mycenean Age to the fall of Rome in 410 A.D., alongside a survey of Greek and Roman political philosophy from Homer to St. Augustine, including a chapter on Judaic Messianism. Even the exceedingly small print does not allow the author to do justice to all these topics. Ferguson justifies the inclusion of this mass of material by emphasizing the interaction between vision and reality, though it is clear throughout that he is far more interested in the effect of vision on reality than of reality on vision. Both political actors and political

philosophers are evaluated according to their relative success in applying their visions to reality. Zeno's Republic is castigated as an "ideal; he gave no indication of transition from the present to the ideal" (p. 115). A major section of the chapter on Plato is devoted to his relationship with Dion and Dionysius II of Syracuse and to the activities of students at the Academy as advisers to the political leaders of the age. Similarly, Ferguson stresses the visions which may have directed the practical activities of famous political leaders. Thus, he considers whether Alexander was a pragmatist or visionary, though with good judgment he refrains from trying, in the space allotted, to resolve the continuing debate on this point. Thus, Tiberius Gracchus acting under the influence of the Stoic Blossius is distinguished from his brother Gaius who showed "some indifference to the more distant implications of his policies" (p. 154). Thus, Agis IV and Cleomenes of Sparta, influenced by yet another Stoic, are visionaries, perhaps prompted by Stoic universalism, and more important to the history of utopias than Spartacus who, leading the slaves in revolt with no philosophic tutor, is "but a practical man with limited objectives" (p. 157).

This conjoining of the history of political thought with the political history of antiquity does pose for us the problem of how one is to view the utopian thought of such writers as Plato and Aristotle (the latter of whom Ferguson does not consider utopian (p. 80). Must we look at these utopias as only "a vision to guide and reform contemporary politics?" (p. 115; italics added). Does Plato become important because of his questionable attempt to institute a philosopher king in Syracuse, because members of the Academy became political advisers, because many centuries later Plotinus may have proposed actually founding a Platonopolis? Are we to take utopias or visions of the desirable as plans to be implemented? Certainly, the dreams of a bountiful Nature with which the book begins cannot fit into this category. Or must we take the literary and philosophic utopias as attempts to tell us about the nature, potential, and limitations of political society, contemporary or otherwise? Is not the importance of Socrates's vision not what it tells us about the political realities of fifth and fourth century Athens, but its posing for us the perennial problems of philosophy and politics, truth and opinion, justice and injustice? To focus on the practical impact of utopias is to give Lycurgus almost as much space as Plato, Blossius as much as Aristotle, and Alexander as much as Cicero.

Ferguson has tried to do too much in this

book and has failed to clarify his purpose. As a result, although there are some fascinating tidbits of utopian thought from such obscure authors as Euhemerus and Scytobrachion who are unlikely to be known outside a small group of classical scholars as well as interesting descriptions of the occasional relationship between political philosophers and political actors, the author has not confronted the problem of utopianism directly, of its meaning beyond the immediate societal influences, and of its philosophic significance for the ancients and for us.

ARLENE W. SAXONHOUSE

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Against Method: Outline of an Anarchistic Theory of Knowledge. By Paul Feyerabend. (Atlantic Highlands, N.J. and London: Humanities Press, 1975. Pp. 339. \$16.50.)

Paul Feyerabend is a prominent exponent of one variety of what has come to be called "the new philosophy of science." In this book, the first he has published in English, he brings together in revised and augmented form the substance of some of his challenging articles (one with the same title and subtitle as the present work) which originally appeared in various collections of not readily accessible monographic studies, such as the Minnesota Studies in the Philosophy of Science and the Pittsburgh Series in the Philosophy of Science. But as he explains, the present essay was intended to be only the first half of a volume. the other part of which was to have been a rejoinder by the late Imre Lakatos to Feyerabend's attack on, among other things, the version of "rationalism" Lakatos advocated. The latter's regrettably premature death made it impossible to realize this plan. The book as it stands is nevertheless welcome, if only because it makes conveniently available many of Feyerabend's characteristic ideas on the philosophy of science, the grounds for his dissent from Lakatos's views on the methodology of "research programs," and his replies to several critics of his previously published writings.

The main thesis of the "anarchism" or "antirationalism" he presents in this book is that with one lone exception, there are no methodological principles to which the procedures of any "progressive" science either do or should invariably conform — such as the principle that a scientific theory is refuted, and should therefore be rejected, if it conflicts with established experimental findings. A strict adherence to such rules, he maintains, can inhibit

the growth of knowledge; and the only principle which will not do so is the "anarchistic" one that "anything goes." However, Feyerabend is sufficiently a "rationalist" to offer reasons and evidence (drawn from the history of science) in support of his central thesis. Indeed, more than half the book is devoted to this task.

Several other closely connected views are also presented at some length. Feyerabend maintains that the familiar distinction between facts of observation and theoretical beliefs is pointless, since all statements codifying observations involve theoretical assumptions. Accordingly, a scientific theory is "tested" by confronting it not with "bare facts" but rather with statements that are inescapably theoretical and at bottom embody some inclusive if tacit conception of the nature of things. Science is therefore said to "progress" by "proliferating" alternatives to what appear to be well-established theories: this is so because the critical observational facts which really test a theory are typically suggested and identified only by some alternative theory. Moreover, in opposition to widely held views, including Lakatos's methodology of research programs, Feyerabend believes that in general there is no "rational" way of choosing between alternative theories. For according to him, theories may be and frequently are "incommensurable," because they are committed to radically different "ontologies," as Newtonian physics and relativistic theory are alleged to be. The decision between such incommensurable theories is in consequence said to be made largely on the basis of esthetic preference, metaphysical prejudice, religious conviction, or personal wishes. He concludes that science is closer to myth than is commonly recognized, and that the much advertised skeptical rationalism of science in contrast with the dogmatism and unreliability of other modes of thought is a "fairy tale."

There is no room for a detailed critique of Feyerabend's various contentions, and I must limit myself to a few general comments, omitting evaluation of the soundness of his curious historical judgments. In the first place, he is attacking what is pretty much a straw man when he argues against a notion of method according to which there are "firm, unchanging, and absolutely binding principles for conducting the business of science." It would be difficult to find many contemporary scientists or philosophers of science who hold that the principles they may be employing in assessing the validity of cognitive claims in science cannot be modified and improved in the light of continuing inquiry, or who maintain that the

adoption of such principles is incompatible with recognizing that their scope may not be unlimited or that discretion must be used in applying them. It would surely be misleading to say that because a judge in a court of law must exercise judgment when deciding a case, he is bound by no legal rules, or that because there are limits to what a given cutting tool (such as a knife or saw) can do a carpenter must foreswear the use of cutting tools. It is equally misleading to hold that because it might be undesirable in certain circumstances to adhere to some methodological precept, the one principle that is being followed is that anything goes. Indeed, in those instances Feyerabend cites in which a contrary of some widely held methodological rule is supposedly followed, it is clearly not the case that anything goes. Consider his claim that a science sometimes advances by proceeding "counterinductively" - i.e., by proliferating new hypotheses which, like Newtonian theory, were allegedly inconsistent with what were regarded as well-established assumptions, such as the laws of Galileo and Kepler. Now, there is no shred of evidence that Newton (or any other scientist Feyerabend mentions) proposed his theory in order to develop an alternative inconsistent with accepted views. But however this may be, let us grant for the sake of the argument that such evidence is found in this manner. What is to be done with this evidence? Should we proceed counterinductively with respect to it, and in consequence perhaps retain the accepted theory? It would be silly, however, to proceed in this way, nor does Feyerabend recommend it, if the rationale of the initial counterinductive maneuver was to test that theory. Surely in such a situation it is not the case that anything goes!

On the other hand, in consonance with his dicta on proliferation. Feverabend maintains that any idea, however ancient and seemingly without foundation (such as Voodooism) is capable of improving our knowledge. But since it is unclear from his discussion what he understands by "knowledge" and its "improvement" or whether he thinks that knowledge is ever achieved, it is difficult to say whether he is faithful to his anarchism in this pronouncement. Moreover, since he offers no explicit general formula stating when two theories are supposedly "incommensurable," and instead tries to explain the notion by examples from studies of perception, artistic styles as well as science, it is no less difficult to decide when two given theories are incommensurable, or to understand why the adoption of one such theory rather than the other contributes to the growth of knowledge. Thus, he asserts that

classical and relativistic mechanics are incommensurable, because the former assumes while the latter denies that there are properties (such as shapes and volumes) which "inhere" in physical objects, so that relativity theory "cannot share a single statement with its predecessor." But if this is so, how is it possible for proponents of these theories to communicate with one another concerning the relation of the theories to experiment? Feyerabend's discussion of this point is not very helpful. He maintains that the Michelson-Morley experiment, for example, can be formulated in each of the two theories, but denies that "it is the same experiment which confirms one theory and refutes the other." The two formulations possess only an identity of reference, though what is thus referred to cannot be stated in a language common to both theories. Just why this is so remains obscure. In any case, he is thus led to maintain the amazing thesis that since the two theories are incommensurable, a decision between them cannot be made on rational grounds in the light of the available evidence neutral to the theories. This outcome is perhaps to be expected if one holds, as he does, that "knowledge ... is not a series of self-consistent theories . . . ; it is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible ... alternatives." However, this makes sense only on the assumption that we have no knowledge whatsoever, neither of singular nor of general truths - an assumption which not only makes Feyerabend's cognitive claims nonsensical, but is also incompatible with just about every action in which we must engage to survive.

ERNEST NAGEL

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The Fascist Persuasion in Radical Politics. By A. James Gregor. (Princeton, N.J.: Princeton University Press, 1974. Pp. 472. \$15.00.)

Since the publication of his book *The Ideology of Fascism* in 1969, Professor Gregor has developed an interpretation of Fascism which, if accepted, would radically alter our perception of twentieth-century revolutionary movements. In *The Ideology of Fascism* Gregor argued that Fascism was no less ideologically coherent than Marxism-Leninism and that the policies implemented by Fascist governments reflected this ideological coherence. In *The Fascist Persuasion* he goes one step further by developing themes that were peripheral in the earlier book. His intent and method are clearly described in Chapter One. He wants us "to

reconsider the now traditional distinctions between 'left' and 'right-wing' radicalism' according to which Fascism and Marxism-Leninism are at opposite ends of the political spectrum. Gregor proposes to bring out their "suggestive similarities" by examining their ideological roots, techniques for mass mobilization, and by disregarding their "obvious (but to my mind and for our purposes, unilluminating) differences" (p. 22).

There is a definite resemblance between Gregor's notion of the related nature of leftand right-wing movements and the views advanced a generation ago by such analysts as James Burnham, Peter Drucker, Carl Friedrich, and Zbigniew Brzezinski, whose works he cites approvingly. But whereas these earlier writers were primarily concerned with the economic and institutional arrangements of Fascist states, Gregor is only concerned with understanding how revolutionary movements mobilize the masses for political action. His message is that the prescription for successful mobilization was first devised by Mussolini's "paradigmatic fascism." The conclusion is that "perhaps Marx was not, after all, the prophet of our time. Perhaps in a curious but not totally incomprehensible sense, Mussolini was" (p. 188).

From this perspective, Gregor sees Mussolini as the true precursor of Third World leaders such as Mao Tse-tung and Castro and of the would-be revolutionaries of the American New Left and black liberation movements. All of them are said to share Mussolini's belief in the primacy of leadership over economic determinism in setting the stage for revolution. Like the Fascists, they also appeal primarily to nationalist rather than class resentments, cater to popular cravings for status and prestige rather than to economic interests, and promise economic expansion and modernization within the framework of national society rather than through international cooperation.

This kind of book often generates more heat than light. Not only is the tone polemical and even downright acrimonious, but the evidence is presented in such a one-sided way that the reader can well understand the apoplectic reactions Gregor mentions in the preface. When Mazzini is made out to be a precursor of fascism, or the contributions of Antonio Gramsci are not brought into the discussion of how Marxist intellectuals must relate to the masses, or Mao's thoughts on the question of revolutionary leadership discussed without mentioning his concept of the reciprocal influence between the political leadership and the masses, readers have a right to suspect that they are not receiving all the information to which they are entitled.

Not that Gregor's book is hastily put together. On the contrary, it is clearly the result of many years of diligent research and meditation. Furthermore, many of his insights are very valuable. He seems to be right on target when he emphasizes the cognate relationship between Marxism and Revolutionary Syndicalism, or when he draws attention to the productivist and modernizing aspirations of Fascist movements. Some of his specific assertions, however, should not go unchallenged. What evidence does he have for stating that Mussolini filled party vacancies with young people, when it is well known that by the 1940s the party was in the hands of aging cronies (p. 248)? Elsewhere Gregor leaves us with the impression that the main reason Mussolini failed to establish a solid working-class basis for fascism was the hostility of the "orthodox socialist parties who maintained a sectarian resistance to fascism" (p. 316). This explanation does not tell us why Mussolini gave only erratic support to his own labor unions. The antilabor bias that was deeply ingrained in fascism may not be immediately evident from a study of Fascist ideology, but it was certainly an integral part of Fascist policy.

Of course, we should not forget that Gregor wants to tell us mostly what the Fascists said in order to gain power and not what they did once they were in power. Nevertheless, to quote his own words from The Ideology of Fascism, "The implied assumption is ... that some identifiable fascist precepts and convictions exercised some influence over overt behavior" (p. 14). That is precisely the questionable assumption, and nowhere is that more evident than in the case of Mussolini's behavior. Even if we accept Gregor's concept of ideology as being primarily a set of observations and intuitions about how to manipulate the masses, it is difficult to explain Mussolini's rise to power on the basis of ideological considerations. Gregor shows that Mussolini was more knowledgeable about the political literature of his time than his detractors have given him credit for, but in the balance this may not mean very much. The question is whether this knowledgeability guided his political actions or merely enabled him to rationalize decisions made on other grounds.

The evidence indicates that when it came to making and implementing policy decisions, ideology served essentially as a smoke screen. Ultimately, people accepted or rejected fascism depending on whether they expected to benefit or lose from its victory in tangible and sometimes carefully specified terms. Thus, members of vested interest groups such as big business,

landowners, the army, and the bureaucracy accepted fascism because they correctly anticipated gains, while industrial workers, share-croppers, and most landless laborers rejected it because they anticipated setbacks.

Had Gregor approached fascism from a social as well as an ideological perspective, he might have come up with a more comprehensive and convincing explanation because it is in the realm of social policy that the real differences between Fascist and Marxist-Leninist movements stand out. But, having decided beforehand to exclude any consideration of the differences, he sees - not surprisingly - only their similarities no matter how superficial they might be. The larger perspective would also have enabled him to give a more historically accurate account of the role that organized violence plays in the rise and consolidation of Fascist regimes, a role which he denies because "mass murder, brutality, and violence do not uniquely distinguish any political system" (p. x). On the contrary, the injection of the principles and practice of paramilitary struggle into political contests is a distinguishing feature of fascism and, for better or worse, an original contribution of the Fascist squads.

When all is said and done, it seems best to conclude that fascism was brought to power by a combination of systematic violence directed against the most steadfast opponents, by the politics of compromise designed to entice powerful vested interests, and by an ideological appeal that was most persuasive among members of the lower middle-class. Fascism was brought to power by identifiable social groups moved by specific expectations and not, as Gregor would have us believe, by amorphous "masses composed of individuals who enjoy no strong community or class ties, who are 'detached' and 'anomic' as a consequence of protracted crises" (p. 414). In other words, Gregor's insistence on the overriding importance of ideology distorts the historical reality of his paradigmatic model. He has written a thought-provoking book, but also one which is only minimally related to fascism in any of its historical manifestations.

ROLAND SARTI

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Political Science: A Bibliographical Guide to the Literature. By Robert B. Harmon. Second Supplement. (Metuchen, N.J.: Scarecrow Press, 1972. Pp. 394. \$17.00.)

This Bibliographical Guide represents the

second supplement, thus up-dating Professor Harmon's two earlier bibliographies on this topic that were published in 1965 and in 1968 respectively. Apparently trying to make his bibliography useful to the specialist in political science as well as to the layman, the editor lists highly specialized literature in the field next to more general works and text books. The entries provide only basic information on primary and secondary literature, with the exception of the section on Reference Sources, which is annotated.

In terms of basic coverage, one-fourth of the volume is dedicated to theory, methodology and reference works in political science, while three-fourths cover the remaining major fields of the discipline. In addition, the volume contains an almost two-hundred-pages long authors' and titles' index, which certainly is an asset for the users.

More than one-third of all entries compiled in this work represent items published abroad. This adds to a diversified character of the volume and helps to awaken the awareness about the contributions of other nations to political science. This broad-minded approach does not apply, however, to Professor Harmon's chapter on American Government and Politics, which does not include foreign publications. Such an exclusive selection of entries for this chapter may wrongly suggest that there are no worthwhile foreign scholarly works available in this field.

Foreign publications that are included in the other chapters require more attention to spelling; and in the case of some generally less known languages, a parallel translation of titles into English would have been highly desirable.

While the coverage of the political science literature, particularly for the years 1965-1970 is good in general, there are some obvious weaknesses and omissions. First, the economic factor in international relations and politics, e.g., foreign trade and aid, is hardly touched upon, with the exception of a short section on the underdeveloped areas. Further, we find among the elaborate subdivisions along regional, political, and ethnic lines also several blanks: there is no entry on Eastern Europe and hardly any literature is listed on most of the Eastern European states. We encounter similar gaps in the section on the Soviet Union, where works referring to the federal structure and the multinational composition of the U.S.S.R. are scarce or not represented at all. On the basis of the given compilation, a reader may get the impression that only Russians, Balts, and Turkmens live in the Soviet Union, and that such issues which are covered within a different context, as nationality problems, civil liberties issues, and the Jewish question, do not apply to this part of the world.

In spite of these shortcomings, the *Bibliographical Guide* must be recommended as a valuable, concise reference work to contemporary political science literature.

IHOR KAMENETSKY

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Ideology, Reason, and the Limitation of War: Religious and Secular Concepts, 1200-1740. By James Turner Johnson. (Princeton, N.J.: Princeton University Press, 1975. Pp. 291. \$12.50.)

Johnson has made a two-fold contribution to the history of ideas and practice with respect to war in the European tradition. By taking the limited period he has been able to go into more detail than I could in my Christian Attitudes to War and Peace. Second, he analyzes the views of a great many writers from the late Middle Ages through the Puritan revolution with a fullness not achieved elsewhere to my knowledge.

In an epilogue the author tells us that we need a fresh appraisal of "the possible relations between ideological and non-ideological restraints on war." To this theme he proposes to devote a future study. It will be welcome.

Some of Professor Johnson's assertions call for discussion. He takes the formulation of the late Middle Ages as the classic doctrine of the just war, seeing in it a fusion of ideological and nonideological strains. I would be inclined to go back further: the just war concept in its essential features was formulated in classical antiquity from Plato through Cicero, and well suited the situation of the Greek city states, which were relatively equal in power, endowed with a common culture, and not desirous of exterminating each other. The conduct of war must, therefore, be subject to restraints. Cicero elaborated the rules for restraint and insisted that the just war must be waged under the authority of the state. Hence a revolution could not be a just war. Augustine took over and Christianized the concept, adding that the motive must be love, and churchmen should not be involved.

For some eight centuries after Augustine Europe was harassed by successive invasions and the code in practice went by the board. In the eleventh and twelfth centuries came efforts at restraint and coincidently the crusade. Johnson objects to my calling this a separate category because the scholastics subsumed it

under the rubrics of the just war. Granted, but they did so only by subversion. Johnson might be the more ready to regard the crusade as a separate category had he used Carl Erdmann, Die Entstehung des Kreuzzugs Gedankens (Stuttgart, 1955), and even more had he given attention to the just war and revolution, which by definition is excluded. The problem became acute in the sixteenth century when the Catholic states attempted to wipe out Protestantism. How could subjects in these states resist without revolution? The solution was found by making distinctions between grades in the magistracy. Lower authorities, also wielding the power of the state, could resist the higher: the princes in Germany against the emperor, the princes of the blood against the crown in France, parliament against the king in England. Before long the device was abandoned in favor of popular sovereignty, combined with the Holy War, which Johnson recognizes as a synonym for the crusade and concedes that it disrupts the theory of the just war.

He takes issue with my statement that in the prosecution of war the crusade is unsparing. I did point out that the crusading Independents committed no atrocities against the Scotch Presbyterians and I might have added against the English Anglicans. Abundant examples can, however, be given of atrocities in crusades in accord with Cromwell's dictum that the Christian should love the enemy as his enemy and hate him as God's enemy. War is more humane when God is left out of it.

The most cheering note in the book is the hope that the concept of humanity may induce restraint.

ROLAND A. BAINTON

New Haven, Connecticut

Discretion to Disobey: A Study of Lawful Departures from Legal Rules. By Mortimer R. Kadish and Sanford H. Kadish. (Stanford, California: Stanford University Press, 1973. Pp. x, 241. \$8.95.)

The problem of political obligation is ancient, recurring, and fundamental. The historical and logically possible responses to the issues which it raises are also varied, if not contradictory. Regardless, much of the literature on the subject tends to portray similarly a basic dilemma. The individual must choose either to obey the law and to act legally or to disobey it and to act illegally. The alternative of disobedience which is lawful by standards within the legal system has seldom been studied.

Discretion to Disobey is important because

of its well-illustrated analysis of precisely this possibility. The central thesis of the book is the lawfulness and desirability of some kinds of departures from mandatory legal rules. The authors, a philosopher and a law professor, do not deny that such rules impose genuine legal obligations. Rather, Mortimer and Sanford Kadish argue that these duties are "remittable" under some conditions. The discretion to disobey lawfully in this sense pervades the American legal system, the major focus of the book. The opportunity to engage in such disobedience is facilitated by "recourse" roles. They extend a "liberty in handling obligations" (p. 35), the nature of which is examined in the first chapter.

The second chapter is a valuable analysis of lawful departures from mandatory rules by officials. At times they have the liberty to substitute their own judgments of what is appropriate for the dictates of the law. To be "legitimated" these "interpositions" must be practically effective, institutionalized, and legally unreviewable. One of the several examples which the authors analyze in depth is the power of the jury to acquit in criminal cases. The existence of this kind of liberty presupposes "deviational" discretion. It is the "exercise of authority in ways ... either unauthorized or prohibited by rules of competence" (p. 42). This authority is obviously incompatible with the classical model of the rule of law, which the authors criticize on familiar grounds.

The third chapter is an analysis of "legitimated disobedience," or lawful departures by citizens from legal rules. The authors assail the prescription by advocates of "law and order" for unqualified compliance with legal rules. Under certain conditions the citizen is, and ought to be, exempt from the usual liability to punishment for disobedience. The norms of invalidity and the lesser evil constitute, for example, possible defenses against criminal prosecutions. A defendant is entitled to argue that the rule which he or she disobeyed is unconstitutional or invalid for some other reason. A person accused of a crime may also claim that his or her "illegal" action was the lesser of the evils under the circumstances. If the judges are persuaded by these arguments, then the defendant is not legally punishable.

This analysis suggests that nonliability to punishment is one of the requirements of legitimated disobedience. Mortimer and Sanford Kadish emphasize exactly this point throughout most of their book (especially pp. 142, 147). Disobedience which satisfies the other requirements, but is punished, is not legitimated. This implication is harsh, but un-

derstandable; otherwise, the conceptual problem arises of how actions which the legal system punishes can be lawful. At times, however, the authors take a position which raises precisely this difficulty. They imply that certain kinds of departures from mandatory rules are legally justified even if punished (p. 99). The justification of this contention appears logically to entail the appeal to a "higher" law. The resort to such a standard obviously changes the nature of the central thesis of the book. The ambivalence which results obscures not only the "message" of the authors but also the concept of law which it presupposes.

The fourth chapter is an evaluation of legitimation as a social strategy. The authors conclude that its values, "despite the risks, may possibly prove compelling" (p. 177). They justify legitimated interposition as a means to achieve justice in special cases and to adapt law to social change. They also argue that deviational discretion may be preferable to the deliberate use of delegated discretion. Mortimer and Sanford Kadish defend legitimated disobedience on various grounds, among them that it breeds respect for law, expands freedom, and encourages citizen participation in government.

The fifth chapter spells out the implications of the foregoing arguments for the concepts of law and legal obligation. The authors describe "principles of acceptance" which are the ultimate criteria for the application of mandatory rules. The existence of these norms must be inferred from the operation of the legal system rather than explicit rules. Legal realists, positivists, and proponents of natural law are criticized for their neglect of these standards. Mortimer and Sanford Kadish also object to the rational-bureaucratic model of a legal system. They conclude with a defense of their preferred model of checks and balances.

The critical part of this chapter vividly illustrates the value of the advice which Ralph Waldo Emerson once gave Oliver Wendell Holmes, Jr. After reading a paper of the latter's on Plato, the great essayist commented: "When you strike at a king, you must kill him." Mortimer and Sanford Kadish strike at the royalty of jurisprudence, but do not slay them. The only fatalities are straw men. Very few, if any, of the diverse theorists whom the authors criticize in effect ignore "principles of acceptance." Most of them were aware of the existence and desirability under certain circumstances of deviational discretion. Their commitment to the rule of law was qualified. For example, St. Thomas Aquinas insisted that departure from the letter of the law is legally justifiable. Strict compliance with a legal rule could be harmful to the general welfare, which is contrary to the intent of the lawgiver.

Other evaluations and concepts of Mortimer and Sanford Kadish are also subject to criticism. The wisdom of their preference for deviational discretion is open to very serious doubt. Confining, structuring, and checking discretion along the lines urged by Kenneth Culp Davis may well be a better alternative. At the least his book, Discretionary Justice, is an excellent statement of the other side of the question. Nevertheless, the merits of Discretion to Disobey far outweigh these defects. It is a dispassionate, clearly written, stimulating, and relatively original study of a most important problem. As such, the book is a valuable contribution to political and legal philosophy as well as the study of the legal process.

WILFRID E. RUMBLE

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Marxism and Imperialism. By V. G. Kiernan. (New York: St. Martin's Press, 1974. Pp. xi, 260. \$18.95.)

Mr. Kiernan is British. He describes himself as an independent Marxist (p. viii). The book is a collection of six essays written at various times and for various occasions, introduced by a long introductory chapter on the Marxist theory of imperialism and its historical formation. Mr. Kiernan is well read in the historical and journalistic literature of the field. He writes with a pleasant, easy style, with occasional flashes of Scottish wit. He follows no party line. He is not quite what I would call a unitarian Marxist. (I suspect he thinks he believes in it, rather than believing in believing it.) He does think capitalism is here to stay, although he wants socialism (in some form not wholly specified) to triumph. He sees that there are important differences between American and European imperialism, and he sees imperialism as a problem of great complexity and diversity, with historical roots in sentiment, in imitative behavior, in upper-class cultures, and in religious and nationalistic feelings as well as in economic interest.

After the introduction, which is a useful exercise in the history of Marxist thought, there is a chapter on "Farewells to Empire," a review of mainly British literature reflecting the collapse of the legitimacy of empire. A chapter on "Imperialism, American and European," traces the uncertain course of imperialist sentiment in the United States, contrasting the "people's imperialism" by which a continent, empty by standards of European technology, was settled, with dubious ventures overseas. Chapter 4,

"The Peasant Revolution," gives Portuguese Guinea about the same amount of space as China, and is rather critical, though not very thorough on Maoist thought. The last three chapters are on India; one, "Marx and India," which digs up some interesting material from Marx's journalist days; one, "Marx, Engels, and the Indian Mutiny." The last chapter is "India and the Labor Party," which is certainly the most entertaining and passionate in the book, and reveals that the British Labor Party was certainly more British than anything else and that its attitude toward India differed very little from that of the Conservatives. There is nothing much on the role of the Labor Party in the transition of India to independence and one would have liked to see another chapter on that, as it clearly represented an extraordinary rapid turnabout in opinion and policy.

I must confess that I found this volume, though frequently entertaining and instructive, another example of the curious sterility of Marxist thought when it comes to understanding complex social phenomena. The real trouble is that the categories of this way of thinking struggle, dialectics, revolution, class, exploitation, capitalism, and socialism - are both too heterogeneous and too incomplete to bear the weight of analysis of really complex phenomena. It is like trying to do chemistry with the medieval four elements. Kiernan is clearly an intelligent and perceptive writer and he constantly transcends his Marxism, but he makes no attempt at any quantitative analysis, cites little real evidence, and includes an enormous amount of hearsay. One of the great tragedies of the last hundred years or so has been that the real moral insights and legitimate indignations of the Marxists should have been encapsulated in such a narrow prison of the mind as Marx's social dogma. This work is perhaps a sign that at least some of the inhabitants of the prison are breaking out of it. If so, one wishes them well.

KENNETH E. BOULDING

University of Colorado

The Ideology of Order: A Comparative Analysis of Jean Bodin and Thomas Hobbes. By Preston King. (New York: Barnes & Noble, 1974. Pp. 352. \$20.00.)

Professor King's subject is absolutist ideology, characterized as "a universal justification for limitless movement towards centralization" (p. 257). The ideas of Bodin and Hobbes are examined in detail as the outstanding exemplifications of this ideology, for not merely do

these authors describe absolutist politics and commend it as an appropriate response to specific circumstances, but they also "recommend ... the concentration of power in an a priori, which is to say illimitable, fashion" (p. 260). Although King provides some contextual information about the Wars of Religion and the Great Rebellion, his enterprise is not primarily historical: rather he is concerned with the logical and empirical validity of the absolutist ideology, both of which he disputes at length.

King finds, indeed, that the absolutist ideology is seriously defective in a number of respects. He objects to its a priori character, for its invariable recommendation is that power be concentrated, increasingly and illimitably, upon a sovereign authority. He does not deny that some concentration may occasionally be called for, but he regards absolutism as such as absurd.

Second, he finds improper the absolutist's subordination of the idea of justice to the idea of order: he argues that order is nothing more than the preservation of a set of rules concerning social conduct, and that these rules may legitimately be submitted to the criterion of justice:

Every concrete order projects a hierarchy of norms. These hierarchies do not exist prior to what they control, but as a part of what they control. A political order, a political system, taken as a rank order of norms, does not precede justice; rather, it is an expression of a reasonably, but never a totally, coherent conception of justice.... Justice is not built upon an order, but is contained within it; and where an order contains an injustice, one does not build justice upon it, but merely changes the particular that is regarded as unjust; and if the injustice is monumental, change of a proportionate dimension may be required, and the existing order disordered in order to achieve a better one (pp. 284-285, emphasis in original).

Third, there is the contention that an absolute sovereign is the best (and probably the only) way of obtaining security and stability. For the absolutist "nothing is more important than order ... there is virtually nothing which we may not be forced to sacrifice . . . to obtain it. Hence the belief that the power of government must be absolute, perpetual, unlimited, above the law - if it is to do its job properly" (p. 283). But King is unconvinced that absolute government does in fact do this "job," and he argues that most men feel (with justification) that security and stability are more likely to be the outcome of an open, pluralistic regime, in which they and their representatives are able to participate in the formulation and execution of public policy.

In scrutinizing the absolutism of Hobbes and Bodin, King thus has useful, if not strikingly original, comments to make. On one point however, he convicts Hobbes of seriously misleading his readers on a matter which other critics have neglected. While preferring monarchy, Hobbes held that a body of men could perform the functions of the sovereign as well as an individual. But King points out that a body must have certain rules of procedure and that the people concerned cannot purport to be above these rules as an individual monarch can purport to be above the law which he himself makes. It is, seemingly, possible for men to act in conformity with a set of rules in the absence of an over-awing sovereign power, a concatenation of circumstances which, in describing "the natural condition of mankind," Hobbes had rejected. Indeed, as King remarks, "Hobbes's entire argument for the absoluteness of the sovereign flows from the assumed impossibility of men being governed by rules (p. 249, emphasis in original).

Although King does not purport to write primarily as an historian, he cannot, one feels, entirely escape the proprieties of the historical mode of discourse, and some of his observations are so surprising as to suggest an inadequate knowledge of the early modern period. He tells us, for instance, that in the Great Rebellion "not only the royalist but also the parliamentary party held that the king could not be disobeyed" (p. 66): in fact, a number of royalist writers (e.g., Henry Ferne, Henry Hammond, John Spelman, James Ussher, even Hobbes when discussing the predicament of the Christian subject confronted by a non-Christian sovereign) made the traditional distinction between disobedience and resistance, allowing the former when the king's commands contravened those of God, while holding the latter to be strictly forbidden, explicitly by the Scriptures (e.g., in Romans 13 and 1 Peter 2) and implicitly by the order of God's Creation. For the Parliamentarians, the specific political order was a creation of "the people" whose representatives were to protect that order when it was threatened by the aspiring tyrant, while texts such as Romans 13 and 1 Peter 2 referred to legitimate rulers and gave no protection to the tyrant. Equally puzzling is the assertion on p. 20 that Locke's political thought pointed "in two contrary directions: the one towards some form of separation of powers, the other towards an unlimited concentration of power in the hands of the people." If Locke did, indeed, give any credence to such a "concentration," many a lecture will need to be rewritten.

Finally, one cannot but deplore the blurring

of the impression of sharpness left by much of *The Ideology of Order* by King's occasional lapses into otiosity. While able to wound Hobbes with the thrust about rules noted above, King is also capable of telling us "that it is far more difficult to conceive of a group, collectively speaking, having a single will, than it is to conceive of a single individual having such a will. A single individual is a biological unit, a group of individuals is not. There is of course nothing startling about this reflection" (p. 275). Indeed.

JOHN SANDERSON

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The World and Ideas of Ernst Freund: The Search for General Principles of Legislation and Administrative Law. By Oscar Kraines. (University: The University of Alabama Press, 1974. Pp. 221. \$8.00, cloth; \$3.50, paper.)

Although the title of this book is somewhat misleading — inasmuch as the "world" of Ernst Freund does not surface very clearly in this short nonbiographical volume — the author provides a good summary of Freund's ideas and a balanced assessment of his considerable influence as a pioneer of important political and legal reforms.

To political scientists of an earlier generation Freund would need no introduction. That generation was less mesmerized by the natural sciences and their supposed methodological precision and was more aware than our own that the social sciences, including law and political science, as Felix Frankfurter once said, are merely conventional, but convenient, departmentalizations of "holistic perplexities." Lines between law and political science were then less arbitrarily drawn, and public law was a common, respected domain of both disciplines. Hence, while I share Professor Kraines's hope that this book will reawaken an interest among political scientists in Freund's work, I entertain no high expectations in that regard.

At the beginning of his professional career Freund held appointments in political science, initially at Columbia and later at the University of Chicago. In 1902 he became a charter member of the law faculty at Chicago, where he served until his death thirty years later. In 1903 Freund helped found the American Political Science Association and he served as its president in 1916. Throughout his career he was a frequent contributor to political science, as well as law journals.

Freund, says Kraines, was neither theorist nor ideologue. Like Frankfurter, his friend and associate in numerous undertakings, he was an activist scholar, pioneering or caught up in movements for social and legal reform. His published work - consisting of twenty books and pamphlets and ninety articles - is marked by extraordinary erudition, acute perceptions of sociopolitical trends, and penetrating analysis. Most of his work is explicitly prescriptive. Because some of Freund's writings are occasional or address matters in which he possessed little expertise, their relevance today is very limited. However, many of his books and articles, despite their stylistic ponderosity, are worthy of renewed study and suggest potentially fruitful lines of further investigation.

According to Kraines, legislation and administrative law are the fields in which Freund's scholarship was most sustained and his contributions most enduring. These contributions are summarized in two chapters, "The Legislative Response" and "The Administrative Response," which together comprise about twothirds of the book. Both subjects preoccupied Freund throughout his career. Of the four books upon which his scholarly reputation largely rests, The Police Power: Public Policy and Constitutional Rights (1904) was the first. and is somewhat atypical for its emphasis upon constitutional law and case analysis. Noteworthy and, at the time controversial, for its expansive conception of governmental power over the economy, it became a standard citation in judicial opinions, displacing Thomas M. Cooley's Constitutional Limitations as laissezfaire constitutionalism gradually receded. Standards of American Legislation (1917), perhaps the broadest and most provocative of Freund's works, minimizes without rejecting completely, the role of the judiciary - and of constitutional and common-law-norms - in the adjustment of complex socioeconomic relations. Freund's emphasis, according to the author, was upon a science of legislation encompassing such principles as "correlation of statutory provisions ... standardization ... and protection of vested rights and equality." To implement these principles Freund proposed numerous reforms, most of them now taken for granted, including the introduction of statutory analysis into law school curricula, provision for professional bill drafting services in legislatures, and delegation of rule-making power to administrative commissions.

As Kraines points out, Freund, together with Frank J. Goodnow and Frankfurter, pioneered the development of administrative law as a fit subject for research and teaching. The author provides a critical account of the contributions and perspectives of each. Freund's casebook, published in 1911 and again in 1928, as well as his last two treatises, Administrative Powers Over Persons and Property (1928) and Legislative Regulation (1932), emphasize the subject of administrative discretion, with Freund, unlike Frankfurter, looking to legislative norms and internal controls, rather than to the judiciary, for solution.

Professor Kraines is to be commended for this generally perceptive account of the writings and activities of one of this century's most eminent political and legal scholars. For anyone embarking upon a reading of Freund, this study, which places the major works in a broad conceptual framework, is a valuable introduction.

CLYDE E. JACOBS

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An Essay on the Theory of Enlightened Despotism. By Leonard Krieger. (Chicago, Ill.: The University of Chicago Press, 1975. Pp. xi, 115. \$9.25.)

In this essay Professor Krieger attempts to demonstrate the usefulness of the concept of enlightened despotism for historical analysis by arguing for a particular understanding of the conceptual status of the term itself. Krieger's argument is of general interest for he concludes that enlightened despotism, properly understood, is a relevant political concept today, "a permanent category, a continual option, and a recurrent choice in our politics." No longer merely an "evanescent stage in the history of autocracy," enlightened despotism is now seen as "a comparatively credible recourse against confusion" which people turn to when "confused and divided about their own interests and values" and unable to resolve "exacerbated social divisions" (pp. x, ix).

Examining enlightened despotism in the context of the eighteenth century, Krieger rejects two established approaches to the concept, and offers a third. The traditional view of enlightened despotism as a simple empirical term describing the predominant political practice of the period is seen as untenable in the face of substantial contrary evidence: actual political practice was much more diverse than the term suggests; enlightened despotism was only infrequently advocated as a political doctrine; and the concept is fundamentally selfcontradictory. Krieger rejects as historically ungrounded the alternative approach of adopting the created category of "enlightened absolutism" and imputing it to the eighteenth

century as an interpretative tool. His solution is to reinstate "enlightened despotism" with a new status as both sign and symbol: it has a specific literal reference, but also alludes to a more general condition. Enlightened despotism which had "a basis in contemporary usage but also transcend[ed] it," Krieger labels a "schema" "in the Kantian sense of a synthesis mediatory between the empirical and the categorical and enabling the organization of the literal manifold under unifying concepts" (p. 34).

The power of the "schema" to illuminate the larger situation rests on its self-contradictory character. The illogicality of the term captures the contradictory character of eighteenth-century life, specifically the tension between liberal "enlightenment" social goals and the absence of any instrument of change other than a despotic monarch. Thus understanding the internal structure of enlightened despotism enables one to grasp the larger political and social picture.

Krieger's attempt to articulate a historically informative concept is frustrated by his methodological goal of establishing enlightened despotism as a permanent, transcendent category, applicable to all historical periods. Failing to root it in concrete historical conditions, he does not realize the informative power of the idea. If, as Krieger says, enlightened despotism was an attempt to resolve "newly exacerbated social divisions," the schema can only be used to illuminate that situation if those divisions are clearly identified. Otherwise the schema only refers to an abstraction. Krieger for the most part remains on an abstract level, referring to unspecified "material conditions" and "intersections of tradition and innovation." Yet whatever content the term has in the essay is due to its specific historical connections. Krieger admits that enlightened despotism "referred to a particular political institution - the institution of monarchy - by whose peculiar characteristics it was inevitably shaped" (p. 89). Yet he never examines the institution of monarchy or any other social or political institution.

The other side of Krieger's failure to make his concept genuinely informative is the failure to explain the appearance of the concept itself. Clearly the doctrine of enlightened despotism appeared only in certain countries, during a specific period. Krieger offers no explanation of this fact. The final, ironic, consequence of the failure to provide a concrete historical basis for the concept is its uselessness as a contemporary category of analysis. Without knowing the specific conditions that enlightened despotism

referred to, one cannot know what modern conditions are analogous to those of the eighteenth century, and so appropriate for a similar analysis.

MARK GAVRE

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The Intellectual Origins of the Prague Spring: The Development of Reformist Ideas in Czechoslovakia. By Vladimir V. Kusin. (New York: Cambridge University Press, 1971. Pp. 153. \$8.95.)

Writers Against Rulers. By Dusan Hamsik. (New York: Random House, 1971. Pp. 208. \$6.95.)

Books written by émigrés or "insiders" persons who participated in events rather than observed them from a distant vantage point tend to have both advantages and disadvantages. On the one hand they usually benefit from a wealth of on-the-spot information as well as a comprehension of the subjective factors involved and their relative importance. The insider need not be limited by the printed word, nor need he falter in the realm of speculative guesswork, the reading between the lines so vital to the outside researcher of closed societies. On the other hand, the pitfalls of this type of writing usually include a tendency to self-justification - individual or collective - as well as the grave risks not only of subjectivism but of the failure to perceive a general coherent pattern and meaning of events.

Vladimir Kusin has almost totally succeeded in avoiding these pitfalls in that he has presented a most broad, highly coherent picture of the ideas behind the Prague spring, in a variety of fields ranging from pure philosophy, economics, culture, political thought all the way to nationalism and foreign policy. One might have hoped for a still broader discussion of these ideas, more of the contrasting views and proposals, especially in the political but also in other spheres. Yet Kusin does provide the essential views put forth and in so doing presents a loyally characteristic picture of the ideas which generated the Czechoslovak reform movement. Where Dr. Kusin is less successful is in his perhaps subjectively influenced neglect of the Slovak intellectuals and their roles as well as their ideas in the reform movement. While he does discuss them in a few pages (in a chapter on national aspirations which is almost entirely devoted to a highly intelligent discussion of Czech nationalism), it is extraordinary that a book which justifiably emphasizes the role of the intellectuals should fail to discuss

the Slovak theoreticians or analyze the complex position of the Slovaks in the reform process.

Moreover, the author does not see the twelve years (1956-1968) he presents as a process but rather a period of gestation - explaining perhaps his insistence upon 1956, rather than the early 1960s, as the beginning. For the gestation did in fact begin in 1956 with the de-Stalinization in Moscow, but as Kusin himself points out, the Stalinist Czechoslovak leaders, unlike their Polish and Hungarian counterparts, were able quickly to stop the immediate reform-minded response which manifested itself among the writers, students, even some party historians and theoreticians (like Kusin himself). Creative minds were indeed striving and working thereafter, and important works were even written, if in many cases suppressed. Nevertheless, the process itself did not resume until the second round of Soviet de-Stalinization, i.e., in the early 1960s following the twenty-second CPSU congress and a number of internal Czechoslovak events - not least of which was the economic crisis of 1962-64, but most of which were connected with the pressures building for de-Stalinization.

The dynamics of the 1963-68 period are missing from Kusin's book: the push for reform, the setbacks, the growing strength of the reformers within the Party, the clumsy efforts of the Stalinist leadership to give with one hand and take with the other. These events culminated finally in the creation of a temporary coalition of sizable forces within the Party to oust the outdated leadership, and brought to power men more responsive to the by now clearly developed demand for political reform. Not that the facts are missing from Kusin's book (though in one surprising inaccuracy he claims that Dubcek was named first secretary of the Slovak CP in 1966 rather than 1963). Rather it is the dynamic of this pre-1968 period - whether begun in 1956 or 1963 - which is missing. This omission need not, however, detract from the overall value of the book, which is, as intended, a comprehensive, sophisticated, and most rewarding exposition of the basic theoretical tenets of the Czechoslovak reform movement.

By contrast, Dusan Hamsik's book, a more personal account of a participant, has much more limited objectives than Kusin's work. Focusing on the Czechoslovak writers' struggle of 1967, which was one of the factors precipitating Novotny's downfall in January 1968, Hamsik also strives to analyze the evolution of the Czech intelligentsia which caused its revolt against the regime. Like Kusin, Hamsik emphasizes the way in which this revolt cut across

Party lines, uniting Communist and non-Communist intellectuals alike, but, also like Kusin, Hamsik is interested primarily in the disillusion and frustration of the one-time believer, the Communist intellectual who cherishes the ideal but seeks a total reform of the Soviet-East European version of its implementation. Thus this book is of value not only for the rare insider's view it presents of day-to-day Communist Party control over cultural and intellectual life in Eastern Europe, but also for its analysis of the motivating forces which propelled a good portion of the Czech intelligentsia in 1968. (Like Kusin he gives little attention, though more credit, to the Slovaks - and to the students.) This analysis is all the more useful because it was written in 1968, the last chapter . closing with January 1968, leaving only a prologue to add the hindsight.

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Social Indicator Models. Edited by Kenneth C. Land and Seymour Spilerman. (New York: Russell Sage Foundation, 1975. Pp. 424. \$15.00. Distributed by Basic Books, Inc., New York.)

This is an important book. It supplies a paradigm for social indicators which relates them to the mainstream of social science. By defining social indicators as variables in models of social processes, it clarifies the conditions under which social indicators will be useful to policy makers. It points to a social accounting framework as the mature form that social indicators will take. And it sets a new high standard for the integration of theory, methods, and data in social indicators research.

Most of the fifteen chapters were written by social scientists who participated in a Conference on Social Indicator Models held at the Russell Sage Foundation in July 1972. Thirteen chapters are grouped under two headings: (1) replication models based on data from repeated cross-sectional sample surveys, and (2) longitudinal and dynamic models using repeated observations on the same individuals or structural units. They are preceded by a brief introductory chapter by both editors and a major essay by Land.

In his essay "Social Indicator Models: An Overview," Professor Land distinguishes three types of social indicators, each designed for different uses. Output descriptive indicators are measures of the end products of social processes and are most directly related to the appraisal of social problems and social policy.

Other descriptive indicators are more general measures of the social conditions of human existence and the changes taking place therein. Analytic indicators are components of explicit conceptual models of the social processes which result in the values of the output indicators.

The model of a social process would be a system of relationships connecting all the variables involved in it. Land would divide these variables into two endogenous and two exogenous categories. The endogenous categories are (1) output or end-product and (2) side-effect descriptive indicators; the exogenous categories are (3) policy-instrument and (4) nonmanipulable descriptive indicators. This closely parallels the structure of Jan Tinbergen's theory of economic policy, one of the most fruitful conceptualizations in modern economics.

Land also suggests a classification of social indicator content areas according to type of activity, institutional organization, and distributive consequences, and gives a schematic representation of the sociological life-cycle, "a lifecycle ordering of distributed social attributes." Finally, he suggests that the life-space of an individual consists of three measurement domains; (1) objective conditions (the external physical and social conditions of the individual's existence), (2) subjective value-context (the individual's beliefs, expectations, and aspirations), and (3) subjective well-being (the individual's feelings, satisfactions, and frustrations concerning components of the first two domains).

The section on replication models begins with a powerful conceptual chapter by Stinchcombe and Wendt on theoretical domains and measurement in social indicator analysis. There follows an exposition by James A. Davis of the log linear analysis of survey replications based on new methods developed by the statistician Leo Goodman. The remaining chapters were written by Otis Dudley Duncan and several younger scholars who have been closely associated with him. Several chapters grow out of the 1971 Detroit Area Study, which was designed under Duncan's direction as an omnibus replication of several earlier surveys made in the Detroit metropolitan area. Chapter 9 by Featherman and Hauser describes their design for a study of social mobility in the United States, replicating the 1962 survey used by Blau and Duncan in their classic work, The American Occupational Structure.

The section on longitudinal and dynamic models gives central importance to *transition* matrices. The members of a population existing at time t are classified into a mutually exclusive

set of n categories or "states." The transition matrix transfers each member from the particular state he occupies at time t to the particular state he occupies at time t+1. Demographers have used such matrices to show the effects of changes in age-specific fertility and mortality rates upon the total numbers and age distributions of human populations.

Richard Stone, in his chapter on transition and admission models in social indicator analysis, applies transition matrices to the movements of students through an educational system, of patients through a system of medical care, and of employees through an organization. Judah Matras applies them to models of organizational growth and transformation, and James S. Coleman to the analysis of occupational mobility by models of occupational flow.

Transition matrices seem to provide an ideal framework for social accounts which would describe the movements of individuals through "a life-cycle ordering of distributed social attributes," including age, educational achievement, income occupation, marital status, and any others that may be relevant to an evaluation of their well-being. Policies to correct perceived inequities would require increases in the transition coefficients which transfer members of disadvantaged groups into preferred statuses; hence, the coefficients themselves may be viewed as end-product descriptive indicators in policy-oriented models of the relevant social processes. Coleman concludes that such a social accounting framework is the mature form that social indicators will take.

The volume as a whole reflects an indigenous revolution within sociology comparable to that which transformed economics during the 1930s, 1940s, and 1950s. Apart from Stone, Duncan, and Coleman, all authors were under 45 at the conference date and at least seven were under 34. Only 17 of the references cited were published prior to 1955; 56 appeared in 1955-64 and 231 in 1965-73! Four of the authors also contributed to Structural Equation Models in the Social Sciences, edited by Goldberger and Duncan (New York: Seminar Press, 1973), another landmark of the sociometric revolution linking it to similar developments in economics and psychology. The Land and Spilerman volume shows us what serious empirical research will be like under the new regime. In 1967 the idea of a Council of Social Advisors was premature; it may not be so in 1977.

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Jean-Jacques Rousseau, Ecrivain Politique (1712-1762). By Michel Launay. (Grenoble: A. C. E. R., 1971. Pp. 511. Price unknown.)

Rousseau's writings treat so vast a range of problems with such richness, subtlety, and paradox, that no single interpretation is likely to be fully satisfying. What Michel Launay, professor at the University of Nice, says of the Emile and Social Contract holds all the more for the whole of Rousseau's thought: "having given birth to thousands of commentaries, they have not for all that yielded up all their secrets" (p. 358). So much is buried in Rousseau's writings that the scholar can unearth ample materials for interpretation and reflection on the subjects with which he is most deeply concerned. Launay's fundamental concern is with the egalitarian and liberating possibilities of politics, and this concern constitutes the unity of his long, sometimes diffuse, important study of Rousseau's writings through 1762 (Emile and Social Contract).

Launay approaches Rousseau as a political writer in a twofold sense: Rousseau is not only a writer about politics; he is molded by real political experiences, beginning with his youthful Genevan political education, and in his writing devotes increasingly knowledgeable and artful efforts to producing political effects on different audiences. In interpreting Rousseau, Launay combines biographical and historical inquiry with analysis of Rousseau's texts.

Launay's approach produces its best insights through investigating Rousseau's understanding of the difficulties of the political writer. Reflecting on the Abbé de Saint-Pierre's shortcomings, Rousseau was able "to diagnose the causes of philosophers' and philanthropists' political impotence, and to try to develop a remedy for it, by a wholly different, more efficacious combination of reason and sensibility.... Ruse or skillfulness, no longer reason, virtue or force, revealed itself as the essential weapon [of the political writer]" (p. 240). Rousseau therefore meditated on the character and situation of his addressees with sensitive subtlety and realistic political analysis, fashioning his writing so as to achieve the greatest impact on his readers. This kind of politicalliterary analysis enables Launay to uncover an underlying unity in Rousseau's apparently diverse political messages. The Nouvelle Héloise, for example, does not reflect a change in Rousseau's principles, but rather it reflects his way of influencing his French readers; it would be his "reformist tactic adapted to a time and a place, to a certain conjoncture, and would not

exclude a more daring policy in other times and places" (p. 306). Launay's account of Rousseau's relation to political forces in Geneva is similarly helpful for grasping how his coherent political thought is adapted to another specific political audience in the *Letter to d'Alembert* (pp. 341-349).

When dealing with political influences on Rousseau's thought, Launay is treading on rather treacherous ground. His account of Genevan political ideas is interesting in itself, but his claims for its utility in understanding Rousseau's thinking are overdrawn: "many aspects of Rousseau's messages, and not only the details, but the very meaning of his works, would have disappeared for us, if we had not discovered the depth of his attachments to a historically defined social milieu, Geneva's class of artisans" (p. 7). Launay shows that certain political conceptions current among Genevan artisans appear in detail in the Social Contract (pp. 43, 66); but since in Rousseau's writing they are grounded in complicated reasoning, I find that the Genevan popular antecedents shed little light on the grounds, consequences, or problems of Rousseau's teaching. The greatest danger of biographical-historical interpretation, to which Launay occasionally succumbs, is to explain away an argument that seems bizarre or uncongenial to one's own perspective instead of trying to fathom the author's thinking. To give just one example, Launay suggests that Rousseau's theoretical rigor is bent by the prejudices of the Genevan petite bourgeoisie, when he argues that a woman's adultery dissolves the bond of the family. Why, Launay asks, should not the true father simply be he who fulfills the duty of raising the child? If, instead of dismissing Rousseau's position lightly, one gives further reflection to the reasons that underlie it, one is led to a deeper understanding of the problematic character of human attachments their natural roots and their conventional developments.

But Launay has too great a respect for the integrity, profundity, and abiding relevance of Rousseau's texts to do this kind of injustice to Rousseau very often. On the whole, he makes intelligent and interesting efforts to grasp the vital coherence of Rousseau's work as a political writer. Because he discusses so many writings, no one is treated exhaustively; but he makes at least some noteworthy points about each. Of particular value is his discussion of the *Emile* and *Social Contract*. He argues convincingly that these two works together constitute a complete whole. Much of Emile's education is a formation of his sentiments and passions such that he will take seriously the contracts — of

friendship and marriage as well as the social contract - through which free men can sustain honest and decent relations with each other. What we learn from the Emile "on the affective conditons for the good functioning of every contract made between men, sensitive and active beings" (p. 421), helps us to make coherent sense of the Social Contract, and in particular to avoid a simply mechanistic misunderstanding. Launay argues forcefully for the realism of Rousseau's politics, and in this connection shows how the neglected (not to say despised) chapters on Rome in Book IV of the Social Contract play a vital role. Rousseau is not abstract or utopian, but concerned with the possibilities of concrete history, past and future; the image of Rome offers to our reflection "a middle term between the rules of right (wooden rules, which are rigid but able to judge the real) and this real, which is never completely made, which is to be made" (p. 444).

In his conclusion, Launay makes clear that the social contract is not the best model of human relationships for Rousseau, but that, in our world, the contractual attitude, although not the most generous, is the most useful for free men. This judgment points to Rousseau's view that political life or citizenship is not the simply highest human attainment. The problem of the ultimate status of politics in Rousseau's understanding is a theme of his autobiographical writings. Launay speaks of working for another ten years on these and the other remaining writings of Rousseau. Launay's method might be especially useful for their interpretation, and we can look forward in a few years to the publication of further results of his researches on Rousseau.

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Marxism and Epistemology: Bachelard, Canguilhem, Foucault. By Dominique Lecourt. (Atlantic Highlands, N.J.: Humanities Press, 1975. Pp. 223. \$15.00.)

The book is a collection of essays by M. Dominique Lecourt, one of the founders of the Union de Jeunesse Communiste Marxiste-Leniniste and a professor at the University of Paris. The essays deal with the philosophy of science of Gaston Bachelard, Georges Canguilhem and Michael Foucault. Nearly three-quarters of the book is devoted to Bachelard, the most important of the three thinkers, while the other two get under thirty pages each.

Bachelard's philosophy of science represents a strong reaction against the influence philosophy has traditionally exercised over science. In his view philosophy's influence over science is necessarily bad, and constitutes an "epistemological obstacle" to the latter's growth. Science is concerned to understand and explain the world, whereas philosophy is motivated by religious, moral, political, and other "ideological" interests. Philosophy therefore has an inherent tendency to corrupt science by chartering it in the service of extrascientific interests. Further, science is experimental, innovative, and dynamic. It is inherently revolutionary and develops by "dialectical leaps." Its history is a story not of continuous development but "envelopment," not of organic growth but periodic "relinquishment." Philosophy, on the other hand, is concerned with eternal and absolute truths. It aims to comprehend reality in terms of static and timeless concepts and has a "tendency to immobilism." In its approach to history it therefore looks for similarities and continuities, and endeavors to demonstrate that no system of thought is ever completely new. Bachelard thinks that such an evolutionary view of the history of thought necessarily misrepresents the growth of scientific knowledge, and offers the scientist a totally false image of his discipline.

In Bachelard's view the scientific is an independent and autonomous activity, governed by its own criteria of truth and values with which philosophy has nothing whatever to do. It creates its own subject matter and develops its own methods of investigation and procedures of verification. Like Louis Althusser, his intellectual grandson, Bachelard argues that the objects of scientific study are not naturally given, but created by the scientist with the help of the instruments and procedures developed by the scientific community. For example, a chemist is concerned to study pure chemical substances. Pure substances, however, are never to be found in nature. The chemist must therefore purify natural substances with the help of available techniques and in accordance with generally accepted procedures. The object of his study thus rests on "a kind of social guarantee." He has no other means of knowing that it is his proper subject matter than the consensus and agreement of the scientific community.

Bachelard's philosophy of science assigns considerable importance to scientific instruments. He calls them "materialized theories." A scientific instrument is not merely a piece of machinery, but an objective embodiment of scientific intelligence. It defines and creates the

scientist's world. Science "instructs itself by what it constructs." This is true "scientific phenomenology." For Bachelard a concept is scientific only "insofar as it has become technical, is accompanied by a realization technique." Bachelard rightly calls his philosophy of science "technical materialism." It is an epistemological counterpart of the familiar Marxist theory of historical materialism.

Bachelard's theory of science leads him to take a very different view of the role of mathematics in science from the one common in the traditional philosophies of science. For him mathematics is not merely a language of expression but a mode of thought: "The physicist thinks the experiment with his mathematical thought." Mathematics does not therefore merely express but also stimulates scientific creativity. Bachelard argues that the hypothesis of the meson, for example, was "initially an essentially mathematical hypothesis" suggested by mathematical equations, and not a result of new experimental data (p. 56).

After expounding Bachelard's basic ideas at some length, Lecourt outlines those of Canguilhem and Foucault. Unlike his teacher Bachelard, Canguilhem is interested in the biological not the physical sciences, and leans towards a vitalist philosophy of nature. However, like Bachelard, he too aims to develop a "science of science," and to combat philosophy's necessarily ideological influence on science. Like him he insists that science develops by revolutionary and dialectical "leaps," and inhabits a very different world from that of ordinary observation.

M. Lecourt's discussion of Foucault is very brief, and centers around the latter's Archaeology of Knowledge. He briefly outlines Foucault's dialectical and evolutionary view of the growth of science. He refers to but does not discuss Foucault's fascinating analysis of the nature of the ordinary and the scientific discourse, or his well-known distinction between practical and theoretical ideologies on the one hand and science on the other.

M. Lecourt has written an excellent book. He makes available to the English-speaking world some of the fascinating ideas of the French philosophers of science. He also points to interesting similarities of approach between them and Kuhn. One consequence of this is that Kuhn appears less original than he is generally taken to be. Lecourt's book has the further advantage of showing that there are other ways of understanding the scientific activity than Popper's and Kuhn's, the two most popular but ultimately unsatisfactory philosophies of science.

Without wishing to detract from this lucid and well-argued book, I would like to end by drawing attention to its two minor defects. First, M. Lecourt does not fully explain why Bachelard and others are led to revolt against philosophy. As a result their preoccupations appear somewhat eccentric and pointless. It would have been helpful if Lecourt had sketched the background of the revolt, and traced its origin to the historic confrontation between Marx and Hegel. Second, M. Lecourt does not subject the three philosophers to a searching critique. He does not, for example, fault Bachelard's theory of truth, his conventionalism, dualism, and ambiguity about the relationship between the scientific and the ordinary world, but remains content to reiterate the familiar Marxist thesis about the inadequacy of a philosophy of science not grounded in historical materialism.

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The Unravelling of America: Wherein the Author Analyzes the Inadequacy of Current Political Options and Responds with a Christian Approach to Government. By Stephen V. Monsma. (Downers Grove, Ill.: Inter-Varsity Press, 1974. Pp. 228. \$4.95, paper.)

Professor Monsma, author of American Politics: A Systems Approach, assays here a critique of leading political philosophies, in both domestic and foreign policy spheres, and offers his own alternative of "Progressive Realism."

The book deserves a wider audience than it is likely to get. Many will be turned off at once by the framework of "Evangelical Christianity" which he explicitly espouses and applies. If they read on, however, they will find a kind of humane utilitarianism, coupled with an unromantic view of human nature, which is thoroughly compatible with a variety of theological (or humanistic) perspectives; and the specific policy applications would win applause from such theologically diverse a crowd as Adlai Stevenson, Reinhold Niebuhr, and Pope John.

Monsma finds in the Bible two principles relevant to the realm of politics. The first is the doctrine of original sin, which he interprets to mean that man, while not totally depraved, is a mixture of noble impulses and "inordinate self-love" (the affinity with Niebuhr is striking). Second, the doctrine of man's creation in the image of God means that it is the duty of government to maximize opportunities for each person to exercise and develop his creative

capacities. Working from these postulates, he finds elements of validity and invalidity in each of the major political philosophies. Burkean conservatism properly emphasized organicity, but provided no independent norm for judging injustices which inevitably corrupt any society, and "the past becomes a prison" (p. 79). Classical liberalism was commendable for its emphasis on the dignity of the individual, but naive about human nature. American liberalism properly emphasizes the use of government to help the disadvantaged — but overestimates the unselfishness and rationality of government officials. American conservatism avoids that trap, only to fall into its opposite, ignoring the injustice that exists under weak governments. The New Left properly points to dehumanization and materialism, but naively locates the source of the problem in the environment rather than in corrupt human nature. The result of such unrealistic diagnoses is, in every case, unrealistic prescription.

What, then, has Progressive Realism to offer? Not a blueprint but a set of middle axioms designed to maximize "true freedom" and minimize the effects of self-love. They entail an "activist, involved, humane politics" characterized by such principles as "rolling incrementalism" and "built-in administrative checks," and a calculus which balances "the relative number of persons who would gain rights or advantages and obligations or disadvantages, and the relative importance and value of the rights..." (p. 124). By way of brief illustration, the author applies the principles to health insurance and penology.

Turning to the international sphere, Monsma rather nicely examines foreign policy attitudes along two dimensions: idealism-realism, and liberalism-conservatism. Following the earlier pattern, he judges each of the four positions thus created (liberal idealism, etc.), develops his own alternative, and applies it briefly to a problem (intervention).

Evangelical Christians who are concerned about social justice will find this book of great interest indeed — for that branch of Christendom, which typically perceives conversion as the necessary and sufficient solution to all social problems, has produced few such treatises. Others will find it interesting, to the extent that they are concerned with the political philosophies of evangelical Christians — but also to the extent that they resist the temptation to stereotype a colleague on the basis of his theology.

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Hegel on Reason and History: A Contemporary Interpretation. By George Dennis O'Brien. (Chicago and London: The University of Chicago Press, 1975. Pp. 188. \$8.50.)

If one accepts as reasonable the intentions which have guided Professor O'Brien in writing this book, then it can be said that he comes close to success. "My hope is to translate the insights of Hegel (and of Kojève) into terms more current in English philosophical work" (p. 8). More specifically, O'Brien has provided the student with an interpretation of Hegel's philosophy of history, based upon the material published under the title Die Vernunft in der Geschichte. This interpretation is decisively influenced by the Hegel commentary of Alexandre Kojève on the one hand, and by a limited but crucial reliance upon Aristotle on the other. It is externally influenced by the author's desire to make Hegel accessible to analytical philosophers of history, or to students of analytical philosophers. One senses beneath the selfimposed rhetoric of simplicity a solid understanding of Hegel's doctrine (with one or two qualifications to which I shall later turn). O'Brien has read widely in both history and philosophy, and he is able to connect his reading to an exposition of the central points in Hegel's study of history. The book is very well written, and contains many illuminating discussions of individual points. I mention some of these which I find especially helpful: the clear exposition of the status of the texts, in both the German and the English translation, upon which O'Brien is commenting (Chap. 1); the rejection of the distinction between method and content in history as it ostensibly applies to Hegel (Chap. 2); a brief but necessary reminder of the difference between Hegel and Darwin (Chap. 3); a persuasive account of how Hegel's philosophy of history, contrary to criticism leveled against him by Kierkegaard, Schelling, and many a subsequent thinker, is a doctrine of individuality (Chap. 4); a fine discussion of the role of passion in history (Chap. 5); and, in the best single section of the book, an admirable account of the sense of Hegel's doctrine of progress in history, and how he reconciles human freedom with necessity (Chap. 6).

If we consider the work as a whole, O'Brien wishes to translate Hegel into contemporary language by explaining the meaning of the famous claim that reason is the "law of the world" (p. 36 et passim). In order to do this, O'Brien both connects Hegel's procedure with that of contemporary philosophers of science, and (more fundamentally) distinguishes it from philosophy of science in order to relate it with

connoisseurship in art (pp. 44, 132, 165ff). Hegel emphasizes objectivity and a priori elements in the study of history, but he is fundamentally concerned to show the unique value and intelligibility of individual style. This strikes me as on the right track, but it runs the risk of transforming Hegel into an "aesthete." It is, however, true that O'Brien counterbalances this particular danger by underlining the political character of Hegel's teaching. The state provides the matrix for the fulfillment of the individual as the concrete universal (cf. pp. 123, 132, 145 and 146-158). Nevertheless, it is not clear from O'Brien's account exactly how the concrete universal, the product of the state, is for Hegel other and higher than a great work of

There is a second difficulty with the analogy of art. O'Brien emphasizes the similarity between Hegel's categories and the key terms of Aristotle's doctrine: four causes, potentialityactuality, etc. (pp. 45, 62, 145 et passim). O'Brien certainly knows that for Aristotle, the appreciation of art is superior to the study of history because it enables us to perceive the universal. But he does not explain the radically non-Aristotelian nature of Hegel's doctrine of individuality. I believe the trouble lies in O'Brien's debt to Kojève. Kojève combined elements of Marx, Feuerbach, and the early Heidegger in his attempt to read the Phenomenology as the penultimate version of philosophical anthropology (the ultimate version being Kojève's own reading). This entailed the virtual suppression of Absolute Spirit, and an exaggerated emphasis upon the "man-made" nature, not just of history, but of truth or wisdom. One result was that Kojève was unable to explain the difference between animal and human desire, or the emergence of self-consciousness; and he also exaggerated the difference between nature and history in Hegel. O'Brien repeats these errors. He accepts the Kojevian version of the master-slave dialectic, which fails to explain how one "human" consciousness can recognize itself in another, prior to the possession of consciousness of what it recognizes (pp. 92, 135, 162). And he does not bring out the fact that Hegel overcomes the split between nature and history by virtue of his extremely difficult doctrine of Absolute Spirit as Absolute Process.

As much as I admire the brilliance of Kojève, it has to be said that his interpretation is already a vulgarization of the *Phenomenology*. Kojève and O'Brien emphasize Hegel's doctrine of individuality, but leave out the key element which renders this doctrine intelligible (if that is the right term): the Absolute. In O'Brien's

case, this omission is no doubt due to his desire to make Hegel acceptable to analytical philosophy. But I think he takes the wrong tack. Perhaps the worst aspect of analytical philosophy is precisely its approach to such human phenomena as history. Kojève did not take seriously those who believe that the model for historical explanation "is the account one might offer for why an automobile radiator cracked on a certain cold night" (p. 38). We are clearly in for a spate of books attempting to assimilate Hegel to the language of contemporary English and American philosophy. This will be accomplished only at the price of trivializing Hegel, as has already happened with Nietzsche. And this result will benefit no one, Hegelians or analysts. Progress in philosophy depends upon facing up to, and comprehending, the most difficult formulation of the most profound teachings. Much as I appreciate O'Brien's book, I cannot bring myself to believe that it will contribute to this goal. On the other hand, it contains valuable pages, and deserves to be read for these alone.

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The Measurement of Children's Civic Attitudes in Different Nations. By A. N. Oppenheim and Judith Torney. (New York: A Halsted Press Book, John Wiley & Sons, 1974. Pp. 84. \$5.95.)

The International Association for the Evaluation of Educational Achievement resolved in the mid-1960s to evaluate the success of civic education in a variety of different nations. This ultimately resulted in the collection of questionnaire data in 1971 from some 35,000 children, aged 10–18, attending school in ten different countries.

This particular monograph is the first from the project and describes unusually carefully the instrument development phase, through item selection and pilot studies (at least development of the attitude, though not the cognitive or perceptual, measures). This seems well worth doing; obviously the forthcoming report of findings will be of great interest to students of political socialization, if for no other reason than because of the unprecedented size and comparability of the cross-national samples.

The basic attitude dimensions measured here are familiar ones in the political socialization literature: citizenship values (participation, efficacy, patriotism, etc.), democratic values (racial and religoous tolerance, egalitarianism, civil liberties, etc.), and to a much more limited degree, perception of the political process and

of government. A variety of different kinds of measures were tried, including projective tests, open-ended questions, and semantic differentials as well as the more standard fixed-response questionnaire items. In the final testing, however, the projective tests and the open-ended questions fell victim to economy.

Two aspects of the measurement are particularly innovative. Factor analyses are reported within each nation, and are used as evidence of the comparability of items across languages and political systems; these also give a rare look at the coherence of children's political attitudes. Also, the initial pilot testing used projective and open-ended questions to try to detect cognitive styles of response, along abstract-concrete, personalizing, egocentric or sociocentric dimensions. Children's free responses were then used to develop a series of fixed-response items to measure these dimensions. The results of this effort will no doubt be of great interest in the final report.

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From Hope to Liberation: Towards a New Marxist-Christian Dialogue. Edited by Nicholas Piediscalzi and Robert G. Thobaben. (Philadelphia: Fortress Press, 1974. Pp. xiv, 114. \$3.25.)

Marxism and Christianity are powerful historical movements that have had a mighty influence throughout the world. Both are movements of total orientation in the sense that they constitute for their members a Weltanschauung, a way of thinking about reality, and a Praxis, a way of transforming reality. Each movement, however, is split into diverse subgroups, which means that the character of its presence in the world varies radically in time and place. Consequently since the emergence of Marxism as a dominant historical force, these two movements of total orientation have been, at different times and places, suspicious of each other, engaged in struggle against each other, but also attracted to and even allied with each other.

The slim collection of essays under review presents two forms of alliance between the movements, but forms that are sharply different — perhaps irreconcilably so — because of their different locations. In the early 1960s, some Christians and Marxists in Western and Eastern Europe entered into "dialogue" with each other. The dialogue, focusing dominantly on matters of theory, has continued throughout subsequent years in conferences and publications with varying degrees of vitality. During the same period, however, other Christians and

Marxists in the Third World, especially Latin America, joined forces in a common and direct revolutionary struggle against privileged classes and reactionary governments. This volume focuses on the tensions between these two forms of alliance — dialogue and revolutionary action.

All of the contributors are Americans, i.e., United States citizens; most of them are Christians; all of them are sympathetic to the Marxist cause; yet in their precise loyalties and particular interests they disagree.

Robert G. Thobaben introduces the issues at stake with a schematic history of the two forms of alliance. His summaries are compact and elementary, yet suggestive. He clearly favors the more humanist-activist orientation he finds in Third World Marxism.

Virtually half the book is devoted to a Marxist-Christian dialogue held at Wright State University between Herbert Aptheker, director of the American Institute for Marxist Studies, and Thomas W. Ogletree, a theological ethicist. The book contains their initial statements on "What May Man Really Hope For?" and subsequent responses and discussion. The heart of Aptheker's statement is that Marxists and Christians are both utopian and share basic ethical assumptions. Therefore in the United States, as a practical basis for "hope," they should form a united front supporting a twenty-five point (but strikingly modest!) political platform. Ogletree, accepting a Marxist interpretation of immediate historical struggles and concrete human hopes, would supplement that with a Christian interpretation of ultimate religious hope as a means of sustenance in a world of insecurity and failure and a means of acknowledging the limits of all historical accomplishment.

The most vivid contribution is by Shepherd Bliss, Protestant clergyman, who out of deep commitment to the Latin American revolutionary struggle, is impatient, even angry, at academic confabs among comfortable Marxists and Christians. He would replace dialogue with praxis and talk about hope with strategies for liberation. To Bliss, the religious symbols of Exodus and Resurrection deal not with abstract verities and ultimate hope, but with revolutionary experiences of New Life.

Nicholas Piediscalzi's concluding essay appeals for reconciliation and proposes that even additional traditions of total orientation — Jews, Buddhists, etc. — be solicited for a concerted drive toward universal justice.

While the book suffers from significant deficiencies in historical and theoretical depth, it may prove useful given its explicitly narrow aim to introduce nonspecialists to the tensions

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between the theoreticians and the activists engaged in Marxist-Christian dialogue. On the other hand, perhaps the nonspecialist particularly needs more of a grounding in Marxist and Christian principles than these essays provide to comprehend the full import of the struggle between these two approaches to Marxist-Christian alliance.

DOUGLAS STURM

Bucknell University

The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition. By J. G. A. Pocock. (Princeton, N.J.: Princeton University Press, 1975. Pp. 602. \$22.50, cloth; \$11.50, paper.)

The "Machiavellian Moment" may strike us as somewhat drawn out in this long and impressive book, but we are informed right away that "moment" means aspect as well as instant. The title, we are told, was suggested by Q. Skinner of England's Cambridge University. An alternative, unsolicited subtitle might be "From Baron to Bailyn," for Pocock presents Machiavelli as the expounder of a tradition that began with Baron's civic humanism of the fifteenth century and culminated with Bailyn's opposition radicalism of the eighteenth.

Pocock's contribution, however, is nothing less than magisterial, even to these illustrious names. While they worked in compartments, he has connected them; and he has done so with a running start from Aristotle, a welcome study of post-Machiavellian Florentines, and a determined march through the thicket of seventeenth-century England. His acute analysis is usually based on an original examination of the texts, each book considered as a whole. When Pocock relies on other scholars, his purpose is to advance the argument, not to confirm it or to answer objections. This is a long book with a long thesis. The writing is "richly textured," to borrow the author's phrase, with complicated sentences on both hands, nonconditional if's (not if x, then y, but if x, then besides that y) and relentlessly expressive adverbs. There is time for prudent concessions and defensive qualifications - there has to be - but not for polemics.

Civic humanism is the republican virtue of citizens participating in rule. In discovering its importance, Pocock and the scholars whose work he integrates call attention to a modern or premodern body of thought that is quite different from liberalism. Liberalism favors rights over community, liberty over duty, representation over participation, and interest over

virtue. Pocock touches on these issues, but with so much to say in exposition, he does not arrange a confrontation between civic humanism and liberalism, or between Machiavelli and Hobbes and Locke. Hobbes, indeed, is drafted into the citizens' army of civic humanism and made to serve with men he would have considered riff-raff. Although Pocock's method achieves an impressive synthesis, it deprives the "Machiavellian moment" of the clarity it would receive from distinction.

The book begins with three chiefly theoretical chapters on "time," in which a general identity of historicism and secularism is asserted. Clearly historicism is secular, since the primacy of history in this world entails the primacy of this world, but is secularism necessarily historicist? It would seem that human reason might offer an acceptable, nonhistoricist basis for secularism; but while Pocock says little about liberalism, he says nothing about modern science. The fact that Machiavelli does not, and Hobbes and Locke do, rely on modern science is worthy of comment.

To make Aristotle a precursor of Machiavelli, some adjustments are necessary to both, particularly in regard to the status of moral virtue. Pocock concentrates on Aristotle's discussion of "polity" in Book IV of the Politics, but he does not directly raise the question so important to Aristotle of whether moral virtue is for the sake of the polis (and thus is "republican virtue") or the polis is for the sake of moral virtue (and when it is, should be called "aristocracy"). Correspondingly, with an attitude of determined innocence, he ignores or deprecates the criminality which Machiavelli thought necessary to virtue, including republican virtue. He conducts himself like a man who has heard of brothels but cannot believe he is in one, although the evidence of "nonmoral" doings comes even to his averted eye even while he is sitting in the parlor.

With the aid of his discussion of Aristotle's "polity" (not to mention shorter treatments of Polybius and Fortescue), of Baron's researches on Bruni, and of extended analyses of Cavalcanti, Savonarola, Guicciardini and (a pretty phrase) "the lesser Ottimati," Pocock places Machiavelli in a context (p. 183). He allows that "his mind was liberated" and that in The Prince he achieved "a breakthrough into new fields of theoretic relevance" (p. 155). but since the context stated the issues for Machiavelli and limited the extent of his possible discoveries, he is in effect cut down to size with the other cornstalks in the field. Pocock salutes Machiavelli's discovery of the innovative prince, but claims that Machiavelli did not, or could not,

solve the problem of making the prince's innovations durable until he discovered republican virtue in the *Discourses*. Apart from the fact that this does not accord with what Machiavelli says at the beginning of *Discourses* III 35, Pocock seems to have too quickly dismissed his own insight that *The Prince* "does not present a single rounded portrait, but a gallery of specimen types of innovator" (p. 180). For perhaps the teacher of princes can offer something valuable to each specimen type of prince, thus round them up, and thereby avoid the instability of relying on one of them.

In considering the Discourses, Pocock again fails to press a promising point. Although his topic is Machiavelli's way of maintaining the republic, and although the theme of his book is the relation of politics and time, he does not mention the "perpetual republic" that Machiavelli flashes before our eyes (Machiavelli denies it in III 17, asserts it in III 22, and implies it in I 20, II 5, and III 3). He concludes too soon that Machiavelli did not accomplish what he had set out to do, that he was unable to revive ancient virtue because he could not correct the mistakes of the ancients (p. 216). In the end Pocock gives him credit only for reviving the citizen-soldier (p. 218); and in consequence he develops the "Machiavellian moment" in general and inconsiderately as an improvement on Machiavelli. He is quite aware of the difference between the "premodern" (i.e., pre-Hegelian) view of history as movement away from "moral stability" and the modern view as movement toward it, or rather, toward "incessant qualitative transformations of human life" (p. 551); and clearly he prefers the latter. But he does not allow Machiavelli to state or to explain his preference, and so to give his own, far broader definition of the "Machiavellian moment." Pocock's chapters on Machiavelli occupy only an eighth of the book, but they are the heart of it: and at the heart there is a fundamental unclarity. Most students agree that a great turn in the world's affairs occurred at about the time of Machiavelli, and they agree that it brought a change toward secularism and realism roughly of the kind now popularly associated with his name. But because of the power of modern historicism, there is great reluctance to attribute this change to Machiavelli, or even to consider that possibility. Pocock is more learned and more penetrating than most, but he shares this reluctance. For him, Machiavelli is not a prince or a founder but a creature in a context supplied by Pocock's own "model."

There remain eight chapters of this work to be summarized. Pocock continues with Florentine republican thought after Machiavelli, discussing Gucciardini's later writing, the Dialogo del Reggimento di Firenze, and works of Giannotti and Contarini in which Venice is presented to Florentines as a "paradigm." Then follow five chapters on Machiavellism in England, where we see it pass from courtiers to republicans as in the course of the Civil War the notion of the "balanced constitution" took over from the "thesis of descending authority." It falls next to the eighteenth-century "country party" opposing the Court Whigs, and here Pocock's treatment of Bolingbroke and of Montesquieu is outstanding. The last chapter is on the appropriation of the "country" ideology by Americans and its abandonment in the Constitution, as argued by Bailyn and Wood. Pocock believes in addition that the messianism which recurs in American history is the work of the Machiavellian moment. At the end of his book he discloses a "conservative" (p. 551) or liberal (p. 552) distaste for a tradition (the Machiavellian!) which rests on moral absolutes. When a man comes to a reasonable conclusion it is sometimes best to leave his reasons in peace.

HARVEY C. MANSFIELD, JR.

Harvard University

Political Science and Area Studies: Rivals or Partners? Edited by Lucian W. Pye. (Bloomington: Indiana University Press, 1975. Pp. 245. \$10.95.)

This will not be the last book on the topic, and that is a pity. Political scientists are forever gazing into the intellectual pool to see how many heads are revealed; each gazes with the intention of identifying which of them most nearly conforms to his or her own conception of truth and beauty. Too many heads are considered monstrous; even two heads evidently are impossible to live with. This is particularly so when one head, "area studies," seems clearly defined and the other head ("political science"? "functional or process analysis"?) seems very murky and elusive indeed.

Of the ten gazers included in this symposium, only Harry Eckstein is skeptical of the area focus as a guide to the discipline's self-realization. After making a very strong case that Western Europe is a better geographic area than most for the conduct of comparative analysis, he nevertheless opts for "problem communities" as a much better basis for testing theoretical propositions cast at the macrolevel of the polity. His view seems to be echoed by Leon Lindberg, whose case for Western Europe seems to be that, if one wishes to elucidate problems and processes of that group of coun-

tries that are in that vague category denominated "advanced industrial societies," then Europe as an area focus makes a lot of sense. To which Eckstein would reply that European countries are less distinctive than we sometimes think, that some "European" countries (e.g., Australia, Canada, New Zealand, Israel and the United States) are not in Europe at all, and that to speak of Europe as a naturally defined analytical category involves gross oversimplification.

Eckstein is essentially correct, although I am not sanguine about macrolevel generalizations that do not easily guide us to the kinds of empirical investigations that can disconfirm them. I think he is dubious too, but this inference cannot be clearly derived from what he writes.

Some of the other authors express skepticism about theories of political science that are unencumbered by the rich and profound diversity of area-centered phenomena. These skeptical observations, especially those of Myron Weiner, Samuel, Huntington, Kalman Silvert, and Alfred Meyer, deserve most careful attention. Huntington, for example, is typically arresting in a brief essay depicting why the American polity is remarkably unique and why, therefore, concepts, research approaches, and theories that derive from the American experience cannot readily be applied elsewhere, not even when carefully "adapted" to alien lands. Weiner, too, is somewhat rueful in his persuasive assessment of how little mileage we have got out of the dominant general theories of political science when these have been paraded as instruments to help us understand the polities of South Asia. He reminds us that Karl Marx, whose conceptual scheme has been used by some to "explain" everything about every country, was not so simpleminded as to argue that his formulations applied to countries like India and China that had not experienced the preconditions of his historical dialectic. In any event, Weiner's enumeration of how many theoretical propositions don't fit that area of the world is entirely sobering.

One difference between Marx and most of the contemporary general theorists, of course, is that Marx did turn Hegel around and produced a large number of empirical propositions that could be disproved. The difficulty, the frustration, and the agony of more recent so-called general theories is that they often do purport to explain everything but wind up explaining nothing at all, or only the trivial. Some of us have said this before but with little apparent effect. The horror of these "theories" is that they are still uncritically adopted and

"applied" to countries as diverse as Albania and North Korea, China and the Soviet Union, Chile and Uganda, and you name the country.

The statements by Meyer and Silvert are the most trenchant. Meyer disposes of the underlying question of this volume with the laconic "why, both, of course!" Area knowledge undisciplined by orienting concepts becomes an intellectual Babel. Concepts unleavened by empirical information gleaned from areas is likely to remain unverified speculation. Worse than that, we are led to believe we have more useful information than is manifestly the case.

Meyer wants to provoke so that we will reassess. He suggests that, given how little the discipline offers by way of theoretical aids, area specialists with no information about the latter have probably done better work because of this ignorance (p. 101). Furthermore, he argues that a narrow focus on data that can be quantified and hypotheses that can be made "operational" will impoverish and has impoverished the discipline. In line with his recent criticisms of those who write about Communist systems, Meyer stresses that their wholesale incorporation (in their work) of the recent theoretical models of the discipline has been a near disaster. I join him in asserting that these "models" have turned into Procrustean beds, and I add that recent revisionist descriptions designed to compensate for earlier uses of the "totalitarian" model have produced near travesties of political science.

Meyer's attempt to describe the nature and evolution of political science by analogy to the field of art (pp. 117-128) is thought-provoking but overworked. Both fields do indeed deal with the human condition, with reactions to situations of overwhelming power and disorder, anarchy and the fear of death. Both fields, as he says, use the methods of comparison, and these areas are most effective when comparisons drawn are bold and apt. But he is wrong, I believe, when he claims that the artist and the political scientist consider some human conditions "too obscene" to depict; when he suggests that few political scientists are concerned with the aesthetic (e.g., elegance) of scientific work; when he describes writers like Dante, Shakespeare, Swift, and Stendahl as 'great political scientists"; and when he fails to acknowledge that, in political science, a major impulse to development is the self-same conflict of generations he associates with the evolution of art.

Silvert is no less provocative in his claim that the dominant major conceptual approaches of the discipline are "antithetical to area studies whose prime task is to establish the cultural parameters of social activity . . . [D] ata from foreign fields could not be absorbed by a discipline so fixed on a search for the common that it had no idea-frame for holding the different" (p. 157). In his analysis of works in political science (including one of mine) published in the 1960s, Silvert makes a strong case for his lament that few of the central concepts there used would go very far to help us understand the dynamics of Latin American politics.

Chalmers Johnson's essay is an attempt to show that the countries of East Asia may lend themselves to the kinds of case-study analyses suggested by Arend Lijphart. What Johnson has to say about the utility of this approach has been said before, and more succinctly, by my colleague, James Fesler. Clearly, case studies that help us to confirm or disconfirm theories, that deliberately use general knowledge to help us better to interpret a given case, that may serve to generate new hypotheses are useful. Even more useful, it might be added, is Eckstein's notion (elsewhere elucidated) of the "critical case" - that singular instance where a theory either must be verified or the theory's author must return to square one and start over again. But case studies can and should be executed anywhere; nothing about them intuitively argues in favor of a geographically circumscribed area encompassing many diverse nations.

The basic underlying argument of this symposium is not "area" versus something else; nor is it, surely, whether research in political science should be "behavioral" or something else. The argument is whether our so-called general theories, our concepts and typologies, our choice of research problems and cur styles of research — tied as they have been to very limited experiences in time and space — will serve as well in developing better comparative and, eventually, universal knowledge about politics. The answer is no.

Therefore (thankfully) more work must be done. The nice thing about several of these essays is that they tell us where, how, and why we have erred, and where we might now turn. Taken all together they should convince us once for all that what we are after is a better political science that produces additive knowledge based on biting propositions about the empirical political world, and the institutions and processes that characterize it. In the meantime, let us give the straw man the title of the book suggests the overdue burial he deserves

self-consciously.

JOSEPH LAPALOMBARA

Yale University

Between Existentialism and Marxism. By Jean-Paul Sartre. Translated by John Mathews. (New York: Pantheon Books, 1974. Pp. 302. \$10.00.)

Contrary to what the catchy title may promise, this book is an anthology collecting some of Sartre's essays and interviews dating from 1959 to 1970, and having little to do with either existentialism or Marxism. It is meant neither for neophytes unable to follow the tortuous logic of one of today's most profound thinkers, nor for scholars already familiar with these recurrent themes, but for those who fall somewhere in between: the educated consumers who have read or heard about Sartre and who would like to find out more without having to wade through his technical works. Thus, it will probably sell well, but add little. While it may have made sense to publish these essays in this form in France where Sartre is standard conceptual fare, their publication in English without even a summary introduction to contextualize and relate the various pieces will simply intensify the present confusion about contemporary French thought. Only Sartre's usual brilliance keeps this haphazardly assembled anthology from being a total intellectual disaster.

Structured as a kind of introduction to Sartre in 300 pages or less, the book is divided into five sections: Self-description, Politics, Philosophy/Poetry/Painting, Psychoanalysis, and Intellectuals. None of the five comes close to providing even a rudimentary account of Sartre's position. The one on psychoanalysis, for instance, merely reproduces Sartre's short introduction to a taped confrontation between a psychoanalyst and a patient who, tired and outraged by the paucity of tangible results after three years of psychoanalytic therapy, challenges the whole enterprise as a fraud. The other three parts in this section consist of the transcribed confrontation and two dissenting opinions by former editorial associates of Les Temps Modernes (where the whole section originally appeared). To anyone even vaguely acquainted with Sartre's views, it is immediately obvious why he would want to publish such a dubious document which, as his co-editors point out, could only have the heuristic functicn of initiating a debate concerning proble200) and the violent character of the doctorpatient relationship, no one not already familiar with the sado-masochistic dialectic of *Being and Nothingness* could possibly recognize this as anything but an amusing but trivial piece. And those familiar with Sartre's earlier works will be at a loss to find anything new in all this.

The same could be said of the section on Politics. It closes with a puzzling interview concerning organization where Sartre paints himself in a corner while being lectured by the interviewer. Conducted by Rossana Rossanda a member of the Manifesto group which had just split from the Italian Communist Party the interview searches in vain for the theoretical determinations of a new type of political organization that would avoid bureaucratization while at the same time performing the function of traditional Communist Parties. Again, this comes as no surprise to anyone who eight years earlier had read Sartre's elaborate explanation of the logic of group dynamics in his Critique of Dialectical Reason. But, on the other hand, no one can even begin to grasp this logic merely by reading the interview (which, contrary to what the book claims, was originally published in the U.S. in the fall of 1969 - afull year before it appeared in England).

The most inappropriate section, however, is the concluding one on "Intellectuals" which opens with a series of three lectures originally delivered in Japan in 1965. Well before the full development of Maoism and ultra-left alternatives to the French Communist Party, Sartre updates the classical Gramscian analysis which, after the XXth Congress, can only end up leaving intellectuals in a state of "unhappy consciousness" as the only viable political posture. This essay could not have been more foreign if it had been left in the original French. Not only is it historically obsolete within the French context - as Sartre himself acknowledges in the short introductory paragraph written for the French republication - but, given the rather different predicament of American intellectuals, it is also completely irrelevant in this country.

The presence of his by now classic essay on "Czechoslovakia: the Socialism that Came in from the Cold" ultimately saves the book. Here we have Sartre at his best. The philosopher, the writer, and the politician converge to produce probably the best essay written on the Czech situation after the Russian invasion. Originally

within an "imposed" socialism as oppressive as any fascism in recent times. His analysis forces a profound rethinking of all accepted notions and schemes in order "to produce new structures which will ensure that the next revolution does not give birth to that sort of socialism" (p. 117). It is regrettable that such a brilliant essay had to be buried within so much unrelated and irrelevant material which will confuse more than enlighten its intended audience.

PAUL PICCONE

Washington University

The Politics of Rights: Lawyers, Public Policy, and Political Change. By Stuart A. Scheingold. (New Haven, Conn.: Yale University Press, 1974. Pp. xiv, 224. \$10.00.)

Scheingold's book analyzes the usefulness of judicial decisions as a tool for achieving social change. By social change, Professor Scheingold means the redistribution of power. While he approaches the topic from the bias that law is valuable to the extent that it facilitates redistribution, the book is not a polemic. The author's analysis draws on the findings of others, augmented by hiw own insights, rather than being the product of new fieldwork.

The most useful insight of the analysis is the distinction made between what Scheingold calls the myth of rights and the politics of rights. He applies the former to the notion that once a court declares the existence of a right — for example, school desegregation — then realization of the right will naturally result. The compliance literature is used to show the flaws in this type of expectation. Scheingold goes on to criticize the heavy reliance on litigation, which the myth of rights emphasizes as the technique for producing change, on the grounds that it is costly, slow, and at times counterproductive.

Having rejected the utility of the myth of rights, the author suggests that court victories, rather than signaling the attainment of a right, can better be viewed as catalysts for political mobilization. Thus judicial recognition of a new right will alter how some people view a situation. Some "have-nots" may be emboldened by the decision to try to use the political process to translate the terms of the court order into reality. With the law on their side, a new self-confidence and sense of purpose may pro-

new coalitions; and in the long run, to a realignment of forces within the political arena" (p. 132). Judicial recognition of a new right may also prompt some of the "haves" and their allies in policy-making positions to re-evaluate programs.

Having indicated how court decisions can trigger the development of political pressures, Scheingold evaluates the likelihood that law will serve this function. He does this by exploring the socialization process of lawyers (in law'school) and the environment in which they practice. Scheingold correctly notes that the legal education process concentrates on imparting analytical skills and ignores or sweeps aside questions about the social consequences of decisions. The rationale that an attorney's responsibility is to represent the interests of his client as best he can and to ignore the public good is basic to the legal profession. The rule of precedent emphasized by the casebook method further conditions lawyers to accept the status

Despite the features of legal education which socialize students not to use their talents for reform, there are some whom Scheingold refers to as "activist lawyers." These range from lawyers who adopt a fairly conventional approach, using litigation on behalf of society's disadvantaged, to the "innovators" who "construct an integrated campaign comprised of legal, electoral, and other political strategies..." (p. 183). Thus the innovative activists, like Ralph Nader, are ones who rather than putting their faith in the myth of rights use the law to mobilize public opinion or to orchestrate group pressures.

Scheingold's analysis leads him to a basically negative conclusion. He sees little likelihood that even as a stimulus for mobilization courtmade law can produce social change, although it is useful in setting policy objectives. Unanswered is whether other avenues of lawmaking such as legislation or rule making in the executive branch hold out greater hope. The implication is that they do, but the success of litigation relative to other techniques receives little attention. Nor does the author fully consider the proposition that courts may serve a special policy function to the extent that they may legitimize demands which have little popular support and which therefore would be unattainable in other arenas.

The Politics of Rights is marred in places by organizational problems. Although the author carefully sets forth the topics to be covered in each chapter, he sometimes digresses and pursues tangential matters. Another difficulty is the failure to define some of the terms used.

Nonetheless the book is useful in prompting a rethinking of the role which law does and can play in policy change.

CHARLES S. BULLOCK, III

University of Houston

Where Have All the Robots Gone: Worker Dissatisfaction in the '70s. By Harold L. Sheppard and Neal Q. Herrick. (New York: The Free Press, 1972. Pp. 222. \$7.95.)

During the late 1960s and early 1970s, such disparate groups as middle-class college students, black ghetto residents and white union workers were venting acute dissatisfaction with established leaders, policies, or institutions. This book, written by two labor economists, deals with one of these groups, the white blue-collar worker, whose alleged "blues" became a favorite theme for media journalists, governmental task groups, and sociological researchers.

The point of departure of this book, as in much of the worker-alienation literature, is the premise that "more and more workers... are becoming disenchanted with the boring, repetitive tasks set by a merciless assembly line or by bureaucracy" (p. xi). This dissatisfaction, the authors contend, is particularly characteristic of the younger generation of blue-collar workers, who are less willing to accept the authoritarian disciplines of the work place and who vent their discontent not only in the work arena but in their voting behavior and other ways.

The major portion of the book reports the results of interview surveys of about 400 white male blue-collar union workers, conducted in a few communities in Pennsylvania and Michigan in 1970 and 1971. About 100 of these workers were identified as having the "blues," based on responses to questions concerning their job satisfaction and perceived advancement opportunities. Most of the resulting analysis consists of simple, bivariate comparisons between these disaffected workers and the remainder of the sample. The workers with the blues were found to be older than other workers; they rated their job tasks as more boring and were generally more alienated from society than the rest of the sample. Among this group, the younger workers appeared more dissatisfied with the nature of their job, and with lack of advancement opportunities, while older workers (over 30) were most unhappy about their pay. Of special interest to the political scientist is the fact that the younger discontented white blue-collar workers in this sample were also more dissatisfied with the existing political establishment and voted disproportionately for Governor Wallace in the 1968 presidential election.

This book is written in simple nontechnical style and contains some provocative insights. From a scholarly standpoint, its results are, however, a very slender peg on which to hang the generalizations and inferences made through this book. No information is provided the reader as to how the workers included in this survey were selected. Statistical tests of significance are rarely cited, and no attempt is made at any systematic use of available techniques of multivariate analysis.

More fundamentally, the authors' conclusions that worker alienation is increasing and that "restructuring of work" is the primary solution to the blue-collar blues appear inconsistent with the results of more systematic national surveys which indicate: (1) that there has been no consistent national trend in worker dissatisfaction during the past fifteen years, and (2) that blue-collar workers, unlike white-collar workers, are most interested in the financial rewards of their jobs rather than job challenge. (See, particularly, Robert P. Quinn, Graham L. Staines, and Margaret R. McCullough, Job Satisfaction: Is There a Trend [U.S. Department of Labor, Manpower Research Monograph No. 30, 1973].)

HAROLD WOOL

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Games for Society, Business, and War: Towards a Theory of Gaming. By Martin Shubik. (New York: Elsevier Scientific Publishing Co., 1975. Pp. 371. \$16.50.)

This book is a delight; its reading yields insight and much pleasure. Shubik surveys gaming, from its use as entertainment through its employment in social science research. Shubik clarifies distinctions between simulation which "involves the representation of a system or organism by another system or organism which purports to have a relevant behavioral similarity to the original system" (p. 12); gaming, which "in contrast to simulation, necessarily employs human beings in some role, actual or simulated in its operation" (p. 13); and game theory, a mathematical "language for the description of conscious decision-making processes involving more than one individual (p. 14). Shubik lays foundations for the development of a theory of gaming, in which analytic (especially those from game-theoretic enterprises) and behavioral (including those from psychology, sociology, economics, and politics) models may be combined for purposes of teaching-and-training, operational gaming, and experimentation. An elegant, elementary exhibition of the analytics is presented in Part II of the volume, "A Game Theory Background for Gaming" (pp. 27-153), with "exercises" at the end of each of the five chapters therein.

For those political scientists interested in placing the earlier descriptions of simulations in political science given by Wayne Francis and Paul Smoker in Guetzkow, Kotler, and Schultz (eds.), Simulation in Social and Administrative Science(Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972 [pp. 245–363]) in a gaming context, this book provides an enlargement of horizons, especially in two of its four chapters on Some Specific Games and Their Uses, namely Chapter 13 on "Military Models, Simulations, and Games" (pp. 279–294) and Chapter 14 on "Political Science, Urban Development and Other Uses" (pp. 295–308).

As one reads the book, one almost hears Martin Shubik talking - he's the very embodiment of an Oxford don. His thought flows fluently. He's given to intriguing challenge, as when he argues: "Perhaps . . . in any application of gaming from kindergarten teaching to major war games success will depend upon the amount learned by the teachers and designers as well as the students and players" (p. 24), and as he later warns: "Problems, such as the meaning and measurement of national interests or the depth of ideology, are well worth studying using a gaming context, just so long as the teacher or experimenter does not confuse his limited tools and special models with the many other levels of reality and creative fiction" (p. 301).

Over the years Shubik has made substantial contributions to the theory of games, as his forthcoming volume with his long-time colleague, L. S. Shapley - Competition, Welfare, and the Theory of Games - will indicate. Most of us also know their essay, "A Method for Evaluating the Distribution of Power in a Committee System," which appeared in the 1954 volume of this journal (APSR, 49 [1954], 787-792). Shubik's edited volume on Game Theory and Related Approaches to Social Behavior (New York: Wiley, 1964) was a worthy precursor of the exciting book in hand. Although Shubik is an economist, he's certainly as broad in his interests and competencies in the social sciences as Kenneth Boulding, even though he may be less well known to political scientists.

Shubik is not only an interdisciplinarian, he is a cosmopolitan, as his researches in an amazing number of places in the world testify. He has conducted experiments with colleagues

of other cultures on at least five of the six continents. He is part of the creative complex at Yale which provides the intellectual foundations from which Yale's new graduate School of Organization and Management is being built. The breadth and scope of Shubik's performance in the book matches his outstanding credentials for having been its author—and enables one to forgive quite easily occasional shades of oracularity, as he shares wisdom with his reader.

Be forewarned: Shubik's subtitle, "Towards a Theory of Gaming" is accurate. As he reiterates at the end of his book: "The different disciplines of the behavioral sciences, together with game theory and institutional studies, are all required in the modeling activities of game construction. In particular, the game models of the theory of games provide the rationalistic benchmarks against which the structure of any game can be compared" (p. 331). Shubik struggles toward theoretical formulations, especially in Part III (pp. 155-199); in my judgment, however, he is far from achieving a theory of gaming. Those concerned with the interrelations of game theory and gaming will find his passage on "A Contrasting of Game Theory Assumptions and Behavioral Assumptions" and his subsequent query, "What is a Solution in Gaming?" (pp. 157-170) seminal. Many of us applauded when Bruce Russett, Marguerite Kramer, and Anatol Rapoport of the Journal of Conflict Resolution decided they would "no longer devote a specially identified section [of their journal] to 'Gaming'" (Journal of Conflict Resolution, 17 [September, 1973], 379). Perhaps a more intimate development of game theory with behavioral substance will accelerate our ability to move more rapidly "towards a theory of gaming."

It is not surprising, given the sweep of Shubik's interests, that not all references are cited or that on occasion the author has failed to update himself. But it was disappointing that the provocative work of Duke, Kotler, and Naylor was not considered worthy of more than mere mentions. Shubik produces a few excellent summaries, as in Table 18 on "Games with More than Three Players" (pp. 270-271) and as in the final section of Chapter 11, on "Some Specific Games and their Uses: Experimental Social Psychology and Game Theory" (pp. 276-278). One regrets that Shubik did not prepare similar condensations for the other parts of his complex book, so that all could see the forests for the trees, even if the book seems not intended as a text per se. Shubik inspires the reader by virtue of his visions for the "Yale Gaming System" (pp. 236-242), involving a computerized system for gaming experimentation. It is sad he didn't create a blueprint with some detail for collaboration to meet the needs for "Joint Usage Among Different Disciplines ... in Teaching and Research ... Among Different Institutions ... via Common Programs and Time Sharing" for work in simulations and operational gaming (pp. 312-318).

Some of us feel bewildered by these new developments, often confusing gaming with the theory of games and are having difficulty in understanding the differences many draw between gaming and simulation. Here's the chance to learn and enjoy!

HAROLD GUETZKOW

Northwestern University

Life and Liberty: A Return to First Principles. By Frederick Martin Stern. (New York: Thomas Y. Crowell, 1975. Pp. 212. \$7.95.)

Because the author of this volume is apparently not an academic and perhaps not known to many readers of this journal, it is in order to say a little about him. Frederick Martin Stern was educated in German universities, is a doctor of law, was an executive in industry in pre-Hitler Germany, and is now an American citizen. He has written three other books, none of which I've read: Capitalism in America: A Classless Society (1951), The Citizen Army: Key to Defence in the Atomic Age (1957), and The Junker Menace (1945).

In this book, as its title indicates, Dr. Stern seeks to show that there is a natural, vital connection between democracy, the social system based on "liberty with equality," and the life process itself. We are told that the principle on which the organic world operates from the cellular level upward, competitive yet cooperative diversification, offers the model on which human society in all aspects should fashion itself.

On this theme the book's deficiencies are serious. Treatment of the biological/social thread is superficial and dogmatic. Sustained argument and illuminating analysis is lacking. Sizable leaps are made from basic yet sketchy biological conceptions to alleged implications for human social life. The claim is asserted and reasserted without apparent appreciation of its many genuine problems.

The proposition that the organic side of the universe in some sense entails cooperative, deliberative democratic forms may be true, although not so long ago the same grounds were claimed to lead to raw social Darwinism. Nonetheless, it may be that something basic to our nature as social organisms is best served by

a democratic system. But little said here renders that complex proposition or the logic of its consequences clear, much less convincing. Much more would have to be presented in order to connect that level of the organic world coherently to human society, democratic or non-democratic.

The complementary thesis of the book is a defense of the U.S. version of liberal pluralism which, Dr. Stern tries to argue, can with reasonable effort be turned more in the direction of a flexibly organic community. To the standard features of liberal pluralism (countervailing, separated power at all levels, individualism, civil liberties, equal opportunity, and regulated competition), Dr. Stern adds those of considerable central political control and features of the welfare state. Thus "liberty with equality."

That concoction forms an unstable but familiar mix. With fair accuracy it does describe the U.S. and, barring the gloomier possibilities, denotes the probable terms and measures on and with which the U.S. will have to hammer out at least its near future. The author's treatment of a broad range of U.S. practices and problems is generally sound. Most of his suggestions for improvement are sensible, straightforward, and welcome for themselves. They are capable of implementation with the nation's currently available spiritual and material resources.

But it all has very little to do with an organic community, for what Dr. Stern defends, even after we include his improvements, remains essentially plural, competitive, unstable, secular, and dynamic. Those qualities simply are not the central features of, or even compatible with the central features of, any description or instance of an organic community with which I am familiar. Let me be clear. What Dr. Stern sees in contemporary U.S. society is there to be seen. It has genuine virtues, attractions, and achievements. Thus what he defends and the manner of defense has its own considerable merit. But it is quite another thing to claim that mainly by extending the principles of liberty and equality within U.S. industrial, plural society we are offered even the beginnings of an organic community. Indeed, if Plato, Burke, and F. H. Bradley are cogent defenders of the organic community, then we would have to take a direction counter to that proposed by Dr. Stern, to restrict rather than to extend liberty and equality. Much more than liberty and equality are necessary to an organic society, for such a society depends on deeply unifying bonds and the shaping power of stable, embracing institutions. Role and station, hierarchy, a deeply binding sense of membership in at least one's "little platoon," habit, custom, ritual, tradition reverenced for itself - all these are standard features in the organic model, to say nothing of the anti-individualistic conception of the human soul which is integral to the organic view. Yet all those features are, to say the least, paradoxical in a pluralist nation, committed to equality and individualism, still wedded to progress, "the possible," and to rapid social change at every level. We probably can do and obviously should do better with what we have and what we are. Dr. Stern's recommendations on these matters are to the point. But if he or we are serious about a more organic community, then the level of analysis and understanding must be much deeper.

It should be noted that this book could profitably serve as a text in some college courses, because it has a thesis, and its ambitions are large and unfulfilled. Thus it would provide a springboard for a more incisive and illuminating analysis of the central problems it raises. It has other virtues also. It moves at a crisp pace over a broad range of social, economic, and political concerns. Its recommendations are meliorative. Its tone is modest and the author's good will is evident everywhere. It is addressed to an important audience: the mature, reasonably well-informed, reasonably open-minded citizens who take thought for the institutions and practices of the United States, and who care about its future. That audience deserves to have its mind taken seriously. Dr. Stern does so, expressing quiet confidence, even good cheer, concerning the nation and how, short of revolution, it can be improved. Those are not minor virtues these days, when we have heard so much that is shrill, impatient and pretentious, cynical or despairing. Despite its serious deficiencies and limitations, then, this is a welcome and useful book.

R. J. ROWAN

University of British Columbia

Handbook of Leadership: A Survey of Theory and Research. By Ralph M. Stogdill. (New York: The Free Press, 1974. Pp. 613. \$19.95.)

Since the start of the twentieth century, the subject of leadership has occupied the attention of many social psychologists. To prepare the *Handbook of Leadership*, Ralph Stogdill, one of the major figures in this research tradition, reviewed more than 5,000 empirical and theoretical contributions. His product is awesome: a

total of eight major sections and forty-one chapters summarize the results of the effort. For the person considering whether to read this book, the section headings should be of interest. They are:

- (1) Leadership Theory
- (2) Leader Personality and Behavior
- (3) Leadership Stability and Change
- (4) Emergence of the Leadership Role
- (5) Leadership and Social Power
- (6) Leader Follower Interactions
- (7) Leadership and Group Performance
- (8) Conclusions

The essential product of this work is a series of inductively derived empirical generalizations. The more a given relationship among variables was studied and replicated, the more confidence Stogdill shows in the findings. I have found no book or article that even closely approximates the comprehensiveness of this work. Indeed, anyone who wanted to determine what social psychologists had contributed to knowledge about leadership would be foolish to ignore this book.

Despite the fact that this is a "handbook," it is written to be read — even from cover to cover. The writer clearly made an effort to help the reader by preparing short chapters, structuring readable sentences, and offering frequent summaries. Because it is so well organized the book should also be extremely valuable for anyone who wished to carry out his/her own literature search in the area of leadership up to 1970.

Any effort to comprehend and order an ordinarily large body of research is to some extent an exercise in projection. No individual can retain a purely inductive stance toward the literature, even if he or she so desires. Sooner or later personal biases influence the way the apparently objective results are reported. Stogdill is no exception.

One bias that Stogdill shows is toward rejection of data and theory about the complex emotional dynamics of leadership. He explicitly omits a discussion of the charismatic leader because "this important variant of the leadership role has not been a willing or frequent subject of research that involves measurement or experimentation" (p. viii). He indicates that the numerous biographical studies of charismatic leaders add little to the understanding of leadership.

Professor Stogdill's approach to topics which have had a large number of studies is to tabulate and count the number of times a given result has been obtained. He does this several times throughout the handbook, the first one pertaining to the identification of leadership traits. Covering all empirical studies from 1904–1970, he finds the two traits most frequently associated with leadership to be sociability-interpersonal skills (49 positive findings, no negative results) and intelligence (48 positive findings, 10 negative results). Summarizing the results of the research on leadership traits, Stogdill does not conclude that interpersonal competence is the trait most often associated with leadership, although his own literature search method should lead him to this conclusion. His review of the literature on leadership training shows a similar bias.

To the extent that Stogdill reaches a single conclusion, leadership behavior seems to him to consist primarily in "structuring expectations in the single pattern that contributes positively to group productivity, cohesiveness and satisfaction. This pattern of behavior is perhaps the central factor in leadership since it is intimately associated with the definition of leadership... philosophies of leadership and training methods that undermine this factor destroy the very foundation of leadership" (p. 419).

In the growing rapprochement between political science and social psychology, the subject of leadership is definitely an area of common interest. The political scientist who wants a well-written summary, with interpretations and conclusions, of what social psychologists have done with this area until 1970 will find none better than Stogdill's Handbook. It has much to offer students of public administration whose concerns pertain to leadership within relatively bounded human organizations. It will be less attractive to those who have attempted to analyze leadership relations by more idiographic methods or who have been most concerned with the political behavior of leaders who have regular face-to-face interaction with a very small proportion of the individuals whose lives they influence.

CLAYTON P. ALDERFER

Yale University

The Formation of the New Left: The Early Years. By George R. Vickers. (Lexington, Mass.: Lexington Books [D.C. Heath], 1975. Pp. 166. \$14.00.)

This lucid work is important, not because it narrates the early history of the American New Left (something which has already been done), but because it offers a mature social science explanation of the New Left's emergence and contributes to concept formation in empirical theory. George Vickers, a staff sociologist for

the Russell Sage Foundation, avoids feckless speculation, rests his argument on public evidence (including archival documents and empirical findings), owns up to speculation where evidence is inconclusive, and recognizes that his study is but a first step toward understanding the New Left.

The author was, himself, "deeply involved" in the New Left - including Students for a Democratic Society - and the book was originally intended as a defense of the New Left which would show how the actions of the activists were "a rational response to 'reality' as they perceived it, rather than a response to some deep, unconscious psychological urges" (pp. ix-x). Vickers criticizes the existing psychological accounts of the New Left centering around the theme of "generational conflict" - in particular, the generation gap views of Lewis S. Feuer, and the nongeneration gap views of Kenneth Keniston and Richard Flacks (which stress intraclass continuity between parents and children). Although Vickers ultimately constructs a neo-Marxist, sociological explanation, I find his views to be quite compatible with those of Keniston and Flacks to the extent that they all emphasize the vital importance of the (upper-) middle-class family socialization experiences of the early activists. Even though he ends up with less than the apology he set out to write, Vickers avoids psychosociological reductionism by employing a neo-Marxist "dialectical" or "relational" approach which joins the "self-active" creation or praxis of the actors to the conditions that structure their action.

Focusing on class culture, Vickers emphasizes the post-World War II differential in child-rearing practices separating middle-class parents (especially those relatively well-educated and holding bureaucratic jobs) from working-class parents. Because these middleclass parents worked not with things, but with data or people in relatively unsupervised fashion, they experienced "self-direction," hence valued it, and then taught it to their children, whereas working-class parents experienced, valued, and taught compliance with external authority. Vickers then analyzes the changing class configuration of postwar America in terms of the increasing role of knowledge in the functioning and growth of the economy especially in government and corporate bureaucracies. "Not only did 'white-collar' and 'service' occupations grow relative to 'blue-collar' and 'farm' occupations, but . . . in white-collar occupations the proportion of 'managers, officials, and proprietors and sales-workers declined, while the proportion of clerical workers and 'professional, technical, and kindred' workers sharply increased" (p. 121). Generally the dynamic sector of the middle-class practicing "permissive" child-rearing spawned the New Left activists — who, in turn, did the bidding of their parents, who were interested in promoting new cultural standards congenial to themselves.

The growth in the role of knowledge required the growth, professionalization, and bureaucratization of universities, and their increasing interpenetration with government and business. But just as future university students (coming, as usual, from middle-class homes) were learning from their parents how, so to speak, to "do their own thing," the universities were becoming highly structured and competitive training grounds for the occupational elite. But many students were psychologically unprepared for survival in those universities — which required conformity to authority and habituation to deferment of gratification. With a sharp neo-Marxist eye for contradictions, Vickers states: "The contradiction between the values and relations within the family, and those within the university, led to discontent among the children from the most advanced layers of the bureaucratic, white-collar strata, which took the form of political and cultural opposition based on self-directed values" (p. 124).

Vickers concludes that the New Left was not really an anticapitalist movement — revolutionary rhetoric notwithstanding:

What the New Left represented, objectively, in this historical process was an attempt to rationalize institutional relationships and cultural organization, to bring them in line with the changes in the social relations of production. The attempt to develop a political theory and practice based on ... "self-directed" values ... was aimed, in this sense, at institutionalizing those values, interests, and beliefs fostered by the working conditions of educated white-collar workers (pp. 133–134; italics in original).

After some fifteen years of New Left activism, the institution of private property remains intact. We have not achieved socialism, but rather the legitimization of marijuana, pornography, abortion, homosexuality, unisex, and nonmonogamous life-styles. The New Left enabled the new managerial bourgeoisie (sometimes called the "New Class") to make a cultural revolution against the old entrepreneurial bourgeoisie, thus giving it a cultural status to match its economic standing. The political New Left may have evaporated, but the counter-culture has successfully permeated American culture. As a result, American capital-

ism has advanced by resolving one of its more recent internal contradictions.

DALE VREE

Stanford University

place to begin any research on the use or development of social indicators.

MARIAN LIEF PALLEY

University of Delaware

Social Indicators and Societal Monitoring: An Annotated Bibliography. By Leslie D. Wilcox, Ralph M. Brooks, George M. Beal, and Gerald E. Klongan. (San Francisco: Jossey-Bass Inc., 1972. Pp. 464. \$12.50.)

The United States Department of Health, Education, and Welfare in its 1969 publication, Toward a Social Report, defined a social indicator as "a statistic of direct normative interest which facilitates concise, comprehensive and balanced judgments about the condition of major aspects of society..." A concern with the development of such systematic data has been the guiding force behind the development of the contemporary social indicator movement. In the words of the authors of this volume, this movement is concerned with "generating social indicators to serve as tools to monitor social change..." (p. 1).

The contemporary social indicator movement arose in the late 1950s in response to concerns by both policy analysts and policy makers that insufficient systematic social data were available to provide adequate intelligence for rational policy and program development and planning. The literature which has evolved concerning various aspects of social indicators - definitions, development, theory, application, policy and planning, and concepts - has been substantial. It has been interdisciplinary in scope and has appeared in a wide range of journals and books. Given this dispersion of scholarly materials, an annotated bibliography such as Social Indicators and Societal Monitoring serves a very useful function.

The bibliographic compilation in this volume has been indexed in four separate categories which facilitate finding material: the Annotated Index, the Author Index, the Key-Word Subject Index, and the Address Index. In addition to these indexes the book contains an introductory essay by Ralph Brooks entitled "Social Planning and Societal Planning" in which he has outlined the stages in the development of the social indicator movement, current issues, and current activities of scholars of the movement. He has brought together in his presentation many of the most central concerns of the individuals involved in the development and the use of social indicators. The bibliographic listings also provide interested individuals a good

From Radicalism to Socialism: Men and Ideas in the Formation of Fabian Socialist Doctrines, 1881–1889. By Willard Wolfe. (New Haven, Conn.: Yale University Press, 1975. Pp. 333. \$17.50.)

Wolfe's book traces the intellectual origins of Fabian socialism up to the moment when that doctrine took definite shape in the Fabian Essays of 1889. He is particularly concerned with the relationship between Fabianism and the radical wing of English liberalism, seeking to show that Fabianism (and indeed English socialism in general) continued to bear a strong radical imprint. The book is scholarly without being dull, and Wolfe is to be complimented on the lucidity and control with which he presents the ideas of his protagonists.

Part I presents the Radical background to Fabian theory, with particular emphasis on J. S. Mill and the land reform movement. The discussion of Mill's socialist sympathies is careful and balanced, though Wolfe somewhat overrates Mill's direct influence on the Fabians by not fully appreciating the contrast between his version of socialism and theirs. Wolfe is on firmer ground when he traces indirect influences via the positivist movement, and via the land reformers, with whom Mill was associated in his last years. The land reform movement was quite crucial in the transition from radicalism to socialism, since the argument that landowners were exploiting the rest of society from their position of monopoly was quintessentially radical, and yet it could be extended by the Fabians to apply to capitalists as well.

Part II contains intellectual biographies of several leading Fabians, and attempts to fill in the intermediate stages which led them from orthodox radicalism to socialism. Shaw went through a phase of extreme libertarianism in the mid-1880s before entering the Fabian fold. With Sidney Webb the path lay through Comtean positivism; with others it was spiritualism or Christian socialism. The variety of the beliefs which eventually converged in the sober doctrines of Fabianism is striking, and Wolfe's careful and well-documented analysis of this intellectual potpourri could scarcely be bettered.

The biographies are used to support some general theses about Fabian socialism. Noting that the origins of Fabianism lay in religion or in substitute religions like positivism, Wolfe argues that the movement itself was a "quasi-religious phenomenon." The Fabians, he maintains, were basically Evangelicals who interpreted their moral duties in social rather than individual terms, and whose deepest objection to capitalism was that it encouraged the selfishness and immorality of capitalist and worker alike. Finally the Society's economic collectivism was a relatively late addition to its philosophy, which never succeeded in supplanting the central ethical and religious elements.

This produces a picture of the Fabians which contrasts sharply with the more usual image of a group dedicated to social planning based on the exhaustive compilation of empirical facts. But how far is Wolfe's picture simply a result of his selection of material? His studies are confined to the period before the group had established its collective identity, when the individual members were more inclined to indulge their private enthusiasms. The discipline of the Society produced some major changes in attitude. If Shaw was a near-anarchist in 1885, he was anything but an anarchist by the 1890s; and Webb's positivist evangelism gave way to the sober historical research which produced those monumental studies of trade unionism, cooperation, etc. that came to typify the Fabian approach to social and political questions. One has to ask how relevant Wolfe's study is to the understanding of the mature Fabianism which exerted a real influence on the labor movement in Britain.

The book's focus of interest also leads Wolfe to neglect the authoritarian and antiliberal aspects of Fabianism. "The perfect and fitting development of each individual," wrote Webb in his Fabian Essay, "is not necessarily the utmost and highest cultivation of his own personality, but the filling, in the best possible way, of his humble function in the great social machine." By 1889 the Fabians had come a long way from the liberalism of Mill.

Fabianism cannot be identified with socialism as such. In its emphasis on centralized planning and on the role of the expert, it differed greatly both from Mill's idea of cooperative production within a market framework and from the communism of Marx and the anarchists. It appealed to a small group of middle-class and lower-middle-class professionals, to whom it offered a worldview which accurately reflected their social position. For that reason it has to be separated both from bourgeois liberalism and from the inchoate radicalism of the working class — with which, however, it was later able to form an uneasy alliance. But if Wolfe's book fails to tell the

whole truth about Fabianism, it is nonetheless an invaluable guide to the intricate tangle of conflicting beliefs which led to its formation.

DAVID MILLER

University of East Anglia, England

Justices and Presidents: A Political History of Appointments to the Supreme Court. By Henry J. Abraham. (New York: Oxford University Press, 1974. Pp. 310. \$10.00.)

Who were the one hundred individuals who have served on the U.S. Supreme Court, and how did they get there? Who were the nominees that did not make it to the bench? What considerations came to play in the presidential selection and senatorial confirmation processes? To what extent did the appointees who served on the Court live up to the expectations of their nominators? These are the questions that Henry Abraham attempts to answer in his "historical political account" that he correctly notes is also "a mini-history of the Court itself" (p. viii). Professor Abraham begins with "prefatory remarks" devoted to the Nixon nominations and then has a chapter that in general spans the different methods of judicial selection and the selection process for Supreme Court justices. This is followed by a chapter on the qualifications or backgrounds of Supreme Court appointees which takes us to the heart of the book, the six chapters that survey all Court nominations beginning with those of George Washington and concluding with Lyndon Johnson's. A brief epilogue completes the book.

To observe that Justices and Presidents is an ambitious undertaking is to state the obvious. It would be unfair to expect so relatively short a book to be able to do for each Court appointment what took David Danelski an entire book (and a splendid one at that) for one such appointment (Danelski's A Supreme Court Justice is Appointed [New York: Random House, 1964]). Rather Professor Abraham presents a broad historical review of the Supreme Court with capsule biographies of the justices, a general audience-oriented version of the politics of their appointments, and brief subjective evaluations of their performance on the bench based in part on the Mersky-Blaustein survey of legal scholars' ratings of the justices. Students should find the book enlightening particularly concerning the less well-known nineteenth- and early twentieth-century Court and justices. Specialists should find Abraham's work highly suggestive, at least implicitly, of hypotheses about the individual Court appointments.

Essentially the book tries to tell us about the nature of judicial recruitment by providing a perspective drawn from the broad sweep of history. I would have preferred, however, a more explicitly theoretical analysis of recruitment. It would also have been helpful had the book addressed itself more systematically and empirically to questions of biases in judicial recruitment with more of a focus on those who did not make it to the nomination stage, the nature and extent of transactions surrounding appointments, and the impact of the individual appointments on the course of Supreme Court policy making (which would require a more precise and objective accounting of judicial behavior than that given in the book).

Taken on its own terms, however, one can note that the more famous and contemporary justices receive more substantial coverage than do the more obscure nineteenth-century justices who are treated in review-book style. For example, the background and politics of appointment of Justice John McKinley is disposed of in little more than a hundred words followed , by the observation: "He [McKinley] would not disappoint his nominator [Van Buren] during his fifteen years on the high bench" (p. 95). The only other mention of Justice McKinley occurs seven pages later: "In the summer of 1852 Mr. Justice John McKinley died after fifteen colorless years on the bench - during which time, because of illness, he was as much absent as he was present" (p. 102).

These quotes highlight another distinguishing characteristic of the book, its highly subjective nature. In many ways the book can be considered a personal interpretation of American history and the Supreme Court. For example, in addition to the speculation about how presidents must have viewed their appointees' performance on the bench, we are told without the presentation of supporting evidence that Earl Warren "was an influential participant in President Kennedy's decision to nominate Secretary of Labor Arthur J. Goldberg" (p. 22); that had Nixon nominated Senator Byrd, "there is little doubt that his colleagues would have given their approval" (p. 40); that "Daniels' record on the Court amply justified Van Buren's faith in him: it proved to be as nondeviationist as McLean's was the opposite" (p. 96); that before Justice McKenna went to the Court he was a Ninth Circuit appeals judge where "he served five undistinguished or even incompetent years" (p. 143); that Senate rejection of Judge Parker "is now all but universally regarded not only as regrettable but as a blunder" (p. 189); that when Justice Tom Clark resigned to enable son

Ramsey to become Attorney General "the country deeply regretted his self-imposed resignation" (p. 230). (Was I the lone oddball who cheered?)

On balance, I should stress that I enjoyed reading Professor Abraham's work and that even if the book tries to do too much and is perhaps too subjective, it after all does present the considered views of an eminent political scientist and leading scholar of constitutional law. That in itself should commend Justices and Presidents to scholars and students.

SHELDON GOLDMAN

University of Massachusetts-Amherst

Political Money: A Strategy for Campaign Financing in America. By David W. Adamany and George E. Agree. (Baltimore and London: The Johns Hopkins University Press, 1975. Pp. 221. \$11.95.)

The Twentieth Century Fund sponsored the research project that led David W. Adamany and George E. Agree to their examination of campaign finance in *Political Money*. The project brought together an academic and a political practitioner — the former an academic with considerable "real-life" political experience (Adamany is formerly State Secretary of Revenue in Wisconsin), the latter a fund raiser who has been active in national politics for two decades. While the idea of such a mix is exciting, the results in this book are something less so.

The authors brought to their task a disillusionment with existing methods of financing political campaigns and a belief that some sort of public finance was a necessary response. From this common ground, Adamany and Agree have produced a well-written contribution to the literature on political finance. Their book is a useful compendium of past campaign spending practices and patterns and a helpful background to understanding the development of current legislation in the field. It is also a pioneering attempt to design a program of government funding in the context of America's candidate-centered culture, a difficult task if candidate independence is not to be further enhanced and the party system is to be protected.

The authors' preferred method of financing U.S. political campaigns — and the major argument of the book — is public financing through a voucher system in general elections, with matching grants in the primaries, and stringent limits on campaign contributions. Vouchers have long been espoused by Agree; he worked with Senator Lee Metcalf on the 1967

proposal for the voucher system of financing presidential campaigns. The plan outlined in *Politicial Money* would involve as a first step a universal voter registration for federal elections, followed by distribution of presidential, Senate and House election vouchers to each registered person eligible to vote in such elections. Vouchers would be imprinted with the voter's name and address, and accompanied by explanatory literature, candidates' statements, and postage-paid envelopes for mailing the signed vouchers to chosen candidates. Vouchered funds would then be disbursed by the Federal Election Commission as payment for certified candidate expenditures.

While the goal of the voucher system, to permit citizen designation of government funds, is worthy, the system is cumbersome because of the different types of vouchers to be accounted for - the vouchers become a kind of "funny money," produced, distributed, and tracked at considerable expense. To encourage direct citizen participation, one questions why it would not be more feasible to take the process a step further - such as to a matching grant system as proposed for the prenomination period, or to a 100 per cent tax credit program within certain limits which would achieve the same goal at far less administrative expense than the voucher system (whose cost Adamany and Agree estimate at \$50 million). It is not enough to legislate tax or matching incentives, or vouchers; action to improve solicitation and collection systems is essential to make them work, and the book's discussion of this is limited.

There are several areas in which the authors are unfair, or perhaps "too quick" in their negative judgments. One relates to disclosure laws. While laws requiring reporting of receipts and disbursements have been on the books for decades, disclosure at the federal level only became comprehensive and effective on April 7. 1972 – a date that will live in political infamy - with the implementation of the Federal Election Campaign Act of 1971. Coming midway through the 1972 campaigns (the Finance Committee to Re-Elect the President had already raised \$20 million before the cutoff date), the law provided only partial opportunity to judge the effectiveness of disclosure with strict reporting requirements. While disclosure would seem not to have changed many presidential votes by November, it brought evidence of financial malpractice (the laundered Mexican checks of a predisclosure contribution) that helped begin the unraveling of Watergate.

Nor are we able to fully test the effective-

ness of disclosure as a deterrent to political abuse at the federal level by experience in the 1976 elections. The limitations on contributions imposed by the 1974 Amendments to the election finance statutes make disclosure of contributors less interesting. The amount that an individual may now legally contribute may not be large enough to make him worry about hiding the fact he gave it, although disclosure of amounts given is still crucial for receiving matching incentive payments in government funding programs. Until there has been greater opportunity than we have had to date to measure the effectiveness of disclosure, it seems premature to dismiss it as readily as the authors do.

Adamany and Agree are excessively harsh also in their judgment on the effectiveness of tax incentives - particularly the system of tax credits whereby a contributor may directly reduce the amount of tax he owes by one-half of his campaign gift up to \$50 (\$100 on joint returns). Again, the quarrel is with the amount of data the authors have on which to base a judgment. Tax credits as an incentive to political gifts have been available at the federal level only since 1972, and were initially at half the \$50/\$100 figure which Congress set in 1974. Tax credits were available in some states before 1972, but the main form of tax relief for the political contributor offered by the states has been a tax deduction based on relatively low income tax rates. The deduction is less equitable in that it is governed by an individual's tax bracket. For the authors to claim that tax incentives have "failed entirely" is a judgment passed before these incentives have really been given an opportunity.

Despite these misgivings, the book is well worth reading and pondering. More blueprints of programs for government funding are likely to be necessary before an acceptable and constitutional system of government assistance can be developed for American politics.

HERBERT E. ALEXANDER

Citizens' Research Foundation, Princeton, N.J.

The Unequal Elites. By Robert P. Althauser and Sydney S. Spivack, in collaboration with Beverly M. Amsel. (New York: John Wiley and Sons, 1975. Pp. 204. \$12.50.)

This concise, well-organized, and carefully researched and written volume should be of particular interest to quantitatively oriented scholars of social and economic inequality between the races; and generally, its findings

should be of interest to all social scientists. The authors' (Sidney S. Spivack directed the study but died before the writing phase) main objective is to determine whether black and white college graduates of equal resources will receive an equal share of rewards in society's market-place, thus the title, *The Unequal Elites*.

The authors compare closely matched (on family social class, quality of education and grade-point average) samples of both black and white male college graduates. In total more than 800 blacks and whites who graduated between 1932 and 1964 were interviewed in 1967 and 1968.

The research indicates that generally the parents of black college graduates had attained higher education levels than had the parents of identically matched white graduates, though the parents of these black graduates held occupations with less, or only equal status. Similarly, the black graduates themselves obtained more postgraduate education than their white counterparts, although their current occupational status is only equal, and their median incomes are lower than the white median incomes by two to three thousand dollars. Thus the inequality experienced by these black elites is multifaceted. Not only have these black graduates achieved more educationally than their white counterparts, but in terms of their occupational status they have not received more for their efforts, and in terms of their income they have been rewarded less. Finally, these black elites report less satisfaction with their occupational positions and general living situations than do the white elites.

The weight of evidence clearly points toward racial discrimination in society's marketplace. As the authors themselves acknowledge (p. 168), however, possible need for achievement differences between these male black and white graduates have not been controlled for. It could be that evidence of less need for achievement among the blacks than whites could partially account for the status and income disparities (this could be considered discrimination in socialization). Conversely, evidence of higher achievement needs among blacks would point toward greater discrimination than that based on the authors' tacit assumption that the need for achievement is equal for both races. Indeed, the authors are remiss for not attempting to control for this variable in their research design. or at least for not reporting on the literature (or lack of it) on comparing need for achievement among blacks and whites.

Except for this problem, the work is methodologically sound and scientifically objective.

Moreover, this book makes an original and valuable contribution to the empirical study of social and economic inequality between the races.

CLIFFORD J. WIRTH

University of Rhode Island

Foreign Policy and the Bureaucratic Process: The State Department's Country Director System. By William I. Bacchus. (Princeton, N.J.: Princeton University Press, 1974. Pp. 350. \$14.50.)

Since World War II, the study of the making of American foreign policy has become a favored subject of social scientists, journalists, and practitioners, and much reportorial, monographic, and essay literature has been produced - in the nature of theoretical and empirical analyses, descriptive and historical recountings, memoirs, and case studies of particular episodes. This volume, according to its author, is intended not to supersede or replace other approaches, but to complement them. In terms of intent, content, and focus, it concerns foreign policy formulation in general; more specifically it deals with bureaucratic policy making - and even more precisely, with the role of middle-management bureaucracy.

While this volume tangentially treats the roles of the White House and other components of the governmental mechanism, as its subtitle indicates it concentrates on the function of the country directors within the Department of State, and it probes the permeative problem of policy coordination. It contributes to the overall understanding of the conduct of foreign relations and, like other recent in-depth studies of American chiefs of diplomatic missions, the presidential special representative, the professional diplomat, the ambassador-at-large and the country team, it helps to fill one of the glaring gaps in the operation of the United States foreign relations process.

Professor Bacchus's stated purpose is "to discover the actual place of the country director in the foreign affairs community, and...to gain insights into the policy-making process as a whole" (p. 9). He admits that his treatment is a "drastically simplified account" which slights many complexities. It is designed to explain "some of the difficulties underlying the continuing attempts at reform," especially that commencing in 1966 which, it was contended, "represented State's last real chance to assume control of foreign policy," and had it worked as intended, the overall influence of the Department of State "would have been substantially

increased" (pp. 7-8). In short, this is a survey of the purposes, functioning, and successes and failures of the country director system during the past decade.

The basic data of this volume are derived from three related sets of interviews, embracing forty-six incumbent country directors (1968–1969), who provided information about their perceptions and attitudes; fifty-four other government officials (primarily "opposite numbers" who worked directly with the country directors), to ascertain their assessments of the system; and approximately forty others, who gave details concerning specific events, irrespective of the official positions held or their involvement in the system. The interviewing process used, including the author's questionnaires, is described in an appendix.

Relatively short, descriptive and analytical in approach, well organized and documented, and lucidly written, this volume is less concerned with the theoretical or scientific analysis of decision making than with the broader institutional and personal qualification aspects of policy making. Although encompassing the intellectually distinguishable political, bureaucratic, and rational approaches to the subject, it emphasizes the institutional component. It consists of an introduction, which explains the author's parameters and design, nine chapters, and three appendices.

The first chapter presents a general overview of the policy process and the specific bureaucratic role of the country director. Chapters 2 to 4 cope with the nature and functioning of the country director system, reviewing the need for and the mechanics of administrative innovation, the organizational posture of the country director, and his interaction patterns within and outside the Department of State, including relationships with field operations. The next chapter presents a more analytic view of selected aspects of the larger policy process, addressing such matters as the participants, the information factor, the consideration of alternatives, and the nature of constraints.

Review of the individuals serving as country directors is the subject of chapter 6, which touches on the interplay between personnel problems and resources on the one hand, and organizational ability to validate and fulfill established objectives on the other. After analyzing questions of effecting bureaucratic change and innovation in chapter 7, the author turns to assessing the country director system from the perspective of those most intimately involved, and discusses its future prospects. The final chapter summarizes the author's views on the functional value of the system and the

possibilities of, and limitations on, workinglevel policy leadership and coordination.

Fourteen useful tables and three diagramatic charts supplement the analysis. The tables deal with such quantifiable matters as the level of country director contact with American embassy staff abroad, country director-foreign embassy relations religions of birth and career progression of country directors, and comparison of age by grade of Foreign Service Officers and country directors. The appendices present a comprehensive profile of 153 country directors who served from 1966 to 1973 (included are such matters as birthplace, race, sex, education, age, rank, career status, promotion, and career progression); a breakdown of country directors by geographic areas of responsibility; and the author's research methodology and interviewing

As with many ventures in foreign relations organizational and procedural reform, the author concludes that the country director system "did not live up to the expectations of its architects," in that country directors could hardly be said to have assumed "full responsibility ... for all activities in the country or countries assigned to them" (p. 288). Nevertheless, ending on an optimistic note, he adds: "In the absence of progress in establishing a cleancut role for State in the new era, the modest success of the country director system at the margins of the policy process represents a not inconsiderable achievement" (p. 301).

In summary, this review of an aspect of Department of State bureaucratic functioning possesses advantages of limited and manageable scope, probing analysis, relative brevity, and scholarly treatment. Its principal contribution lies in its enhancement of understanding of the intricacies and interactions that characterize the foreign policy system, as well as the problems of aligning expectation and pragmatic operation of institutional processes. It should be of special interest, therefore, to both practitioners and students of the conduct of foreign relations.

ELMER PLISCHKE

University of Maryland, College Park

The Vision and the Dream of Justice Hugo L. Black. By Howard Ball. (University, Alabama: University of Alabama Press, 1975. Pp. 232. \$8.50.)

The late Justice Hugo Black, who died in 1971, got another round of recognition when his long-time colleague William O. Douglas resigned from the Supreme Court in 1975. For

example, on the latter occasion Philip Kurland, the distinguished lawyer-scholar of the University of Chicago, remarked that Douglas and Black had had more profound influence in shaping the Constitution of the United States than had any other Justice since Chief Justice John Marshall.

We cannot know what Black would have thought about Kurland's assessment. A comparatively modest man, Black always declined to make public comment on similar if less sweeping judgments about his contributions to the country through the Court. If, in private, a presumptuous acquaintance asked what he thought about statements concerning the great extent of his influence, he was inclined to respond that the main influence he had tried to exert was toward making judges apply Constitutional provisions in their original meanings.

That Black tried consistently to be a constitutional literalist is amply illustrated in this book. Professor Ball here has compiled Black's views, primarily as expressed in his Court opinions during thirty-four years, on a number of major constitutional issues, including substantive due process and government's prerogatives in regulating the economy; procedural due process; and First Amendment guarantees of individual liberty. Another section is devoted to Black's opinions and views on what is entitled "Substantive Due Process: Regulating Social Relations." In describing Black's constitutional literalism or the pre-eminence he accorded to the First Amendment, Professor Ball is not telling us anything new. Nevertheless, he does usefully show that Black's approach to issues in the several emphasized areas remained constant, and his influence on the Court remained strong, to the last. (Unfortunately, a miserable job of copy editing - revealed in misspelled words, incomplete sentences, and other typographical errors - detracts further from this limited substantive contribution. The publisher owes the author an apology.)

Black's constitutional concepts are clear and have been influential, but they are not quite so self-explanatory, so completely and automatically persuasive as Mr. Ball apparently believes. Neither are those concepts, despite admirable internal consistency, totally free of inconsistency as Black applied them.

Anthony Lewis, a perceptive writer and an admirer of Justice Black, recently wrote in *The New York Times* that "almost nothing is explicit in the Constitution — judges have woven the crucial decisions from generalities." I believe Mr. Lewis would agree that the Bill of Rights is more explicit about boundaries of action, for governments and individuals, than is

much of the rest of the Constitution. In any event, even if the First Amendment's commandments are as absolute as Black thought them to be, they are not always unambiguous as guides in concrete cases. The first part of the First Amendment is: "Congress shall make no law regarding an establishment of religion..." Justice Black repeatedly stressed that "no law means no law." But in a variety of cases, Black and his colleagues had to exercise judgment about whether statutes or programs in question involved "an establishment of religion."

As Ball recognizes, Black hated the "balancing" concept espoused and applied by a number of his brethren - earlier by McReynolds and Cardozo and later by Frankfurter and Harlan. But it is hard to see what other intellectual and judicial process was involved in the Korematsu case in 1944 where the Court upheld, in a Black opinion, the "quarantining" of U.S. citizens of Japanese descent in wartime. Chief Justice Warren said, after his retirement, that Korematsu was a decision which, in retrospect, he thought constitutionally erroneous and one which he personally regretted, since the Court had supported the position he had argued as Attorney General of the State of California. Black, by contrast, in an interview with Anthony Lewis published shortly after the Chief Justice's death, said that he thought the decision was a correct one, because people were rightly fearful of the Japanese, and, had Japanese forces attacked the Pacific Coast, a lot of innocent Japanese-Americans "would have been shot in the panic" (p. 206). That position more reflects a practical balancing of possible results than an absolute view of constitutional commands.

I join in the belief that individual liberties and the democratic process in America are enhanced because of the long presence of Hugo Black on the Supreme Court. Yet at least some of the "clear, explicit and lasting Constitutional boundaries" which Black recognized so precisely and believed in so passionately were partially the product of his own personal convictions, informed by a thorough reading of history as well as by other influences in his life. Professor Ball, beyond reciting some of Black's more important opinions, nicely summarizes the Justice's beliefs about the efficacy and fragility of American democracy, and about the rule of law. Other authors have illuminated the context in which his views became law, or factors of influence on the law. But many questions remain, some of them relating to the origins of particular judicial positions, others to the motivating impulse that made Black such a fierce competitor in political, legal, and philosophical

arenas. Not even in the aggregate collection of books about him has the whole story of Hugo Black, as a man, lawyer, politician, philosopher, and jurist, yet been told.

STEPHEN P. STRICKLAND

University of California, San Francisco

Coastal Resource Use: Decisions on Puget Sound. By Robert L. Bish, Robert Warren, Louis F. Weschler, James A. Crutchfield, and Peter Harrison. (Seattle: University of Washington Press, 1975. Pp. 206. \$10.95.)

Saving the Coast: California's Experiment in Intergovernmental Land Use Control. By Melvin B. Moguloff. (Lexington, Mass.: D.C. Heath and Company, 1975. Pp. 136. \$12.00.)

These are two quite different books dealing with a single but complex issue: the appropriate political institutions for governing shorelines in the United States. The first is an analysis of the economic, demographic, political, and natural resource setting of Puget Sound in the State of Washington and the events leading to the adoption of the Shoreline Management Act in 1971. The second is a critical evaluation of California's brief experience under the Coastal Initiative passed in 1972. The first is written from the multiple perspectives of the economist and a political scientist consciously utilizing political economy and public choice frameworks for analysis. The second is less concerned for theory than for practical judgments concerning the promise of the Coastal Initiative to deal with issues of coastline use and manage-

Despite the differences in approach and perspective, the stories they tell are similar. In both states, environmental groups were frustrated in repeated efforts to obtain legislative action on proposals to impose controls on coastline development. The result was in each case a resort to the initiative process and approval by the voters of measures establishing a broad role for the states in shoreline management. In neither case did the initiatives go as far as the environmentalists wanted, particularly in limiting local involvement in shoreline use and management decisions. The statutes call for continued responsibility for local government in shoreline decisions, with state political units having authority to review local decisions with respect to their conformity to the standards set down in the law.

The study of Puget Sound is a comprehensive examination of the complex ecology — both natural and human — of the total region.

It depicts clearly the diversity of the region and the natural division and interdependencies of interests with respect to the development and use of the shoreline. This diversity of values is clearly displayed in the vote on the Shoreline Management Act, with the voters of the more sparsely settled areas opposing the Act, while the voters of the Seattle metropolitan region strongly supported it. The authors demonstrate the continuing struggle between local interests in sparsely settled, undeveloped, and economically less well-off areas who want the perceived benefits of development and the urban interests who look upon these less densely occupied regions as their playgrounds.

The authors provide a learned discussion of the principal elements of analysis of the political economists: externalities, public goods, common pool, flow resources. But the application of these principles in terms of the actual situation in Puget Sound is disappointing. The authors obviously believe in some form of regional government, although they recognize the difficulty of sorting out local, regional, statewide, and national interests even in as distinctive a region as Puget Sound. They believe in regional government because it reduces costs for interests that participate in environmental decision making only at high cost to themselves and with difficulty of sustained efforts. But just how does one apply the principles of political economy - i.e., which externalities should be dealt with by which level of government? Agreeing that some resources are public goods, what system of public management will provide for optimization of the public welfare? Who should have what kind of voice in what kinds of decisions? If one accepts the idea that the locality is too narrow a framework to encompass all interests concerned with the locality's resources, what is an appropriate broader framework? And how does one tell what it is? What is the appropriate weight to give local interests concerned for jobs and development as against broader regional or national interests concerned for environmental protection? And what would they conclude if dominant local interests were for environmental protection and the broader regional and national interests preferred development? There are no answers in this study despite the sophisticated tools of analysis.

The Moguloff book offers no such comprehensive discussion of the physical and social environment of the California coastline or of the events leading to the Coastal Initiative. It does offer an assessment of what the regional and state commissions did during their first year, particularly in coming to terms with the

political environment in which they operated. This involved two major tasks: granting or denying permits for development and developing a coastal plan. The plan is only now taking form and therefore is not included in this assessment, but the permit-granting function is evaluated. The essential argument is that the regional and state commissions have not stopped development along the coastline, that they could not without substantial risks to the viability of the coastline control effort itself, but that they have made a difference. In effect, they bargained with developmental interests, making them pay a substantial price in terms of accommodating other interests in bringing their projects to fruition. The author emphasizes the dangers inherent in the control effort: that denials of permits for development would lead to bitter and prolonged battles over alleged "taking" of property without due compensation. He concludes they have done an effective, if necessarily imperfect, job of keeping coastline protection in an important place on the political agenda.

These are both useful books by authors who are thoughtful and well informed. They recognize complexity, multiple sets of values and the realities of the political systems in which decisions must be made. It is not clear from their analysis, however, why shorelines should be considered apart from other natural environments that are threatened by development. What are the consequences, for example, of imposing controls on shoreline development controls that have differential effects on various groups wishing to use shoreline resources while failing to impose similar controls on adjacent resources that are not defined to be within the "natural" shoreline boundaries? Any definition is bound to be arbitrary, but it is not clear why the shoreline or any other natural phenomenon should define a resource management area. A metropolitan region, while not "natural," does have boundaries capable of definition in demographic or economic terms which may be as meaningful as the boundaries of "natural" areas such as shorelines or river basins. Perhaps the area west of the Cascades in Washington and the part of California south of the Tehachapis constitute appropriate regions, not because of physical geography but because people interact intensively in those regions, depend on each other, and compete with each other for resource use.

Whatever one's conclusions may be, these studies have provided useful analyses and arguments regarding the environmental management problems of our era. They give us insights into resource management design questions and sen-

sitize us to the significant issues.

DEAN E. MANN

University of California, Santa Barbara

In Command of Tomorrow: Resource and Environmental Strategies for Americans. By Sterling Brubaker. (Baltimore, Md.: The Johns Hopkins University Press, 1975. Pp. 177. \$7.95.)

Like Brubaker's To Live on Earth: Man and his Environment in Perspective (1972), this new book, also a Resources for the Future Study, is intended for the general reader.

The two books furnish a quite comprehensive view of many of the physical, social, and economic problems faced by man in attempting to ensure his long-run future on earth. In the new book, as he did in the first, the author declines to join the doomsayers. He says he is optimistic, in the Western tradition, that man can control (In Command of Tomorrow) his fate in the long run in spite of materials depletion and environmental degradation.

Brubaker's main concern in the new book is with the United States. For many political scientists, as well as other readers, one basis of his focus on the United States will be debatable. He feels that only the United States is sufficiently well organized to achieve the necessary consensus to adopt and maintain over the long run the strategies to counter depletion of basic resources and degradation of the environment. He does not ignore global implications, but his emphasis is on what the United States can do in its own interests, as well as in serving as an example to other nations.

For students of political feasibility, Brubaker's discussions of the possible major strategies for man, and especially for the United States, are perhaps the most interesting parts of the new book. He contends that there are only two paths for man to follow in securing permanent occupancy of the earth, and that the choice between paths cannot be too long postponed. One path is to try to find a "modest but sustainable place in the natural system by changing to a technology that is nondepleting in character" (p. 4). Brubaker refers to this path as a "low-technology" course. He notes that this approach, while promising to extend the time of man's tenure on earth, presents some inevitable difficulties. The reasons are many: many materials inevitably dissipate with use, and recapture of some materials would require large amounts of energy. Further, the loss of metals over time would make it difficult to capture and utilize energy. The first path presents the dangers of inevitable economic decline.

Mr. Brubaker's second path, which he thinks is the path we should choose, is in part based upon his belief that a high production society is necessary to sustain a worthwhile culture, and in part on the belief that presently Americans, and eventually others, will settle for no less. He thinks we should make

a calculated effort to escape the constraints of resource exhaustion through new technologies. It would not simply rely on enlarging the conventional supply of exhaustibles but rather would seek to replace them with inexhaustible resources. The difference is between a temporary reprieve and a permanent solution. The second path means finding an inexhaustible source of energy as a prerequisite to other technology that would make use of more common materials. Given ample energy, we might make a direct attack on entropy, using common rock and sea water as resources, or perhaps we could reach for the alchemist's dream of elemental transmutation (p. 5).

Clearly, the "alchemist's dream" is suggested with tongue in cheek!

The book is excellent in its explanations of issues, and the strategies prescribed seem sensible: But the book will arouse disbelief among a number of readers. First, many will not agree that technology can be counted upon to achieve such heights. Second, although many of the strategies suggested will find approval by many observers of the economic, social, and political scene, there will be disagreement on what seems to be Brubaker's belief that these strategies can be followed without much more positive, even authoritarian, government than we now have.

ROBERT H. PEALY

University of Washington

TV Violence and the Child: The Evolution and Fate of the Surgeon General's Report. By Douglass Cater and Stephen Strickland. (New York: Russell Sage Foundation, 1975. Pp. 167. \$5.95.)

Since its introduction in the United States, television has been the object of great hopes and fears. Proponents of television have pointed to its potential for expanding our experiential horizons through almost instantaneous exposure to people and events around the world. Critics have warned, however, of its potential for constricting our intellectual and social horizons by converting active humans into passive vidiots. In addition, many have expressed deep concerns about the possibly harm-

ful consequences of exposing millions of children and adults to a television diet rich in violence.

This fear led eventually to the establishment in 1969 of the Surgeon General's Scientific Advisory Committee on Television and Social Behavior. Three years and 1.8 million dollars later, the committee reported "...a preliminary and tentative indication of a causal relation between viewing violence on television and aggressive behavior; an indication that any such causal relation operates only on some children (who are predisposed to be aggressive); and an indication that it operates only in some environmental contexts..." (p. 76).

In TV Violence and the Child, which was commissioned by the Russell Sage Foundation, Douglass Cater and Stephen Strickland provide a highly readable history of the events leading to and immediately following the publication of this conclusion. Cater, as director of the Aspen Institute Program on Communications and Society, and Strickland, as director of the Washington, D.C. office of the Health Policy Program of the University of California, San Francisco, bring to their discussion first-hand experience with media and public policy issues. Chapters are devoted to describing the controversial selection procedure for committee members and the internal conflicts over the development of a modus operandi; to summarizing the findings of various laboratory and field research projects on the effects of televised violence on children, of institutional analyses of the television industry, and of content analyses of television programming commissioned by the committee; and to chronicling the responses of the public, the government, and the television industry to the report. On the basis of their interviews with participants and their investigation of public records in the controversy surrounding the report, Cater and Strickland conclude that the nation's scientific, public, and governmental institutions are ill-equipped for recruiting expert talent to provide quick and clear findings to guide public policy decisions, especially when the problem is complex and politicized.

The book's strength lies in the authors' ability to synthesize information from a variety of sources to provide a comprehensive history. Students of public policy decisions will no doubt welcome this, but they should not turn to TV Violence and the Child in order to find a social scientific analysis of a governmental commission. Indeed, there is little social science in this discussion of the Surgeon General's committee. No references are made to analyses of previous commissions, and no social scien-

tific concepts or models are employed as explanatory tools. Despite the book's value as a chronology of events, one cannot help wondering whether the material presented is sufficiently detailed to be of value to students of public policy. Those who wish to develop general models of governmental decision making and who hope to use TV Violence and the Child as a source of data are likely to be frustrated. The detailed data necessary for developing generalizable conclusions is probably lacking.

RONALD P. ABELES

Social Science Research Council, New York

Thomas McKean: Forgotten Leader of the Revolution. By John M. Coleman. (Rockaway, N.J.: American Faculty Press, 1975. Pp. 352. \$16.95.)

As a biography this book has shortcomings, the most serious of which is the decision to end the narrative in 1780, before McKean's service as governor, 1799-1809. The omission is particularly puzzling in view of the inclusion of seemingly tangential material on McKean's early life. In chapters II ("Education under the Rev. Francis Alison") and VII ("Robert McKean and the New Jersey Medical Society") Thomas McKean is not even the central figure. This is not to say that these chapters have no value. Indeed, they are instructive pieces on two aspects of colonial social history that students of colonial education and medicine should welcome. But, they belong in journals. In this book they are distracting. The remaining parts of McKean's early life are well done, depicting a remarkable colonial success story. From decidedly middle-class beginnings Mc-Kean made his way, through politics, marriage, and social contacts, into the elite circle of the middle colonies. His use of a legal career as a stepping-stone to political office is particularly revealing of the inextricable mixture of the lawyer's art and politics in the colonial era.

Professor Coleman's understanding of the intricacies of Pennsylvania-Delaware politics is acute, and his explanation of the coming of the Revolution to those two colonies is expert. The account of the manner in which the radicals maneuvered Pennsylvania into support for independence in 1775, with propaganda, class appeal, and not a little military intimidation is a side of our revolution not often seen. Professor Coleman's picture of the American Revolution

The handling of the internal workings of the revolutionary governments, both state and national, is on a par with the recounting of the coming of the rebellion, and it is here that the book makes its most significant contribution. Professor Coleman does not succeed in elevating Thomas McKean to the stature of a John Adams. McKean is always present, however, often in the inner councils, sometimes on the periphery - but consistently a participant. This is certainly to be expected, as McKean's service on the national level dated from the Stamp Act Congress and his was the longest tenure of any of the members of the Continental Congress. What Professor Coleman has provided is a close look at the revolutionary governments, from local level to national level, through the eyes of one participant. This, rather than McKean's career itself, is the book's significance.

JAMES P. WHITTENBURG

College of William and Mary

The Politics of Revenue Sharing. By Paul R. Dommel. (Bloomington: Indiana University Press, 1974. Pp. 211. \$8.50.)

When several prominent mayors labeled the revenue sharing program a "gigantic doublecross" and a "cruel hoax" it may have come as a surprise to some who had heard the same mayors strongly supporting the passage of revenue sharing several years before. The explanation of this paradox is the main focus of The Politics of Revenue Sharing. Professor Dommel examines the various modifications that initial proposals underwent before passage of the State and Local Fiscal Assistance Act of 1972. The coalition necessary for passage of the Act included Republicans and conservatives who wanted to decentralize decision-making power as well as liberal Democrats who were under pressure from urban interests who badly needed funds. Dommel's analysis traces the development of the issue of revenue sharing and makes understandable the change in attitude of the mayors.

The 1950s and 1960s saw increasing pressure on state and local governments for greater services while at the same time tax revenues were not rising proportionally. Growing dissatisfaction with the proliferation of federal aid programs and the sophistication of grantsmanship necessary to obtain federal funds generated disenchantment leading to several proposals to replace the existing federal grant system with

was further developed by a Johnson task force headed by Joseph Pechman. It was spurred by the expected economic boom in 1964 and the wish to avert fiscal drag by distributing surplus revenues to the states. The plan was subsequently modified so as not to depend on a surplus but to provide a fixed percentage of federal income taxes to the states. The money would have no strings attached and would supplement existing federal grant programs.

Dommel traces the capture of the initiative for revenue sharing by the Republicans after Johnson's abandonment of the Heller-Pechman proposal subsequent to the 1964 elections. The many revenue sharing bills introduced by Republicans covered a large range of proposals but the dominant thrust was to use tax sharing as a means of turning much federal policy making over to the states by abolishing many federal grant programs.

In the presidential campaign of 1968 both candidates favored some form of revenue sharing and the focus of the various protagonists shifted from Congress to the White House. The cities, however, were still suspicious of proposals that would funnel federal aid through the states. They felt that such a program would give too much discretion to state officials who would not see urban problems as major policy priorities. This would be particularly dangerous to the cities if categorical grants were replaced by revenue sharing.

In August of 1969 President Nixon announced his plans for revenue sharing as part of his "new federalism" program. His primary concern was the decentralization of governmental power, but he did not propose that the existing grant structure be replaced by revenue sharing. According to Dommel this was the key to liberal congressional support for the plan. The cities were promised a share of the funds and were assured that the categorical grant programs would not be eliminated. Thus liberal members of Congress were put under much pressure from urban interests to support revenue sharing.

After explaining the changes in the revenue sharing package necessary to gain the support of the diverse constituencies which finally came together to make passage possible, Dommel spells out the final compromises reconciling the president's plan with House and Senate concerns. Liberals and urban interests had to be assured that revenue sharing would be "add-on" and not "substitute" money. One of Dommel's conclusions is that mayors and liberals were not entirely wrong when they cried "double-cross" after seeing Nixon's 1974 budget. It recommended many cuts in grants-in-aid to state and

local governments, some of them being explicitly justified by the availability of revenue sharing funds.

Dommel's book is particularly good in putting the revenue sharing issue in the context of the overall policy-making process. A major strength is the clarity with which he traces the development of the issue. From the early proposals to replace the categorical grant system to the final compromises to attract urban interests, he pinpoints the shifts in the strategy of proponents and their bases of support. His analysis makes us better able to evaluate statements about what revenue sharing was intended to be.

In the final chapter he presents some data on the initial reports on the use of revenue sharing funds. By now these data have been supplemented in subsequent reports and analyses. The impact data, however, are not the main point of Dommel's book and can be found updated in several books oriented toward the economic and distributional effects of revenue sharing. Dommel's purpose is to analyze the development of the revenue-sharing concept and its journey through the policy-making process, a task which he executes admirably.

JAMES P. PFIFFNER

University of California, Riverside

Shoreline for the Public: A Handbook of Social, Economic, and Legal Considerations Regarding Public Recreational Use of the Nation's Coastal Shoreline. By Dennis W. Ducsik. (Cambridge, Mass.: The MIT Press, 1974. Pp. 257. \$12.50.)

Fifty-five per cent of the American people live within the 50-mile coastal belt bordering the Atlantic and Pacific oceans, the Gulf of Mexico, and the Great Lakes. Only one-third of these 60,000 miles of shoreline are suitable for recreation; only 5½ per cent of that one-third is publicly owned. Yet public demand for recreation is increasing at an annual rate of 12 per cent, and the recreation industry is the fourth largest and fastest-growing industry today.

The paradox of shrinking land supply and increasing demand for water-oriented leisure activity has been created by a policy-making process which has historically and systematically underrepresented the value of public open spaces. To encourage the development of environmental and ecological priorities in shore line policy making, Congress passed the Coastal Zone Management Act of 1972, which provides matching funds for coastal states that create state coastal zone management programs. The

Act mandates expansion of public recreational opportunities by "giving full consideration to ecological, cultural, historic and aesthetic values as well as to the needs for economic development" (p. 7).

Since the tendency has been to promote private economic interests at the expense of the more diffuse, ill-defined social welfare concept, Dennis Ducsik, Instructor in the Department of Civil Engineering at MIT, has written this book to provide a "groundwork for the development of effective policies to combat the continuing encroachment on the public interest in the shore line, and to bring about a more effective representation of that interest in coastal decisionmaking" (p. xiv). Ducsik sees the conflict as between "the need for expanded recreational opportunities for the public (especially in urban areas) ... and the desire for intensive private development. .." (p. 4).

Part I of the book develops the historical, philosophical, and institutional reasons for the problem and arguments supporting the environmental/ecological perspective. He sees the landarea interface as a resource rather than a commodity which must be enhanced to protect its opportunities for human recreational uses and to preserve its vitality as an ecological system. Supply of suitable coastline is limited by site problems and natural factors such as pollution and erosion, but more importantly by the economic and political relationships which tend to safeguard and enhance the rights of private property owners. "The private market is ill-suited for the allocation of recreational resources for public use.... Public recreation ... ranks low on the capital intensive scale, its value to the public is diffuse, costly to collect and possibly unquantifiable" (pp. 67, 69).

Having established the need for government intervention to promote public needs. Ducsik focuses in Part II on legal strategies for change because "it is the law that will shape and coordinate policy alternatives. . ." (p. 84). Two chapters examine the law of the seashore, and common law approaches to create availability and accessibility of littoral open spaces for the public. Chapters 8, 9, and 10 each deal with possible state policy tools: purchase of land through state powers of acquisition or easement or both; the use of state police power, expanded to include open-space objectives within its scope; and land-use controls such as zoning, setbacks, official mapping, and other regulatory strategies. The final chapter ably summarizes the rationale for collective action, the need for administrative flexibility in building a case, and the necessity to balance social and economic objectives in setting priorities for shoreland

allocation.

While the language of the book tends to be technical, dry, and a bit repetitive, and the rationale traditionally pro-environment, the technique chapters present the relevant case law clearly and concisely, emphasizing both the advantages and disadvantages of each option from the policy maker's perspective. Indeed, one of the most useful aspects of the book is its focus on institutional interactions between state and municipal governments, and between legislatures and courts, which should provide policy makers with a clear map of past relationships as a guide to future activity.

CAROL S. GREENWALD

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Pork Barrel Politics: Rivers and Harbors Legislation, 1947-68. By John A. Ferejohn. (Stanford, Calif.: Stanford University Press, 1974. Pp. 288. \$12.50.)

Professor Ferejohn tests two hypotheses about congressional behavior in this study of the Corps of Engineers' appropriations: (1) the institutional distribution of influence in Congress will affect legislative outcomes, and (2) programs which can be distributed among constituencies will receive favored treatment by Congress. Ferejohn is ingenious and effective in presenting many tables, graphs, and regression analyses to support his arguments in defense of the hypotheses. Most of his empirical conclusions are consistent with commonly held views, and some of the data suggest new hypotheses about relationships among congressional influence of alternative models of congressional behavior. Unfortunately, the Corps of Engineers is a very special agency, and therefore it would be hazardous to use this one sample to verify any general statements. And even within the restricted discussion of the Corps, the conclusions drawn by this sophisticated analysis are not always persuasive because of the limited data and tests.

The three chapters of Part I present a summary of the economic and political benefits-costs literature, and informal arguments for the hypotheses. A more careful defense of the hypotheses is contained in the book's concluding chapter. The second part, four chapters, reviews the budget cycle of the Corps, and the House, Senate and Conference Committee treatments of the aggregate Corps budget. Part III, the most ambitious section of the volume, analyzes the distribution of corps projects among states by evaluating marginal changes

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associated with membership in key House or Senate committee posts.

The conclusions that can be drawn from the analysis of the aggregate appropriations to the Corps are flawed by limiting the study to one agency, and when others are considered, the comparison is to an aggregate of other bureaus. As the author points out, the Corps "correctly considers itself to be a Congressional agency, and therefore it is not surprising that Congress and its committees have a different role in regard to the Corps budget. To test his hypothesis that a divisible budget will be favored by a constituency-serving Congress he would have had to deal with a number of agencies whose budgets satisfy his condition of divisibility among states, and there are a great many of them. In fact, most of the budget is divisible when you consider inputs as well as outputs.

The heart of the volume is the empirical evidence about the distribution of projects by the influence of congressional members. Ferejohn concludes that the states of members with influence over the appropriation process will gain by their membership. Influence will be exercised through well-recognized attributes such as committee membership, seniority, chairmanships, and so on. Generalized influence in the legislature, however, has no constituency payoffs; to be effective influence must be specific to an agency. These hypotheses are tested through a set of regressions of memberships, seniority, etc., on the number of new starts of construction in a state initiated by the House or the Senate or proposed in the president's budget.

If a state receives relatively more projects than it "needs" because it has relatively more members of key committees, then influence has a payoff. Reasonable, but one might argue that the distribution of representation is a function of willingness to invest political resources in response to constituency demands and that "influence" is no more than a mechanism to allow a state to pay a "higher price" for "more valued" goods. If there is a political marketplace then the analyst has to separate out the forces of demand and supply to discover whether there is any independent weight to be given to congressional organization. Unfortunately, Ferejohn's efforts are marred by the variables he has analyzed, but he has made a beginning.

A questionable part of the empirical analysis is the use of the number of Congress-initiated construction starts per state as the dependent variable — the measure of political payoffs to influence. The number of project starts and the state as a unit of aggregation should have some

positive value to a legislator, but they may not be the project's most significant characteristics for the state or for the legislator.

The number of projects need not be closely related to the size of current or future appropriations within a state. Since projects come in many sizes, a state that has proportionately more starts need not have proportionately more appropriations or constituency benefits. Certainly, the features of a project and its scale are of importance, and one should expect that the legislator is forced to make tradeoffs among a project's many dimensions. The legislator may appear to be very effective by sponsoring a project, but if it is small and single-purpose, his influence for constituency payoffs may be illusory. New construction starts are necessary for a state or district to be favored, but they are not sufficient. In fact, Ferejohn properly stresses that majority rule will bias the budget toward small projects distributed over many states. Therefore, the number of projects necessary to get political consensus may enter as a constraint, but if the leadership varies the distribution of sizes, the budget not be "grossly" inefficient. A better variable would have been the anticipated scale of future appropriations.

The second part of this variable, the total per state, seems even more questionable. The anecdotal and theoretical literature on congressional behavior stresses the satisfaction of district, rather than state, constituency interests as a legislator's objective. Even Ferejohn, when he elaborates his arguments in the final theoretical chapter, deals only with district constituency representation. He chose not to use construction starts in a district as the dependent variable because he found a negative relationship between influence and starts - a surprising conclusion that calls for more analysis than the few sentences Ferejohn presents. He defends the state rather than district variable on the basis of the cohesiveness of state delegations. But not all states have cohesive delegations. It would have been interesting to use cohesiveness of delegations as an independent variable to see how powerful it proves to be. In passing, the author mentions that California and Texas are cohesive delegations and that these states have several members on key committees. Possibly, these two facts are not independent; cohesive delegations overstaff key constituency committees and are rewarded by a larger number of starts. Whatever the pattern of causation for a state, the exclusive attention to the state variable may have led Ferejohn to make incorrect judgment about the payoff for generalized congressional influence. Leadership roles outside of the public works committees may have significant payoffs where the leader needs it, in his district. Senior legislative leaders have careers in Washington, not in state offices, and therefore they may not be as active in state coalitions and objectives as members who choose more parochial roles. A total neglect of district payoffs may have seriously weakened any conclusions that could be drawn about the influence of nonpublic works leadership.

I do not offer the above criticisms to discourage readership and serious study. On the contrary, the dissertation is a useful addition to the growing body of quantitative Congressional studies. There are sections, e.g., the relations between the Senate and House, which have substantial conceptual improvements on earlier works and present impressive empirical evidence. The criticisms are meant to help in the cumulative process of research. If others deepen Ferejohn's work on the Corps and extend it to other agencies we will have a much firmer understanding of the relationship between legislative institutions and policy outcomes.

JULIUS MARGOLIS

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Blacks and the Military in American History: A New Perspective. By Jack D. Foner. (New York: Praeger Publishers, 1974. Pp. x, 278. \$10.00, cloth; \$3.95, paper.)

Jack Foner's Blacks and the Military in American History, demonstrates that Afro-Americans played significant roles in every American war beginning with the Revolution in spite of the resistance to their participation by generations of politicians and military officials. The Revolutionary War set a pattern in which blacks were accepted into the military during hostilities, when white manpower was in short supply, the response to mobilization was poor, or there was danger of black defection to the enemy. Admission was delayed to avoid alienating slaveowners, and when peace came blacks were discharged as quickly as possible. With lower manpower demands the military returned to its conventional practice of favoring whites.

This pattern continued well into the twentieth century, but Professor Foner limits himself to a presentation of historical detail, and leaves institutional patterns and changes undiscussed, although they are implicit in the material.

For example, Foner shows that successive generations of blacks believed their performance in war would prove their worth as

DuBois in 1918 felt that war would bring "'an American Negro with the right to vote... work and ... live without insult" (p. 109). By World War II and the Korean war, blacks struggled to gain entry but with less hope about the results, although in 1967 Lt. Col. George Shaffer said of the high casualty rate of blacks: "'I feel good about it. Not that I like bloodshed, but the performance of the Negro in Vietnam tends to offset the fact that the Negro wasn't considered worthy of being a front line soldier in other wars' "(p. 205).

Second, Foner demonstrates how institutions such as the American military have acted as carriers of their society's values by consciously teaching their allies the theory and practice of racism.

Third, racism has assumed radically different forms in the years between 1900 and 1970, according to Foner. Blacks were admitted into the military by 1945, but were concentrated in labor battalions, and disciplined by discriminatory procedures. Army "justice" for black soldiers was so swift that Thurgood Marshall remarked in 1951: "even in Mississippi a Negro will get a trial longer than 45 minutes..." (p. 190). Official desegregation of the military had begun in 1954, but supportive facilities were still being desegregated in 1963.

Finally, the military has been viewed as a channel for black upward mobility, but Foner's study suggests that the military's absorption of large numbers of unemployable blacks mutes economic and social discontent. For example, 64 per cent of eligible blacks were drafted for Vietnam in 1967, but only 31 per cent of eligible whites; 27 per cent of black soldiers were assigned to the infantry, but only 18 per cent of whites, and 20 per cent of army fatalities were accounted for by blacks. To complete the cycle, higher proportions of blacks end up in military courts and receive dishonorable discharges, which officially certify them as unemployable. Foner's work suggests that desegregation has integrated racism into the military rather than eliminated it.

In spite of its limitations, Foner's study will be a valuable sourcebook on the military's contributions to the status of blacks in the United States.

DIANNE M. PINDERHUGHES

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Who Shall Live? Health, Economics, and Social Choice. By Victor R. Fuchs. (New York:

that pervades the American health care establishment — especially concerning the future role of government — a calm, articulate analysis of health policy and health care is a particularly welcome and useful contribution. And it is precisely this that economist Victor Fuchs has given us in his lean, eminently readable monograph.

Who Shall Live? deals with the ends and means of American health policy. Joining a growing number of leading observers of the American health scene, Dr. Fuchs (Vice President and senior research staff member, National Bureau of Economic Research) encourages us to view the pursuit of health (evidenced by reduction in mortality and morbidity) as the objective of national health policy and to view health care itself as one of the factors, together with heredity, individual behavior, and environmental influences, that affect morbidity and mortality. Among the data used to buttress his argument, Fuchs points to selected mortality data by age, race, and sex that is worth pondering. Together with what is known about etiology of various causes of death, these data highlight the broader fundamental theme of his book, that the connection between health and medical care is much more tenuous than we are inclined to believe. The importance of genetic and environmental factors and personal behavior and "lifestyle" appear to be much more important than most would suspect, common sense notwithstanding. This is the theme of a fascinating section analyzing mortality data for the contiguous states of Utah and Nevada.

The bulk of Fuchs's book is devoted to the medical care system (with separate chapters on the physician, the hospital, drugs, and health care financing) reflecting the fact that the health crisis in America centers on financial and geographic access to medical care services. Moreover, the principal health issue in the United States remains National Health Insurance - how to redistribute equitably the growing per capita cost of medical care among the population consistent with the efficient production and appropriate utilization of health care services and without exacerbating the already high rates of inflation in this sector. In focusing on physicians, hospitals, and drugs, Fuchs deals with the lion's share of medical care costs.

His treatment of these subjects goes far in helping the reader understand the behavioral relationships and incentives at work in the American health care system. Such understanding is fundamental if we are to be successful in controlling growth in per capita costs of care in the years ahead. But, no matter how well we do on future cost containment, we are faced with

the question of who is to pay the cost of medical care services. If there is a single theme in his chapter "Paying for Medical Care" it is that such costs cannot be shifted from consumers to some other party like government, or business. Costs always come back to the individual, if not directly, then indirectly through increased insurance premiums, reduced wages, higher prices of the products of American business, or higher taxes. To be sure, alternative payment schemes can mitigate the devastating costs to specific families or individuals from prolonged or catastrophic illness, and mechanisms can be devised that shift the burden of paying for health care from the lower income to higher income families and individuals, but in the last analysis the average per capita cost of care will be the same for a given level of services utilization.

Written by a specialist, in language everyone can appreciate, Who Shall Live? is a clear contribution to better public policy in health.

MICHAEL S. KOLEDA

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Crime and Criminal Justice: Issues in Public Policy Analysis. Edited by John A. Gardiner and Michael A. Mulkey. (Lexington, Mass.: D.C. Heath & Co., 1975, Pp. 211. \$14.50.)

Research in the arena of crime has grown geometrically in the last decade. Once almost exclusively the province of sociologists, crime has begun to attract the sustained attention of political scientists, particularly as government agencies such as the Law Enforcement Assistance Administration look for hard-nosed analysts of public policy. The purposes of this volume are to provide an analysis of the program evaluation literature in criminal justice and to speculate about where future research should proceed.

On balance, John Gardiner and Michael Mulkey have done a less than skillful job of selecting and editing contributions (fourteen in all). With few exceptions, the length of selections is too short (median — eight pages), the ideological array is depressingly narrow (neo/pseudo-liberal) and the substantive focus is highly skewed (four essays on police). This substantive imbalance reflects, in part, how LEAA and other organizations have allocated research dollars, but it results also in highly repetitive discussions of the same literature. In contrast, scant attention is paid to the (trial) court system. There is only one selection on prosecutors, one critique of the NAC-CJSG's

Courts, and nothing on grand juries, the administration of bail, sentencing or public defender systems, to name only a few areas where research and field programs have been undertaken. In sum, the framework of criminal justice has been poorly conceptualized.

As with many edited volumes, the quality and originality of these contributions vary widely. By my somewhat crude count, I would rate the proportion of "worthwhile" selections to be slightly less than fifty per cent, and some of these present substantial problems. Two essays are well done - James Q. Wilson ("Crime and the Criminologists," reprinted from Commentary), and Wesley Skogan ("Public Policy and Public Evaluations of Criminal Justice System Performance"). Wilson makes a useful distinction between causal and policy analysis of crime by observing that policy planners are constrained politically and logistically from designing programs which go to the roots of crime. Notwithstanding definitional problems of crime which Wilson characteristically evades (crime = street crime), this cautionary warning about the limits of government and its institutions, especially post-Vietnam, seems well founded. It is, nevertheless, subtly ignored in some subsequent chapters. Robert Martinson, for example, after a painstakingly long secondary analysis of attempted correctional reforms, concludes that few, if any, types of programs consistently produce lower recidivism. Yet this result should surprise few (or need twenty-five pages of reiteration), if Wilson's warning is even partially heeded. Rather than dwelling upon inept research designs, Martinson might well have reflected on why prison "reforms" apparently do not affect postprison, crime-related behavior and on resultant policy implications. In contrast, Skogan undertakes a concise review of consumer evaluations of police performance with appropriate concern for the varying methodological integrity of the several studies; more importantly, he offers some specific, well-thought-out suggestions for future research [e.g., controlling for citizens' initial expectations about performance quality, and differentiating the various tasks which police perform]. With similar opportunities at hand, others in this volume merely toss out at the reader a slew of general, unrelated research questions which someone, one day, might (often ill-advisedly) pursue.

Three other chapters are problematical, primarily in their lack of resolution or full development of ideas. Lief Carter raises the important dilemma (drawn from his excellent empirical study of a prosecutor's office) of the limits both of flexibility (discretion) and its

obverse, uniformity (lack of discretion), in criminal justice systems, but goes no further; his policy implications are abstruse. Michael Mulkey provides a concise review of the law and social science literature on plea bargaining, but also fails to go further. One possible advance in plea bargaining research would be to break down the phenomenon by broad offense type, particularly as localized data bases on felony plea bargaining are now being developed. The City Bar Association of New York has undertaken one such study related to the impact of legislative restrictions on charge reduction for drug offenses. Finally, Jameson Doig provides a creative interpretation of the historical forces which led to the juvenile justice system, but his policy conclusions [to spend more money on community facilities and/or reduce juvenile justice "overreach"] miss a significant part of the problem. The policy irony of juvenile justice seems to be that while many youths are detained, labeled and possibly incarcerated, without due process, for "offenses" which are not punishable under adult criminal codes, many other juveniles disproportionately contribute to the serious crime rate without having to face the relatively more severe penological consequences which confront adults.

What, then, does this volume add up to? The book is much too expensive to be used in undergraduate teaching and not very well done for graduate seminars. It could have served as a useful reference work for criminal justice researchers and policy planners, but the level at which contributors write and the understandings assumed of their readers perhaps vary too widely. Some assume in-depth familiarity with the literature, while others treat readers as novices to the phenomenon of, say, plea bargaining or white collar crime. In sum, I question whether literature reviewing and (idle) speculation can produce good books in the absence of the presentation of original research findings, innovative theorizing, or solid text writing. The functions and uses of books are not infinite; such things should be better mapped out in advance.

JOHN PAUL RYAN

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Making the Papers: The Access of Resource-Poor Groups to the Metropolitan Press. By Edie N. Goldenberg. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 164. \$13.00.)

Lacking the wherewithal to exert direct influence upon public policy decisions, resource-poor groups try to communicate their

demands to officials indirectly through the news media. This case study, by a political scientist at the University of Michigan, explores the conditions surrounding group bids for access to the metropolitan press. Specifically the focus is upon four resource-poor groups in Boston - a mass-based organization lobbying on behalf of the elderly, a community improvement organization, a group of the poor on Cape Cod promoting the cause of low-cost housing, and a welfare rights organization trying to improve the lot of mothers with dependent children. The principal targets of these groups are the Boston Globe (and Evening Globe), the Herald Traveler, and the Record American. The data were derived in 1971 and 1972 when the author served as a reporter intern and also interviewed 142 persons representing the news media, resource-poor groups, policy makers, and social welfare professionals.

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In the growing literature examining the relationship between press and politics there is certainly a place for a book describing the transactions of press, interest groups, and public officials. Moreover, there is justification for narrowing that scope, as does Goldenberg, to the interplay of resource-poor groups and the press. The problem with this particular effort, however, is twofold: (1) the depth of coverage, specific findings, and general conclusions probably do not warrant presentation in book form; and (2) a few key questions hinted at in Goldenberg's discussion scarcely get the attention they deserve.

The author divides the discussion into five parts. The first is a brief but still too detailed airing of the nature of access and the strengths and weaknesses in David Truman's formulation. Person A has access to Person B when the former has a hearing with the latter who has not as yet foreclosed the possibility of granting A's requests. Access is relational, direct or indirect, not always transitive, measured in a variety of ways; it is only a hearing that may or may not lead to a successful outcome and, hence, distinct from influence. Part II lists the group goals and resources that affect decisions to seek access to the press. Part III, the most rewarding, discusses the subtleties and nuances of newspapers as institutions and reporters as individuals that play upon how group leaders perceive and approach the press. Part IV makes explicit a point easily inferred from earlier chapters, i.e., the competition between the media, the plurality of political interests, and prevailing social norms figure in the success that resource-poor groups have in obtaining access. Part V adds such generalizations as "the initial interactions between resource-poor groups and

metropolitan newspapers occur as a result of group initiative or newspaper initiative or both" (p. 133), and "the likelihood that a pattern of interaction between a resource-poor group and a metropolitan newspaper will become regularized depends somewhat on how, when and with whom the initial contact between group and institution occurred" (p. 141). Since the author provides detailed summaries at the close of each chapter, the concluding chapter repeating the content of those summaries appears superfluous. The monograph leaves the impression that the raw material for an excellent journal article has been stretched beyond its limits; the didactic and redundant style combined with the publisher's overkill in openness of format, headings, and subheadings makes for an unfortunate outline-like quality.

The principal question raised in this study is what variables affect the relationships between resource-poor groups and newspapers? Credit Goldenberg with a meticulous and seemingly exhaustive listing - individual and group goals, public and private; nine specific group resources; four characteristics of nonmedia targets; a dozen or so identifiable properties of the newspapers; and the perspectives, role conceptions, sensitivities, capabilities, goals, attitudes, resources, and strategies of reporters. Almost obscured by this impressive inventory of variables is a concern that has occupied students of the press and politics at least since Walter Lippmann, i.e., how do political interests and journalists mutually take part in the collective enterprise of construing when a happening is an event, an event is newsworthy, and a newsworthy event is a story? Valuable though Goldenberg's exercise, it is typical of efforts that Herbert Blumer two decades ago labeled and criticized as "variable analysis." It does not answer the vexing questions raised by Lippmann and others. To be sure, an accounting of variables advances understanding so long as we regard the media only as Goldenberg does i.e., "as an intermediary mechanism between the governors and the governed" (p. 1). But as indicated in the works of Leon Sigal, Gaye Tuchman, Tamotsu Shibutani, Harvey Molotch, and Marilyn Lester, the social construction of political news is an inclusive, adjustive process and not simply mechanistic. Making the Papers contributes only marginally to clarifying that total aspect of relations between interest group and the press.

Dan Nimmo

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Barriers to Establishing Urban Ombudsmen: The Case of Newark. By William B. Gwyn. (Berkeley: Institute of Governmental Studies, University of California, 1974. Pp. 93. \$4.50, paper.)

This monograph carefully chronicles the success in authorizing the first municipal ombudsman in New Jersey and explains Newark's failure to fill the position. Originally assigned to assist and assess the operations of that city's prospective ombudsman, Tulane's Professor Gwyn has revised his report to be another study in the Institute's Ombudsman Activities Project submitted to the Office of Economic Opportunity. He examines the movement's development and the reasons for the particular type of office established by ordinance (text included as Appendix II). For complete coverage, the views of principal participants are offered.

Although the earliest proposal is traced to the wake of the 1967 riots, "race was not an important factor in the defeat of the Ombudsman" (p. 24). The detailed account begins with Mayor Hugh Addonizio's rejection of a state commission's recommendation for a Newark civilian review board and his endorsement of a local study urging an ordinance based heavily on Professor Walter Gellhorn's Model Ombudsman Statute. But Addonizio's plan was doomed when Newark could not secure the state legislation necessary to provide the subpoena power that was apparently required for OEO funding.

The bulk of the volume analyzes the efforts of Kenneth Gibson, Newark's first black mayor and a "technocratic moralist" (pp. 28-30), to overcome political attacks on the ombudsman idea resulting from the municipal council's hostility toward his administration. Because Gibson's commitment to the change was initially "tepid" (p. 11), OEO took the initiative in February 1971, proposing a two-year demonstration project. But the federal agency's objection at first that Newark's draft ordinance did not adhere closely enough to the Gellhorn model caused "a delay that may have contributed to the eventual failure of the whole scheme" (p. 13). Obligated by official legal advice to seek council approval, Gibson had to abandon his timetable and appointed the black selected by his search committee to another position.

Even passage of the ordinance (May, 1972) did not signify victory. Five months later, after several postponements of consideration and last-minute attacks by two municipal police organizations, Gibson's candidate — a black lawyer who had not resided in Newark since 1968 — was rejected as a "carpetbagger," 6-2

(one abstention). "That most of the council on this occasion were voting against the office of Ombudsman and not the person nominated to fill it is suggested by arguments recorded in the minutes" (p. 49), the author argues convincingly. Moreover, Gibson "appears to have made a mistake" in refusing to offer another qualified nominee (p. 67).

If establishment of a classical ombudsman and his appointment should depend upon a reforming mayor and a city council of traditional politicians, Gwyn recommends an executive ombudsman, over whom the council has no control.

A reading of Gwyn's well-documented case-study helps one understand the lack of demand in New Jersey for ombudsmen with any real authority. "Action Now," a program established (1971) with Model Cities funds, has been handling citizen complaints in Newark (Appendix I) and elsewhere. Several municipalities now assign such duties to administrators or even to so-called "ombudsmen." But the only significant change has been creation of a state Department of the Public Advocate (1974), including a Division to deal with executive agencies.

HARRIS I. EFFROSS

Rutgers University

Bureaucratic Politics and Foreign Policy. By Morton Halperin, with Priscilla Clapp and Arnold Kanter. (Washington, D.C.: Brookings Institution, 1974. Pp. 340. \$8.95, cloth; \$3.50, paper.)

Wisdom is the hallmark of this book. Its principal author is a man of impeccable credentials. An in-and-outer, Morton Halperin left Harvard to serve as Deputy Assistant Secretary of Defense responsible for political-military planning in the Johnson administration and, briefly, as a senior National Security Council staff member in the Nixon administration. Clearly he is an experienced and thoughtful participant-observer of bureaucratic politics and this book reflects his background. Using the antiballistic missile (ABM) controversy as a case study, Halperin explores foreign-policy making, emphasizing that the arena is largely closed to those not in the executive branch. This White House-Pentagon twilight world behaves according to the rules of "bureaucratic politics." The book aims at illuminating the policy process of this dark arena for the scrutiny of political scientists and the instruction of potential deputy assistant secretaries.

It is for this exposure of the foreign-policy process that Halperin deserves the most praise.

This topic is little understood because it is little known. While in the past five years Allison, Campbell, Destler, Hoopes, Janis, Rourke, myself, and others have joined Halperin in a growing movement to examine how the bureaucracy makes foreign policy, the quest is still merely exploratory. In comparison to the more accessible domestic-policy arenas, empirical data are sparse. Hence Halperin offers his reader unique insight into the process.

Among his most original insights, the author notes the strong parallels between Presidents Eisenhower and Johnson (pp. 108–109), the well-developed art of leaking information to news reporters (pp. 173–189), and the contrasts between military and civilian officials (pp. 227, 256–258). Contrary to popular myth, generals and admirals are less likely to be "good soldiers" than are their civilian counterparts.

As long as the reader seeks sagacity, the book proves excellent. The problems occur if he seeks more, for the methodology lacks structure, precision, and ties to the relevant literature. Indeed, no systematic overall theory is apparent. While specific generalizations abound, these rules of bureaucratic behavior do not add up to a unified whole - even an arbitrary, heuristic framework. Neither can these generalizations be called hypotheses for they are not falsifiable. The study also lacks a clear definition of its level of analysis. Sometimes the unit of analysis is a major organization such as the army, navy, or air force, sometimes a small Pentagon staff office, sometimes an individual man. Although the book contains frequent and extremely lengthy quotations of memoirs and syntheses of memoirs to provide factual examples, it is not well tied into the theoretical literature of political science. For instance, Chapter 14 considers field-headquarters relations. Although this is one of the classic issues of public administration, not one of the many relevant theoretical works is among the twenty-seven books cited.

If wisdom is sought, Halperin provides it. His book is chock full of perceptive insights tempered by first-hand experience. If systematic analysis is the goal, however, the book is less satisfactory. The study is uneven and disjointed. Indeed the argument presented yields little explanation of the ABM controversy that is supposedly the focus of the work. The concluding chapter unwittingly suggests that Richard Nixon's 1968 election and changing Soviet defense strategy had a greater impact than bureaucratic politics. Perhaps the book is best compared to the many memoirs it cites; in this category it excels for it is an analytical

memoir.

DAVID HOWARD DAVIS

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Policymaking by Plebiscite: School Referenda. By Howard D. Hamilton and Sylvan H. Cohen. (Lexington, Mass.: D.C. Heath and Co., 1974. Pp. 294. \$20.00.)

As the authors point out in their preface, school referenda are the most numerous examples of direct legislation in America, and perhaps the world. The approximately 1,000 school-bond, 2,000 school-budget, and 5,000 tax-levy elections annually exceed the combined number of referenda in city, county, and state governments.

This book, in attempting to inventory and analyze these referenda for both the professional educator and the political scientist, is clearly an ambitious undertaking. It makes a useful contribution in several ways. First, it is an excellent source book on state rules and regulations on school referenda, state referenda outcomes, and local case studies. Second, it presents an extensive review of the literature with an attempt to use the authors' original research to draw conclusions from the disparate findings of referendum studies. Finally, it offers an interesting methodological note in explaining the use of Q-factor analysis to uncover voter profiles in actual election situations.

The review of the literature and the campaign histories include some novel and intelligent conclusions regarding referenda campaigns and outcomes. The authors, both political science professors, rightly conclude that, contrary to popular academic belief, a large turnout will be associated with passage of a referendum and a low turnout with defeat in most urban areas. This happens because a low turnout electorate is skewed by an inordinate proportion of retirees and other persons opposed to taxation, while a high turnout electorate will include more members of minority groups that typically vote yes. One of the interesting findings from their own research in Austintown and Bowling Green (Ohio) is that there is a substantial amount of economic rationality in voting. They found that the less favorable an individual's tax/income ratio, the most likely he or she was to vote no.

The problem with the campaign histories and the review of the literature, however, is that it is somewhat artificially divided into seven chapters: The Decisions of the People: Voting Rates, Approval Rates, and Determinants; School Levy Elections: Strategies and

Campaigns; Bond Elections: Issues and Strategies; Communications, Knowledge, and Influence; School Referenda and Community Conflict; Voting Patterns - The Correlates; and The Voter's Decision-Making Process. As might be expected from this listing, there is both a good deal of overlap between the chapters and a lack of integration. Indeed, no real attempt is made to determine the dynamics of the voter's decision-making process. The chapter of that name is simply another review of the literature on the correlates of voting. Nor is much attempt made to integrate these chapters with the following chapter on voter profiles (obtained from Q-factor analysis) in order to come to some sort of conclusion about the decisionmaking process.

The chapter on Q-factor analysis is perhaps one of the more interesting in the book. Q-factor analysis involves having a set of subjects rank statements (the Q sample) about an event, issue, person, etc. The rankings by each subject are correlated with the rankings of each other subject and the resulting matrix of correlation coefficients is factor analyzed to obtain clusters of persons who ranked the statements similarly. The authors obtained four attitudinal factors in their analysis of the Youngstown tax levy controversy. These included (1) the "good citizens" who identified with the school establishment; (2) the "alienates" who were parochial, suspicious of authority, and populist in their orientation (also the only group opposed to integration of the schools); (3) an ambiguous or cautious group; and (4) the "caretakers" who favored maintenance of only traditional services because of their intense concern for economizing.

The authors proceeded from the Q-factor analysis of "how" the attitudes were sorted to a survey research concern for the frequency distribution of these factors in a given electorate. They analyzed an election in Austintown, Ohio, and found a strong relationship between the scaled attitudes and the direction of the vote. In Austintown, knowing whether a respondent was a "good citizen," "caretaker," or an alienated individual was more valuable information for determining the direction of the vote than were the typical demographic variables employed by most voting research. Because this book is also directed at professional educators, the authors recommended Q-factor analysis as an "intelligence" tool to guide campaign strategy in a specific election situation.

Despite problems in writing style and organization, this book represents a useful and needed addition to the rather sparse field of referen-

dum research. While it is flawed in the coherence of the theory presented, the authors must be given credit for bringing together in one book, the vast body of state rules and local campaign histories, and the academic voting behavior research.

CHRISTINE H. ROSSELL

Boston University

The Presidential Campaign: The Leadership Selection Process after Watergate. An Essay. by Stephen Hess. (Washington, D.C.: The Brookings Institution, 1974. Pp. 121. \$2.50, paper.)

Stephen Hess, a political scientist and a veteran of presidential campaigns, has undertaken a critical reflection on our presidential selection system. His essay focuses primarily on two questions: how well does this system work, and how may political parties reestablish their control over it? To this task, he brings not only first-rate credentials, but a cool, dispassionate perspective, an incisive, analytical approach, and a willingness to stick his neck out in making judgments and offering reform proposals.

Hess concludes that on the whole the selection system works well. He is hardheaded enough to characterize presidential campaigns as vulgar, costly, wasteful, chaotic, lacking in creativity and intellectual vigor, and failing to entertain. Nevertheless, he adds, they do expose the candidates adequately to the people and the people, in turn, to the candidates. Do the campaigns test sufficiently the key qualities personal, political, and executive - that he finds to be essential for a president? He concludes that the first two are adequately tested while the latter is not. Since, however, Mr. Hess regards character and "political instinct" as much more important for the voter to ascertain and for a president to possess, the failure to test for executive qualities is no great indictment of the selection system.

Unlike those who criticize campaigns as excessively long and too burdensome for the candidates and the electorate, Hess asserts the functionality of the campaign as an ordeal. Campaigns, he contends, serve as both testing and training grounds, enabling the electorate to learn about the candidates and affording the latter useful lessons about the country and its people; the length of campaigning only enhances such opportunities. At the same time, campaigns bring forth new policy initiatives and open up to greater scrutiny policy issues already in the public sector.

Hess is critical, however, of the system's ability to produce enough potential leaders. He would widen the pool of talent by expanding the number and types of professional politicians — and nonpoliticians — under consideration. Thus he offers an imaginative proposal: having special groups of politicians — mayors, governors, and Congressmen — as well as associations of businessmen, educators, and labor leaders jointly serve to nominate candidates to be considered by the national convention. Unfortunately, Hess fails to explore the nature and problems inherent in such innovations.

Hess's second theme is the value of more disciplined political parties and the need for their regaining control over the presidential campaign. Watergate, in Hess's eyes, was partly attributable to the divorce of the presidential nominating-election process from both political party and professional politician. Hess proposes to magnify the roles and importance of parties and professional politicians at the expense of the individual candidates, the mass media, the money-givers, and the public relations firms. Parties do have it within their power, he asserts, to regain control of the presidential campaign process.

To attain these ends, Hess calls for strengthening the parties' national committees and conventions. He favors a mixed public-private financing system in which the national committees receive and control the funds in presidential campaigns. He recommends that the national committees also propose a code of ethics and conduct, binding on the candidates for the nomination and election process and backed by the ultimate sanction - denial of the presidential campaigns. He recommends that the national committees also propose a code of ethics and conduct, binding on the candidates for the nomination and election process and backed by the ultimate sanction - denial of the presidential nomination! Bi-annual party conferences should examine the state of health of their parties and should prepare platforms for the upcoming presidential campaigns. The choice of the vice-presidential candidate should reside primarily in the national committees which should also be empowered by law to respond in an election year to television-radio addresses by an incumbent president of the opposite party.

Readers of Hess's essay may find some difficulty in reconciling the author's conclusions that the present selection process works rather well with his strong insistence on the need to reestablish party control. Moreover, some of his proposals seem to raise more

questions than they answer. On the whole, however, Hess's provocative, well-written essay on the presidential selection system deserves serious consideration.

ABRAHAM HOLTZMAN

North Carolina State University

Aspects of Early New York Society and Politics. Edited by Jacob Judd and Irwin H. Polishook. (Tarrytown, New York: Sleepy Hollow Restoration, 1974. Pp. 150. \$7.50.)

This brief volume is the consequence of a symposium held in October 1971 under the sponsorship of Sleepy Hollow Restorations. Aside from dealing with New York politics from 1689 to 1789, no common theme or methodological approach ties the different essays of this book together. The book is also unfocused in form as well as content, consisting of two perfunctory comments by the editors, four formal papers and two critiques, and an address by Richard B. Morris entitled "The American Revolution Comes to John Jay."

In a highly derivative but well-written essay, "New York in the American Colonies: A New Look," Milton H. Klein argues that colonial scholars have too exclusively emphasized developments in New England and the South and have not looked carefully enough at the Middle Colonies, especially New York, which, because of its heterogeneous character, presaged the kind of country America was to become. The point is well taken. Yet in pursuing it Klein tends to belabor the obvious, to superficially treat complex issues, and to never confront the meaningful and challenging historical problem of how a society whose intellectual heritage was community and consensus adjusted to and channeled its enormous diversity.

Patricia Bonomi's "Local Government in Colonial New York: A Base for Republicanism," is a knowledgeable survey of local institutions with particular emphasis on developments in Kingston before the Revolution. Briefly comparing the way local government operated in New York with that of England, Professor Bonomi finds the former to have been more vigorous and broadly based and takes issue with those historians who have stressed too exclusively the extreme aristocratic nature of politics in colonial New York. While there is much of value in what she says, I cannot help but feel she has pushed a useful point too far by downplaying or ignoring the role of the judges of the county courts who held their positions by appointment, the dominance of

the Whig theory of government and the role of deference, as well as the uneven distribution of wealth, status, and education — all of which in important ways affected how power was actually accumulated and used. This essay will undoubtedly add additional fuel to the already heated debate among historians over how much and what kind of democracy existed in colonial America.

In "The Age of Leisler - New York City, 1689-1710: A Social and Demographic Interpretation," Thomas J. Archdeacon uses a computer to analyze tax assessment roles and an early census of Manhattan. He argues that the Leislerian movement is to be explained mainly in terms of the resentments of the city's Dutch population over rapidly increasing English influence. Although Archdeacon's evidence clearly does seem to indicate "an unmistakable Dutch aura" among Leisler's followers, the significance of this for the movement is asserted rather than proved, while the economic sources of the conflict tend not to be sufficiently explored. Still, to those scholars who see enough conflict in early American history to reject consensus synthesis, but who do not find the class-conflict orientation of the Progressive historians acceptable, and who have therefore stressed the importance of religious and ethnic differences, this essay will prove a useful reference point.

A brief study by Edwin G. Burrows on "Military Experience and the Origins of Federalism and Antifederalism," uses commonsense arithmetic rather than sophisticated statistical analysis to examine the military careers of the fifty-four Federalists and forty-nine Antifederalists who were candidates for the New York Ratifying Convention. Burrows finds a strong correlation between Federalism and service in the Continental Army on the one hand and Antifederalism and the state militia on the other, and argues that the militia experience bred a localist political outlook while service in the Continental Army bred a more cosmopolitan outlook, and in the context of the late eighteenth-century America, a more nationalist spirit. This interpretation has already been stated by a number of scholars. Moreover, Burrows never confronts the sticky question of what effect a localist and cosmopolitan attitude had in predetermining the military choices of individuals, thus reversing cause and effect. Nonetheless this is a useful essay for it strongly supports what seems to rapidly be becoming the dominant way of interpreting the struggle over the ratification of the United States Constitution.

Bonomi and Archdeacon's essays are par-

ticularly valuable. One can only feel they would have been better served as journal articles where they would have attracted the immediate attention they deserve, rather than being buried in a book where they will have to wait to be discovered.

RICHARD E. ELLIS

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Bureaucratic Encounters: A Pilot Study in the Evaluation of Government Services. By Daniel Katz, Barbara A. Gutek, Robert L. Kahn and Eugenia Barton. (Ann Arbor: Survey Research Center, Institute for Social Research, The University of Michigan, 1975. Pp. 264. \$12.50, cloth; \$7.00, paper.)

As a government expands its activities of service and regulation, it expands the number of occasions for contact with its citizens about practical matters. Such activities as job training, medical aid, and driver licensing, important enough to justify government involvement, are important to citizens as well. From a citizen's standpoint, contact with a government bureaucracy is likely to be an important part of his experience of government overall. The authors of Bureaucratic Encounters believe that a study of citizens' dealings with public bureaucracies is important in determining: (1) the utilization of government services, (2) the quality of experiences with government agencies, and (3) public support for the political system.

The study focuses on Americans' experiences in seven areas of government service and constraint. The service areas are job finding, job training, medical aid, retirement aid, unemployment compensation, workmen's compensation, and public assistance; the constraint areas are taxation, driver licensing, traffic violations, and the police. Public experience with these government activities was gathered in spring 1973 as part of the Institute for Social Research's semiannual national Omnibus survey.

The study's questionnaire (reproduced in the Appendix) was extensive, and enumeration of the study's findings on utilization and experience alone takes the reader serially and unremittingly through ninety-seven pages. This long enumeration, however, contains a number of findings which challenge familiar stereotypes. Among the most interesting are data about the characteristics of users of the seven government services. Contrary, for example, to the "popular notion" (p. 26) that needy blacks prefer not to work is the finding that "twice the proportion of blacks as whites utilize public employment

agencies, and twice the proportion seek job training..." (p. 26). Nor did the study find support for the notion that minority groups and the poor are generally the heaviest utilizers of the services surveyed. Such characteristics as age, race, level of education, income, and occupation relate to differences in utilization of particular services, but service utilization overall is surprisingly generalized across these categories. These sorts of findings led the authors to assert that governmental services in the seven areas "...do not comprise a welfare state for the deprived and needy; they are the way of life for the whole nation" (p. 38).

The findings on experiences with government agencies contradict other popular notions. The respondents, for example, did not find service agencies to be slow and muddling in handling their cases, nor did their experiences reflect the stereotyped image of faceless, alien bureaucracies. Other noteworthy findings relate to perceived fairness of treatment and awareness of appeal procedures; interesting as well are the relatively unfavorable differences in findings for constraint agencies.

While, for example, almost 60 per cent of the service agency users "felt that their situations had been improved or their problems resolved by the agency they visited" (p. 115), nearly half of the respondents with experience with constraint agencies believed that their problems were worsened by the agencies' handling of them. There was a similar difference between service and constraint agencies in perceived fairness of treatment.

Most engaging, however, is the study's discussion of the nature of public support for the political system. In reference to David Easton's three levels of system support, the authors assert that it is necessary, in gauging the actual degree of support, to define indices which embrace the "actual cognitive structures" of system members (p. 140). The terms of theory must be defined in reference to measurable reality. Accordingly, the authors through factor analysis of survey responses arrived at nine attitude dimensions of system support. Based on both experience and general beliefs, the attitude dimensions represent sentiments of attachment to the political system, and include interpersonal trust, confidence in national leadership, symbolic nationalism, and evaluation of personal experience with government bureaucracy. The survey respondents were then related to the factors by age, race, income, occupation, sex, education, relative deprivation, and working status. There is not strong support in all dimensions from all categories of respondents, but lack of support is discontinuous. The political system appears by and large to be supported by various groups for various reasons. To be truly disaffected, one must be alienated multidimensionally.

Bureaucratic Encounters provides an outside-in view of governmental functioning in areas affecting the lives of ordinary people, and illuminates a common but important area of political experience. The quality of that experience, in contrast to all that has been written recently about the alienating effects of modern bureaucratic society, is surprisingly positive. Most important, however, Bureaucratic Encounters adds needed clarity and attitudinal substance to the study of system support. It is now possible to consider this fundamental political attribute in concrete, human terms.

DANIEL MADAR

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The Domestic Presidency: Decision-Making in the White House. By John H. Kessel. (North Scituate, Mass.: Duxbury Press, 1975. Pp. 149. \$7.95.)

This research monograph seeks to describe how domestic policy is formulated in the White House. The author observes quite correctly that more is known about congressional decision making than about White House decision making and sets out to provide some remedy. Wisely, he restricts the focus of investigation in several ways. Time: Only the Nixon White House. Policy area: Only domestic policy. Focus: Only the workings of the Domestic Council. What we have, then, is a case study, rather than a comprehensive and systematic inquiry. Yet this is a good way to proceed when not much prior work has been done.

The great danger in this approach is that the monograph easily degenerates into story-telling. Neustadt's Presidential Power is a case in point. Anecdote is piled upon anecdote, completely smothering whatever systematic theoretical formulations the work once contained. Fortunately, in Kessel, theoretical considerations and interests predominate over anecdotal components. Kessel can hold in check the anecdotal element because he approaches the subject matter not as a historian-journalist, but as a social scientist, with a theoretical orientation, explicit definitions, and specified hypotheses. He has an explicit methodology, and an awareness of the basic rules of evidence and inference. On these grounds alone, the book is a most welcome change from the more customary literature.

The author's conceptual apparatus is derived from group theory. The units of analysis are the staff members of the Domestic Council, but the level of the analysis is the group whose members share attitudes and have "a reasonably stabilized pattern of interaction..." Kessel goes on (p. 28) to state that "... our interest in more inclusive organizational units... lies in how the larger units constrained and delimited the activities of the Domestic Council staff."

The author's technical tools include depth interviews, scale construction, and correlational analysis, all of which appear to have been competently used. The interviews were conducted in 1972 with twenty members of the Domestic Council, up to and including Kenneth Cole, then Deputy Director under John Erlichman.

This book opens with a discussion of the history of executive staff organizations generally, and of the Domestic Council specifically. This discussion is placed into the context of the various factors (e.g., presidential character, power situation, calendar, etc.) which affect the president's effectiveness and the behavior of the President's administration.

Next the author examines the recruitment process (unsystematic) and background (highly educated, but otherwise fairly diverse) of the Domestic Council staff, the policy attitudes of the staff members, the communications patterns, and the power patterns. He finds (1) that fairly high levels of policy consensus existed on most policy issues (particularly economic issues); (2) that the leaders occupied the important communication positons; and (3) that the power patterns in the main followed the formal hierarchical channels (legitimacy) but that expertise also served as a power base.

A chapter entitled "The Political View from the White House" deals with the perceptions of the Domestic Council staff members of their tasks, and with the staff's perception of and relationships with such significant political entities as the president, voters, cabinet members, congressmen, party leaders, and lobbyists. Most interesting among the many findings are these two:

(1) The staff quite uniformly divided the political environment into three broad classes according to their relative importance: foremost, the president; second, voters, cabinet members, and staff colleagues; and only third, party leaders and interest group spokesmen.

(2) The staff members' perceptions of the president's policy position were fairly uniform and were similar in location to their own policy positions; and the staff members' perceptions of the public's policy preferences were generally accurate, except on the issue of "governmental activism," where they underestimated

the public's willingness to use government authority and resources to achieve desired ends.

The next chapter deals with the "Activities of the Council." It contains detailed discussions of the Council's major functions: clarifying options, determining priorities, backstopping the president, collecting information, and monitoring the agencies. The example of the development of revenue sharing is used to illustrate these functions. At its creation the Domestic Council was given an exceedingly broad mandate: "to serve every policy need from the first glimmer of a social problem through an exhaustive evaluation of the ultimate government response" (p. 81). This mandate, of course, was well beyond the capacity of a staff of twentyeight persons; the activities just listed had emerged as the major functions by the end of

In the last chapter, Kessel assesses the effectiveness of the Domestic Council in the period from its inception in July 1970 to the end of President Nixon's first term. This rather limited time frame was dictated, in the first instance, by the author's research calendar. But it was also determined by the Watergate-related resignations and shifts of personnel. The activities and effectiveness of the Domestic Council of President Nixon in his second term and of President Ford will require new investigations.

The effectiveness of the Domestic Council in its first two and one-half years is assessed from three perspectives: the needs of Richard Nixon, the needs of the voters, and the needs of the presidency. Kessel concludes that the Council served very well the needs Nixon had for a vast, but orderly array of facts, presented in written form, with decisions made in physical isolation. As a result, the Council "became one of the most important elements in the Nixon White House" (p. 114). Kessel also finds that the Council staff was fairly well attuned to voters' needs to have their policy positions understood and taken into account. The needs of the presidency seem to have been served better by the Council's staff than by other White House agencies. The Domestic Council staff seems to have been least affected by the generally prevailing paranoia of the Nixon White House, and was less ready than others to regard political opponents as enemies.

The Domestic Presidency is a small and unpretentious yet insightful book of considerable importance. It can be recommended not only to students of the American presidency but also to everyone who has an interest in political organization.

PETER W. SPERLICH

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The Washington Lobbyists for Higher Education. By Lauriston R. King. (Lexington, Mass.: D.C. Heath & Co., 1975. Pp. ix, 127. \$11.00.)

This is the most useful book yet to appear on Washington representation by higher education interest groups. Its ostensible purpose is not to analyze conclusively the pattern of relationships between higher education representatives and political decision makers, nor to contribute toward a theory of policy making or policy systems. Rather, the author intends simply to discuss the evolution of the Washington representatives from a barely visible presence in the late 1950s to a self-consciously political presence today. In the course of developing his thesis, Professor King deals incidentally with contextual forces, and refers briefly to the more salient aspects of the changing interrelationships between Dupont Circle and Capitol Hill.

The conceptual or theoretical approach of the book is to treat higher education as a "developing interest group," although there is insufficient attention to that notion to make a significant contribution to systematic analysis in political science. The historical context for the analytical portion of the book is thoughtfully and succinctly developed, with a demonstrated acquaintance with the important literature in the field.

The greatest virtue of the work, however, and the major intellectual stimulus to the reader, is the Section (Chapters 4, 5 and 6) which addresses important political changes on the part of the representative groups. King discusses what he names the "traditional," "pragmatic," and "activist" perspectives of the Washington representatives, with a focus on evolution from the first to the last.

The traditional perspective "holds that advanced learning has an intrinsic value for the cultural and intellectual life of society, and as such should compel the support of the major institutions, including government, of the society in which advanced education takes place" (p. 65). This perspective, says King, places higher education apart from other social and political interests, and manifests a pattern of "snobbish elitism." Higher education's occasional involvement in national politics, therefore, must eschew the "tedious work" of mobilizing support and engineering compromise in order to maintain professional dignity and transcend the "narrow concerns of selfinterest." According to the author, the key historical event which weakened this perspective was the mobilization of the higher education community behind enactment of the Higher Education Facilities Act of 1963.

The pragmatic perspective stresses the tactical need for forming and maintaining coalitions of higher education groups in order to speak in one voice to policy makers, but retains the view that when higher education becomes politically involved "it should do so in a rational rather than an adversary manner" (p. 71).

Finally, the activist perspective sees higher education as standing "in competition with other interests equally worthy of support" (p. 79) and therefore in need of more forceful and conventional lobby representation. This perspective is held by a small minority within the higher education community.

Dr. King focuses on the critical presidential transition, from Logan Wilson to Roger Heyns, at the American Council on Education, as a turning point in the triumph of the pragmatic over the traditional perspective at One Dupont Circle. I believe that he is too harsh, however, in his assessment of the Council's political leadership under Wilson. His discussion of the Council and its relationships with the more narrowly based groups is essentially accurate, but lends itself to much more theoretical treatment in terms of policy making and authority patterns between an interest-aggregating organization and a variety of interest-articulating groups within a developing policy system.

This book, in summary, is well researched, well written, and cogently argued. It should have pedagogical value that far transcends its focus on higher education.

LAWRENCE K. PETTIT

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Pollution, Prices and Public Policy. By Allen V. Kneese and Charles L. Schultze. (Washington, D.C.: The Brookings Institution, 1975. Pp. 125. \$6.95, cloth; \$2.95, paper.)

Pollution, Prices and Public Policy is another, but less technical endeavor in a growing list of works concerned with the merits of using the "market or pricing" approach as an alternative to the "centralized bureaucratic" approach in controlling environmental pollution. In part, this book by two economists, Professors Kneese and Schultze, is a summary and synthesis of previous work in the field. More significantly, it analyzes new material on the limitations of past and current federal legislation in water and air pollution control, legislation that relies on the bureaucratic approach, and then sets out to show in a systematic and logical fashion the advantages of

the pricing approach over the bureaucratic technique.

Starting with the point that the current legislation in pollution control is not cost effective (and that this is particularly defeating in troubled economic times), the authors state the theme of the book - national pollution control programs can be designed to reduce substantially the cost we are now paying to clean the environment. According to the authors, this is not being achieved. The current bureaucratic approach which relies on national ambient and emission standards and which is administered and enforced by elaborate implementation plans, extensive monitoring and testing, fines, court proceedings and large subsidies is simply not working effectively. The main reason for the failure of this federal legislation is that the pollution problem, as embodied in thousands of individual pollution sources, is too technical, too diversified, and too diffuse to be handled by a centralized bureaucratic method. Additionally, since only part of the cost of control is ever recorded in governmental budgets, with the burden of the cost being borne by the polluter and the consumer, control goals are not tempered by full cost and are likely to be overzealous.

What the authors recommend instead is regulation by the pricing system, or more specifically, effluent charges. They argue that if we put a price on air and water, just as prices are placed on other valuable resources, consumers will use them more carefully. This approach, by their logic, places the onus of the technological decisions where these decisions can best and most efficiently be handled, with the firm or the operator, leaving the responsibility for policy decisions, that is, how much control we want, with our representative institutions.

A number of previous studies on the cost effectiveness of effluent charges are cited as evidence for the utility of the market approach (although it is not clear whether these studies are based on actual institution of effluent charges or if they are premised on simulated data). Also, several specific suggestions for administering the effluent charge approach and for achieving its political acceptance are offered. For example, to administer it, the authors suggest a minimum national charge, plus — in the case of water control — regional

nical background that will enable them to analyze better these pricing approaches. Furthermore, they see the need for educating political representatives on alternatives to our timeworn bureaucratic solutions. Finally, they call for consideration of the pricing alternative in other policy areas, such as health insurance and energy policy, contending that the complexities of these areas are equal to those of the environmental area and thus not suited to the bureaucratic technique.

The arguments for the pricing alternative are worth close scrutiny if only to force more critical thought about government's role as problem solver. The arguments are also worthy of examination because the logic and evidence offered by Kneese and Schultze show that current approaches are inadequate to solve complex, diffused problems such as environmental pollution. There appears, nevertheless, to be an underappraisal of the bureaucratic approach and overappraisal of the pricing approach. First, the federal government's regulation of both water and air pollution has no doubt been flawed, but in many locales where water and air quality have been measured, improvement is evident. While Kneese and Schultze concede this improvement, they contend that it was achieved at too great a dollar cost and that fundamental changes in industrial processes or transportation modes were never encouraged. However, it must be remembered that these improvements are actual reductions accomplished in many regions of the country, not the simulated or localized achievements broached as evidence for the pricing system. Moreover, in actual nationwide programs, the slack and uncertainty that accompany program implementation are very likely to inflate cost.

A second point that needs emphasizing is that the pricing approach shares many of the same problems the current system has. There is still the political problem of setting charges or standards and the administrative problems of developing, testing, and implementing effluent measuring devices. In fact, it may be politically more difficult to accomplish this under an effluent system since polluters are actually assessed a dollar charge; whereas polluters may not have to pay under the standards approach unless convicted or put under court order.

A third important consideration in the assessment of the pricing method is that there is

in their cost structure. Furthermore, it is possible to transfer the effluent charges to the consumer, a strong possibility in oligopolistic markets.

In short, some of the problems associated with applying the pricing method do not receive adequate attention. Nonetheless, the market approach is one that deserves more study and in fact is already receiving attention through experiments in housing allowances and income maintenance. The book's rather critical treatment of current methods of massive government intervention coupled with its cogent presentation of the market alternative should stimulate greater discussion among scholars, policy makers, administrators, and citizens; and it is hoped that it will stimulate a more critical analysis of these alternative problem-solving approaches.

JOHN F. SACCO

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Harry Hopkins and the New Deal. By Paul A. Kurzman. (Fairlawn, N.J.: R. E. Burdick, Inc., Publishers, 1974. Pp. 219. \$9.75.)

Professor Kurzman states quite accurately that his "study makes no pretense at being an exhaustive study of Harry Hopkins in the 1930s." He passes lightly over Hopkins's early life and then in a series of sketches traces his development from a case worker in a private welfare agency to the head of the primary relief agencies of the New Deal. This description includes references to his administration of a division of the Red Cross and his directorship of the New York Tuberculosis and a Health Association as well as his directorship of the New York Temporary Emergency Relief Administration when Franklin D. Roosevelt was governor.

The significance of these activities in his conduct of the New Deal relief agencies is detailed. The first agency, the Federal Emergency Relief Administration functioned well because he assumed that the quickest way to the millions of needy people was streamlined federal administration using existing governmental machinery. In the succeeding agency, the Civil Works Administration and the Works Progress Administration, direct federal administration was used. The policy shifted from direct relief to work relief to work adjusted to the individual skills of people on relief.

Professor Kurzman shows special interest in the impact on social work of Hopkins's handling of these varied activities. He used many social workers along with others. Historically social workers had been case workers and administrators raising money for private welfare agencies. In the relief agencies they were called upon to take heavy responsibilities and learned the administrative skills associated with the federal civil service. From the New Deal on social workers in their many varieties of case work and many facets of public welfare administration have grown in numbers. Hopkins is given credit for having established the high degree of respectability which the profession now has. As a result of his vigorous administration of relief Hopkins is also given credit for breaking down the barriers among the federal, state and local governments.

The study notes that Hopkins's influence with Roosevelt went far beyond the relief field. In arguing for relief funds Hopkins is given credit for pursuading Roosevelt to adopt a policy of "deficit financing." He is given equally broad credit for his participation in the activities which resulted in the social insurances. It is noted he was unable to pursuade his colleagues in the administration of the wisdom of trying for health insurance at the time old age insurance was adopted.

Two other significant influences are described. In the struggle with Secretary Ickes over work relief as contrasted with funds for public works built by private contractors Roosevelt was pursuaded to take funds originally earmarked for Ickes and shift them to work relief. The second major shift in policy was his acceptance of the necessity for partisan politics to become significant in the Works Progress Administration.

There are a few words about Hopkins's desire to become president.

One is left with the feeling of having been let down when he has finished the book. These years were rich in the development of American social change and politics were closely intertwined. Hopkins was a major participant. Professor Kurzman has noted some of Hopkins's creative activities. But there is little of his fights and friendships with a crowd who believed in what they were trying to do. The New Deal hardly appears. Hopkins was one of some hundreds who made that era what it was. But Hopkins was a good deal more than just one of them.

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As Orange Goes: Twelve California Families and the Future of American Politics. By Karl Lamb. (New York: W. W. Norton, 1974. Pp. 322. \$8.95.)

Karl Lamb's book celebrates what he considers the "remarkable diversity" of political views found through intensive interviewing of twenty-three Orange County voters, all living within a few doors of each other in the affluent suburban housing tract of Ramona Heights. They have climbed the social ladder in different ways; they share common residential preferences, but their life styles show the persistent influence of ethnic, class, and regional origins. Their party loyalties are divergent and attenuated; their beliefs predictably are negative - politics is a dirty business, politicians cannot be trusted - and Watergate, about which they were asked a year after the main inferviews, elicited some feelings of betrayal, a few increments of activism, and much fascination. Still, in his closing chapters, Lamb finds in them a "yearning for integrity." They are ready for positive leadership - to restore balance in government, to foster individual achievement, to safeguard the environment. A future "coalition of hope" will surely include suburban voters like these.

So much for the cumulative thrust of Professor Lamb's inquiry. Twelve families make up his "microsample," as he calls it. No sample design is made explicit, nor does one find an interview schedule, although from time to time the topical focus of conversation is mentioned, and Lamb notes that he has based his own guide on the one used in Political Ideology by Robert Lane. The respondents seem to have been chosen in part, if one can judge by a recurrent theme, to show that the denizens of affluent suburbs are not necessarily rightist authoritarians. Orange County is well known as a "bastion of right-wing conservatism"; those in thrall to the ecological fallacy might expect any selection of its residents to be reactionaries, however well-educated, well-off and cooperative respondents they might be. Not so, we learn. "These voters are not strangers to decent impulses, nor to human sympathy. Such a statement only seems remarkable ... [given] ... the assumption of a political scientist like Walter Dean Burnham that repressive fascism will be supported by suburbanites as a matter of course" (p. 303).

Various considerations might well justify study of a small number of respondents intensively. Systematic and extensive studies of American electoral behavior have amply shown what behavioral science can contribute. Surely

something can be added by a phenomenological approach. The "human scale" has been neglected. Political inquiries call for empathy if social reality from the perspective of those experiencing it is to be fully appreciated. Reducing the comments of human beings to numerical values is a suspect research method to Lamb. Doesn't it somehow violate their humanity? His own work, then, is not a study in quantitative social science whose findings rest on probabilities. Rather, he aims to present a few voters as they really are - flesh and blood, not skin and bones. His subjects have "intrinsic interest," he hopes. His readers are to judge for themselves whether his word-portraits resonate with their own experience.

It is true that qualitative insights are not automatically gained by statistical summaries of cross-sectional responses to precoded questionnaires. Neither, however, does Lamb's assurance that attention is confined to a "microsample" guarantee that discoveries of more than local and limited interest will emerge. The new phenomenology also, in its search for valid generalizations, must find commonalities and tell us what they are. Tracing the vagaries of opinion and belief to be found in the talk of a few individuals is not much help for disclosing the modalities of American political orientation. Nor does the outer appearance of a man or woman, posing for a closeup portrait, reveal much about the psychological dynamics of his opinion formation, or about her readiness for political action. The structuring vision of firstrate novelists like Dostoyevsky or Conrad or Proust can make their characters real for most of us. But what Beatrice Webb called "truthtelling fiction" often becomes flat and onedimensional, even if the author is Dickens or Trollope. Political scientists who have worked with a few people studied in depth have been most successful when they were explicit and cogent about their own structuring vision and used their evidence to exemplify its empirical content. They have not let the subjects speak for themselves, nor have they introduced a sequence of straw-man arguments, stereotypes, or simplistic commentaries to be shown up as unable to account for even a few "real" people's views. Although Lamb's respondents are well known to him after many hours of informal conversation, his efforts to recreate them for us do not come off too well. He lacks the evocative power of Lane or Riesman, although he writes with clarity and conciseness. He is not very careful about definitions (on page 191 privatism includes localism, but four pages later it is out) nor about checking his own evidence (compare what Marie and Arnold think on pages 142-143 with Lamb's report on page 175 that they don't feel poor people somehow deserve their fate). He sometimes resorts to quantitative summaries, for purpose left unclear: "Ten of the respondents, or nearly half of the microsample, express intense pride in their American citizenship.... Six are immigrants or the children of immigrants..." (p. 168). We are not told how many of the other thirteen are second generation.

Lamb's cases, oddly enough, are presented with scarcely any history. Their topographical origins are sketchily and unsystematically noted; the paths by which they reached their present homes are only occasionally and partly charted. No processes of political socialization are described. "Party identification is absorbed from one's parents and other aspects of the childhood environment," he tells us, seemingly unaware of the spate of inquiries into this uncertain connection. For none of his people does he tell us much more. One is said to have become a Democrat to rebel against his Republican precinct-worker mother, but this is explored no further. Nor do we learn of the coming generation. Their offspring seem to have no aunts or uncles, no grandparents. Despite the ethnic diversity in Lamb's microsample, his discussion of privatism does not touch at all on the possibilities of differential "other-regardingness" raised by Wilson and Banfield. Lamb does turn to the classic studies for ideas. The American Voter is source for the notion that "party identification may be a more powerful psychological force than the voter admits, even to himself." With V. O. Key, he holds that "voters choose, stay with, or switch to parties nearest their attitudes or convictions." From his own work, Lamb finds that people have "opinion molecules" rather than consistent views:

The riots at the 1968 Democratic convention had to be supported by Russia and China; but it's a mistake to blame all America's ills on the Communists... International bankers obviously profit from wars, but this phenomenon is not related to the support given by American Jews to Israel. At one moment, senile southern senators are identified as "running the country"; at the next, international bankers rule the world. The Marcuses are not hampered by a requirement of logical consistency" (p. 219).

Neither here nor elsewhere do we learn what logic unifies the political world for Karl Lamb. Presumably it is superior to the Marcuses'.

For the next year or so, this study will help to provide undergraduates with up-to-date illustrations for propositions and insights housed in the now classic studies of American opinion and voting behavior. Of course, those classics included a lot of illustrative quotations also — and they were not badly chosen.

ELIZABETH WIRTH MARVICK DWAINE MARVICK

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Phase II in Review: The Price Commission Experience. By Robert F. Lanzillotti, Mary T. Hamilton, and R. Blaine Roberts. (Washington, D.C.: The Brookings Institution, 1975. Pp. 209. \$9.95, cloth; \$2.95, paper.)

Conventional wisdom has it that governmental price and wage controls don't work, or else they do work and are a bad thing. Nevertheless, the federal government periodically has felt compelled to supplement fiscal and monetary policies with direct action against inflationary wage and price changes, sometimes by exhortation and threat, sometimes by legal regulation. Academic critics, content that such steps were meaningless or unwise, rarely have bothered to analyze alternative regulatory standards for price and wage controls, or to discuss the political ramifications of the institutional arrangements that have been or might be utilized by government controllers. The Brookings Institution's current series of Studies in Wage-Price Policy, however, has begun to address these problems, apparently on the assumption that some sort of incomes policy will inevitably be with us. The series includes the excellent Exhortation and Controls (Craufurd Goodwin, ed.), comparing the wage-price policies of each national administration since 1945, and separate volumes on each "Phase" of the Nixon Administration's program. In Phase II in Review, Lanzillotti, Hamilton, and Roberts, economists who served on the Price Commission, focus on the period (Nov. 1971-Jan. 1973) which saw the most serious effort to develop and enforce an effective regulatory regime. They set out to describe how federal price control actually worked during Phase II. what theories underlay the Price Commission's policies, what problems and economic consequences ensued, and what can be learned from this massive experiment in regulation.

The unarticulated theory of the Price Commission's regulations, one gathers, was to produce pricing behavior similar to that which would prevail under perfect competition. Price increases would be permitted, but only as "justified" by increases in a firm's unit costs. Furthermore, no firm could raise prices if that would increase its profit margin. This controlled "pass-through" of costs would avoid

production-distorting cost-price squeezes resulting from increases in factor prices not subject to controls (such as imported commodities, raw agricultural and forest products, and taxes) but would not allow a firm to reap windfall profits. Unfortunately, the authors suggest, the Commission failed to implement this theory fully, bowing to a variety of practical, administrative, and political considerations. Most firms, it turned out, had unreliable data on unit costs and productivity for each product; the commission therefore adopted regulations which allowed more flexibility in calculating cost passthroughs. Processing thousands of individual price-increase applications threatened to hamstring both the firms and the Commission staff; the Commission therefore negotiated flexible pricing agreements with large multiproduct firms. These departures from "economic logic," the authors charge, were "too generous." Economic theory suggests that regulatory energies should not be expended on competitive markets, like retail trade; nevertheless, political considerations, the authors state, led the administration to extend coverage to retail prices with bad results. Retailers, pursuant to the regulations, passed along cost increases from the unregulated farm sector; it therefore appeared to consumers and workers that price regulations were ineffective, while wages were effectively controlled. Labor representatives quit the Pay Board. Small firms ultimately were decontrolled.

Whether strict adherence to "economic logic" is possible, or would have avoided these problems, is doubtful. Since regulators lack perfect information, and stringent pricing rules risk allocative distortions or noncompliance, regulators are always likely to "err" in the direction of "generosity." In the absence of a completely closed system, the cost pass-through theory fails to confront consumers' and workers' concern for distributional equity - the central political problem of governmental wage-price controls. By concentrating on price control economics alone, Lanzillotti et al. provide little insight into the political dynamics surrounding the problem of equity during Phase II, and do not suggest rules or institutional arrangements which might provide solutions.

The book does offer an interesting analysis, based on several econometric models, indicating that despite the relatively moderate Price Commission regulations, the rate of consumer price increases (except for the basically unregulated food sector) was about 2 per cent lower during Phase II than it would have been without controls, and that the controls contributed to recovery from the 1969-70 recession. The

authors report relatively few controls-induced distortions in production. Why then were the Phase II controls abandoned? Were they becoming more disruptive as the economy approached full production, and if so, could sensible adjustments have been made? Was the decision to loosen controls in January 1973 based purely on a postelection return to orthodoxy by Nixon and his economic advisers, or were there persuasive business-community complaints behind the change? As insiders, the authors might have addressed these broader issues fruitfully. Instead, they provide a rather formal and narrowly focused analysis of the Price Commission experience, of interest to economic historians and future regulators, but a disappointingly limited contribution toward understanding the politics, potentials, and limits of governmental wage-price regulation.

ROBERT A. KAGAN

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Racial Conflict and the American Mayor. By Charles H. Levine. (Lexington, Mass.: D.C. Heath & Co., 1974. Pp. 149. \$13.50.)

Are political science models, approaches, and techniques appropriate to studying mayoral leadership in older American cities where racial polarization has occurred? Charles Levine thinks not. Levine critically evaluates the pluralistic model of mayoral leadership and presents alternative models of mayoral leadership within racially polarized contexts. The author draws upon case studies of three cities and their mayors - Carl Stokes of Cleveland (1967-71); Richard Hatcher of Gary (1967-) and George Seibel of Birmingham (1967) - to illustrate the problems of applying a pluralist model of governance, positing that "The obstructionist tactics of racial politics help to explain political immobilism in many American cities and the demise of innovative-minded mayors" (p. 23).

The book deals with four central concepts: leadership, race relations, pluralism, and public policy. Levine examines leadership from the perspective of mayoral enterpreneurial qualities — goals, resources, and leadership structures. His central hypothesis is that under conditions of racial conflict and polarization, opportunities for mayoral leadership are vastly minimized. When scarcity of resources grows and conflict over allocation expands, Levine finds the pluralist model to be deficient in relating such an urban setting to leadership performance. Consequently, the pluralist model

is also deficient in measuring leadership effectiveness.

Levine studies mayoral leadership in the contexts of polarized cities (Gary and Cleveland) and a pluralized city (Birmingham). He assesses the effectiveness and policy impact of their mayors both from a pluralistic perspective and what he terms a "conflict perspective." In applying these two frameworks, Levine finds significant differences in leadership performance. The author concludes by developing a rough sketch of a contingency theory of mayoral effectiveness.

This is one of the better books written about mayoral leadership and the changing urban environment. Like Kotter and Lawrence's book, Mayors In Action (1974), Levine draws upon a rich interdisciplinary background to take a fresh look at the relationships of environment, leadership, and politics. The author's style makes rough reading in early chapters, torn as he is between demonstrating a grasp of research theories which relate to the subject and writing clear, concise, and understandable prose. Levine has some meaningful points to make, but they are frequently obscured by jargon and rather vaguely applied concepts. What he means by a "conflict model," for example, is not fully developed, and to the extent that it means how well the mayor mobilizes the black community and distributes rewards largely to that constituency, this definition comprises a very narrow, and even highly normative assessment of effective leadership performance. This is not a model. Rather, it is a value-laden judgment about the desirability of such a leadership style (as opposed to be the pluralist model which may have normative characteristics as well) without much concern over the costs, trade-offs, or even policy consequences of this approach to mayoral leadership.

Levine's work raises numerous questions concerning how political scientists study urban governance. Many older cities are operating under the new, more intense politics of scarcity, declining tax bases, heavy service demands, growing black population and declining white population, high unemployment, and loss of manufacturing jobs. Such cities are ripe for more open conflict, with new more intense groups clashing more directly as they contend for or resist change. A fragile polity is highly susceptible to polarization and increased hostilities with the consequence that mayoral-led coalition building and bargaining opportunities are greatly reduced. Whether or not contingency modeling will be any more helpful to political scientists in exploring the new urban

terrain can be seriously questioned. Available descriptions of mayoral behavior may well be what Levine calls "partial, static, and superficial at best" (p. 131), but contingency theory typically leads to the conclusion that "it all depends" and we're back to situational analysis. Besides advocating the application of contingency modeling as a means to develop a "science of mayoral leadership," Levine also wants political scientists "to better service political leaders ... rather than use political leaders mainly as research subjects," and "to reenter the world affairs with something unique to offer rather than function merely as informed citizens" (p. 141). These exhortations, well intended as they may be, smack of social science engineering at its worst, and a misunderstanding of the uses and perhaps limits of social science research at best. On the whole, however, Racial Conflict and the American Mayor takes us beyond the world of "Who Governs" into an uncharted research field which is a major contribution in itself.

DONALD HAIDER

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Public Planning: Failure and Redirection. By Robert A. Levine. (New York: Basic Books, Inc., 1972. Pp. 195. \$6.95.)

Most public bureaucracies have been created with the intention of directly and indirectly serving the public on a permanent basis. The fundamental planning behind the organizational structure, policies, and procedures is critical as many of these programs become institutionally petrified beyond the point of making effective changes and corrections. When intended plans become obsolete or unworkable, the forces of public immunization prevent timely remedial action. The range of purposeful functions falling into the planning dilemma category varies from those of simultaneous similarity to those of pure contradiction.

Mr. Levine, the author of *Public Planning:* Failure and Redirection, has extracted several generalizations from a series of cases in which public planning has failed and in which it has succeeded. He contends that, "to be successful, planning has to change its simple-minded premise of a world in which planners lay out optimal courses to be selected goals, bureaucracies translate these courses into action, and implementation follows as neatly as building construction follows a blueprint. It will have to move toward the sophistication of recent descriptive analyses" (p. 14). Using this contention as a foundation for public planning im-

provement, the author sifts through his cases in order to present a corrective to the ills of planners and analysts.

The shotgun approach is used in criticizing the federal bureaucracy and corresponding situations at the state and local levels. It is clear that the well-documented and highly criticized planning faux pas of monolithic public institutions provide a fertile ground for inductive reasoning. The cases concentrate on tax policy, military planning, education, manpower, urban renewal, public assistance, and general fiscal and monetary policy. There seems to be, however, a lack of continuity in drawing parallels among these cases. The wide variety and scope of subject matter makes the shotgun approach less penetrating and the proposed solutions less convincing.

The book is arranged to show that highly administered systems that were planned and implemented "to operate through detailed application being levied on one set of people called a clientele by another set of people called officials" (p. 17) have generally failed. The solution to this problem rests with Adam Smith's ideas on the principle of self-interest and the mechanics of the competitive market system. The public planning function can be improved as generally stated in the book:

... the beginnings of a way out of some of our current dilemma lie in the market-like and bargaining systems that combine the workable features of decentralization, self-administration, personal economic or political motivation, and the gross application of public policy rather than systems administered in detail by public officials to private clienteles according to plans laid out in detail by public planners (p. 23).

Although the author realizes that the market mechanism is not a universally applicable solution, he advocates injecting "bureaucratic competition" internally or externally to the public organization where there is redundancy or overlapping. He suggests that the planner make this concept a part of his tool kit in examining alternative solutions to planning problems.

Mr. Levine is too idealistic, given the complexity of the many situations involved in public planning. Although he recognizes this complexity, he seems incapable of dealing with it. His philosophical solutions are analogous to Elliot Janeway's recommendation to investors to "buy low and sell high." The questions the investor must answer by himself are how low is low and how high is high? The investor needs a great deal of sophistication to recognize the quality, the relative market of comparable investments, and expectations of conditions which could affect the investment. Even after

an analysis is made, an element of risk remains. The public planner covers a larger gamut of variables. He must be sensitive to internal as well as external factors that affect desired goals, and the risks are sometimes greater.

In general, as the passage quoted above illustrates, the writing is difficult to follow. An analysis of one or two well-known and controversial programs would have made the book more interesting. Furthermore, a simple approach to Mr. Levine's ideas would have been more stimulating to the reader, especially if he was a planner involved in the specific topic.

MELTON L. SPIVAK

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Policy Studies in America and Elsewhere. Edited by Stuart S. Nagel. (Lexington, Mass.: D.C. Heath & Co., 1975. Pp. 229. \$16.00.)

This volume is the first in a series of books emerging from the pages of the Policy Studies Journal and the enterprising efforts of its coordinator, the Secretary-Treasurer of the Policy Studies Organization, Stuart Nagel. The present work stems mainly from the first issues of the Journal so that many of the essays included here were published initially in 1972-73. Most of them have not been much altered or brought up to date, and consequently the usefulness of the several literature reviews is considerably curtailed. Thirteen essays are included, five on theoretical or general issues involved in policy analysis, and the rest dealing with particular areas of substantive policy. Some of the essays were originally written as two or three separate and distinct pieces by different people, and little effort seems to have been made to make them into coherent presentations. Indeed, one problem throughout is the minor irritations of mistakes in grammar, syntax, and flow of ideas that effective editing should have corrected.

Some of the individual essays are helpful guides to the literature of a subfield or to the institutions and resources available for the interested investigator. Especially helpful are the comments by Richard Rose on British policy studies, Dean Mann, Geoffrey Wandesforde-Smith and Lennart Lundqvist on environmental policy, Richard Merritt on foreign policy, and Theodore Marmor and Hugh Heclo on poverty and welfare policy. Discussions of theoretical issues and approaches and of methodological matters are disappointing and well short of the state of the art.

The larger question raised by this volume and generally by the burgeoning literature and program expansion in the field of "policy analysis" is whether there is, in fact, a field with enough intellectual bounds and coherence to encourage us to hope for important growth in policy theory. On the strength of this volume one might conclude there is not.

Here as elsewhere the efforts to construct broad theoretical structures very often sound just like general theories of politics and lack any particular policy component. Some policycentered theoretical efforts have defied operationalization and remain, however seductive and plausible, untested and untestable. What theoretical strength there is in the policy literature seems largely to be found within particular subfields. Budgeting studies, for instance, have developed a substantial, if contradictory, bundle of theoretical arguments. But welfare budgeting may follow different political rules from defense budgeting. We have long supposed that foreign policy processes were distinguishable from those governing domestic issues. Environmental issues cannot easily be subsumed under the same theoretical rubrics that serve educational issues.

This volume helps persuade me that it might be useful, and sooner rather than later, to acknowledge that if we investigate one policy sector (recognizing always, of course, that boundaries are blurry) we will not necessarily learn anything about another; that the best way to study policy seriously is to study it in the particular; that, in short, there is no intellectual field, at least not for research purposes, called policy studies. Rather, there are many areas of policy that richly repay the investigator who is willing to master the substantive details on which the play of contending interests, the politics, the outcomes and the impacts all depend.

ROBERT H. SALISBURY

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Maryland and the Empire, 1773: The Antilon-First Citizen Letters. Edited by Peter S. Onuf. (Baltimore, Md.: The Johns Hopkins University Press, 1974. Pp. 236. \$10.00.)

Ironically, as political scientists have moved away from an earlier emphasis on abstract theory toward a more sophisticated understanding of the realities and complexities of political behavior, historians concerned with the American Revolution increasingly have become addicted to a one-sided emphasis on the intellectual theorizings of the Founding Fathers.

Formerly, under the influence of Beard and Jameson, American historians derived their causal explanations primarily from economic and social factors. Now, under the influence of Bernard Bailyn (who is not responsible for the excesses of his disciples), the pendulum has swung toward a dependence on intellectual abstractions and constitutional theorizings as the primary determinants of political events during the formative years of our national development. Both the introduction and the content of this edition of the literary exchanges between two of Maryland's leading Revolutionary figures give solid evidence that it is time once again for the pendulum to reverse itself, this time, one hopes, stopping at a point somewhere between the extremes of earlier followers of Beard and Jameson and the current excesses of Bailyn's disciples.

In 1773 Daniel Dulany, author of the most effective protest against the Stamp Act of 1765, was Maryland's accepted intellectual leader. When the Legislature failed to establish a list of fees to be collected by the colony's officeholders in the course of their duties, Governor Eden opened a lengthy controversy by proclaiming the establishment of a set of fees on his own authority. Dulany rose to the defense of the governor in a series of exchanges in the Maryland Gazette, while Charles Carroll – fifteen years his junior and a Roman Catholic – responded with charges that Eden had usurped powers properly belonging to the Legislature.

As Peter Onuf points out in his introduction to this reprinting of the Dulany-Carroll exchanges (previously published in 1902, but without annotations), the "issue was peculiarly local; its implications, however, were as large as the first British Empire, and central to the constitutional crisis that destroyed it" (p. ix). In his thirty-page introduction, Onuf puts aside the "personal bitterness" and the "insult, innuendos, and recrimination" which "seem to overshadow the discussion" and concentrates instead on the "two diametrically opposed and ultimately irreconcilable ideas of the constitution" which underlay the debate (p. 16). In doing so, Onuf adds to our knowledge of the intricacies of Revolutionary constitutional theorizings, but he misses an opportunity to bring to light the very real and very human complexities of colonial political life.

Onuf correctly notes that Dulany was "the defender of established government" (p. 17) while Carroll was a member of an embattled Catholic minority. But, as the articles in the Maryland Gazette make clear, the debate between Dulany and Carroll involved far more than simply a mirror image of "the country and

court philosophy which dominated English politics in the eighteenth century" which Onuf ascribes to it (p. 17).

The issue under debate was control of revenue, and both Dulany and Carroll were fully aware of what was at stake. Carroll complained that in Maryland "government is almost independent of the people," and "if it can settle their fees without the interposition of the legislature, administration will disdain to owe" any obligation at all to the people (p. 214). Dulany answered that neither "patriots" nor legislators were to be trusted any more than governors and judges, and that legislative supremacy was no more desirable than executive domination. The two men were not debating abstractions; both understood that the exercise of power is not a theoretical problem. The personal "insult, innuendo, and recrimination" which Onuf feels will be of interest only to the superficial reader are in fact essential elements in this debate between two men who represented not simply different constitutional theories but bitterly competing political factions and contending interest groups.

As this literary exchange records, the dispute with England - and Dulany's mistake in siding with Governor Eden - gave the representative of the Catholic minority faction (and allied interests) an opportunity that might otherwise not have been available. How well Carroll took advantage of his opportunity is readily apparent in these selections from the Maryland Gazette. Onuf's well-written but one-dimensional introduction offers little that is new. But the Antilon-First Citizen letters which record the heated exchange between Dulany and Carroll are in themselves worth the attention of anyone interested in gaining additional insights into how the crisis with England merged with and contributed to dramatically accelerated changes in the political life of the emerging American nation.

THOMAS C. BARROW

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Public Claims on U.S. Output: Federal Budget Options in the Last Half of the Seventies. By David J. Ott, Lawrence J. Korb, Thomas Gale Moore, Dave M. O'Neill, Attiat F. Ott, Rudolph G. Penner, and Thomas Vasquez. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973. Pp. 218. \$3.75, paper.)

The American Enterprise Institute has been described as a right-of-center "counter-Brookings." As one might expect, this volume is

something of a counter-counterbudget. Despite its subtitle, the study focuses upon the 1974 budget much as the Brookings products have dealt with the annual budget documents. Rather than engaging in wide ranging speculation, the AEI scholars confine themselves mainly to a critical examination of the specifics of the 1974 Nixon administration recommendations, looking more for evidence of waste than for missed opportunities.

To some extent, this conservatism can be attributed to the circumstances of the time: 1974 was a year for keeping the lid on expenditures, even Brookings agreed. More importantly, however, the AEI analysts appear to share a conservative disposition toward government and its role. On the whole, they find that government is probably doing too much especially, but not exclusively, in the area of social programs. Worse, what government does do, it often does not do well: needed government activity (e.g., in support of agriculture) is often seen as going not only too far, but in the wrong direction. Great concern is shown for the health of the private sector, and fear that government will take over legitimate private functions (e.g., health insurance and care). Unfortunately, this general perspective, which seems to be increasingly fashionable and perhaps even influential in the middle 1970s, is not clearly spelled out. It must be inferred from the disparate essays on major budgetary categories, for the editor attempts little real summation or synthesis.

The most common recommendation throughout is that the budget be cut. Thus, in the areas of defense (the B-1, among other things, is deemed expendable), agriculture, science, technology, industry, manpower, and housing, the AEI analysts suggest that we would be better off without some of what the Nixonians were willing to propose. In most cases, policy alternatives not included in the administration's budget proposals are not discussed, but there are exceptions. In agriculture, the idea of providing support tied to the farmer, not the farm, is proposed as a temporary measure, but is scarcely elaborated. In education, the idea of a voucher system for primary and secondary education is considered at length, and is viewed with favor for its potential for encouraging the private sector and providing consumers of education with a wider array of options. The plan is not fully endorsed, apparently because it would be expensive. National health insurance and income maintenance alternatives are also explored, but major departures from the status quo are rejected in favor of modest modifications.

These essays - ten in all, counting an introduction and a none-too-helpful "overview" - follow a common format, moving from projections (through 1981) to point-by-point analysis. The projections are of little interest today, because too many unanticipated events have occurred since 1973. The analyses, each by a different author or authors, are uneven, but all are at least reasonably thorough discussions of the substance of the 1974 budget. Their most common shortcoming is in detailing too few alternatives, but the most serious weakness overall is the book's failure to explicate the philosophical basis for the generally conservative approach taken. The explanation that the various authors would not always agree in detail rather misses the point. If the volume has enduring value - i.e., if there is any reason now for us to examine a study of the 1974 budget - it is because such efforts can be, in effect, works of applied political philosophy. This one is that, and it is interesting mainly for that reason. Future efforts would be more useful if, in this respect, more explicit.

CHARLES WALCOTT

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Mediacracy: American Parties and Politics in the Communications Age. By Kevin P. Phillips. (Garden City, N.Y.: Doubleday, 1975. Pp. 246. \$8.95.)

Kevin Phillips is a young newspaper columnist, author of the much-discussed *Emerging Republican Majority* (1969), a former appointee in the Nixon Administration (before he resigned in disgust in mid-1970), and a leading right-wing populist intellectual.

As early as 1970, National Review editorially expressed some reservation about Phillips's class-oriented mode of political analysis, which it characterized as "a sort of neo-Marxism cum statistics" (see: "Marx Goes Statistical," NR, 22 [July 14, 1970], 720). I suspect that one of the causes of National Review's discomfort was not Phillips's attention to class, but his preference for the "wrong" class - namely, the working or lower-middle class. More recently, Phillips has had a falling out with America's pre-eminent conservative spokesman, William F. Buckley, Jr., whom Phillips has portrayed pejoratively as a culturally chic elitist. Buckley, in turn, called Phillips an "eccentric theorist" and a "voice from Philistia" whose writings are "orotund" (see: Buckley, "A Voice from Philistia," NR, 27 [August 15, 1975], 900). These squabbles indicate that Phillips is no run-of-the-mill conservative. Furthermore, he is

no ordinary pundit, for as Walter Dean Burnham of M.I.T. says in his jacket blurb, Phillips is "one of about five people in the United States who have a really good understanding of American electoral politics."

Phillips states the thesis of this book in these words:

During the previous century, the nation's economic elite was conservative, and liberal-conservative struggles were rooted in that economic context. This book will examine the rise of a 'new class' of affluent liberals — and the impact of that rise. A new correlation is arising — among education, wealth, and liberalism — to replace the old one — among education, wealth, and conservatism. And, on a number of issues, opposition to liberal elitism is strongest among the groups historically in the vanguard of opposition to conservative economic elites (pp. 1–2).

There is, indeed, a good dose of neo-Marxism (usually unacknowledged) in Phillips's analysis of American politics. It is axiomatic for Phillips that "economic upheaval and political change go together" (p. 13). With the Industrial Revolution, says Phillips, manufacturing became the dominant economic factor, and political power was transferred from a rural-based aristocracy to an urban middle class. With the post-industrial Revolution (which America more than any other country - is now going through), the production, consumption, and dissemination of "knowledge" is becoming the dominant economic factor. As a result, political power is being transferred from the conservative business elite to the knowledge (or "liberal") elite. By 1980, it is projected that roughly 40 per cent of the U.S. gross national product will be accounted for by the shaping and marketing of ideas, information, and services, but only 25 per cent by the manufacture of goods. The new knowledge elite will - presumably in the fashion of all elites - promote its own interests: "Instead of having a vested interest in stability, as did previous conservative business establishments, the knowledge sector has a vested interest in change - in the unmooring of convention, in socioeconomic experiments, in the ongoing consumption of new ideas" (p. 17). If "liberalism" connotes an openness to change, the knowledge elite cannot help but be liberal, for "change is as essential to the knowledge sector as inventory turnover is to a merchant or manufacturer. Change keeps up demand for the product (research, news, theory, and technology)" (p. 33).

Phillips foresees a shift in the meaning of the terms "liberal" and "conservative" — hereto-

fore defined according to social relations obtaining in the industrial era. In the post-industrial era, liberals will divide into a New (knowledge elite) Left and an Old (blue-collar) Left. Conservatives will divide into an Old (business elite) Right and a New (anti-elitist) Right. Phillips foresees an intensification of ideological politics based on class conflict. As the elitists of the Old Right and New Left coalesce, the populists of the Old Left and New Right will converge. Although the populists will be "conservative" on social and moral issues, Phillips leaves no doubt that the New Right will be "profoundly unconservative" (p. 206; italics in original) in that it will favor an activist New Deal-style government which will control the economy. Moreover, the New Right will push for *public* control of the free-enterprising media elite.

In contrast to his *Majority* book, Phillips does not expect that the Republican Party will be the vehicle for transferring power to the people: "despite the shift in control of the national party from the northeastern establishment to the new forces of the South and West, one can only say that power has passed from the boardrooms of Manhattan and the clubrooms of Boston to the petroleum clubs of Texas and the defense industry suburbs of California; as of 1974, it has not passed to blue-collar workers, northern urban ethnics, or southern Wallaceites" (p. 197). Indeed, Phillips speculates that the Republican/Democratic party system may itself be obsolete.

Phillips's book is thought-provoking, but it is not as persuasive as it might have been. It is not well organized. Its tone is too often flippant and chatty. The book abounds with maps, charts, and poll data, but too often we are not told the sources of the information. Too often evidence is marshalled for the purposes of polemical generalization and speculation, omitting consideration of exceptions, nuances, and other complexities. But despite these difficulties, this book will serve as a foil for significant controversy.

DALE VREE

Stanford University

Electoral Reform and Voter Participation: Federal Registration — A False Remedy for Voter Apathy. By Kevin P. Phillips and Paul H. Blackman. (Washington, D.C.: American Enterprise Institute for Policy Research and Stanford, Cal.: The Hoover Institution on War, Revolution and Peace, 1975. Pp. 135. \$3.00, paper.)

In this small book Kevin P. Phillips and Paul H. Blackman examine turnout in U.S. elections, assess the importance of various factors affecting it, and review proposals for "some version of federal voter registration" (p. 1). In pursuing these objectives, Phillips and Blackman discuss the law and practice of registering voters in various states of the union, the United States in earlier periods, and in Canada, Great Britain, Finland, Sweden, West Germany, and Australia. The book is intended to be of value to policy makers.

The authors' argument can be summed up as follows: Federal registration of voters would be an easy and popular "reform" that most probably would not greatly affect turnout in elections. Proposals for such action should be carefully scrutinized, nonetheless. They may be an opening wedge for greater federal regulation of elections, and they *might* alter the American party system radically.

Thus, Phillips and Blackman hedge their bets. The book's subtitle, "Federal Registration: A False Remedy for Voter Apathy," sets its general tone, while an assertion that federal registration has a "far-reaching possible impact" (p. 4) provides an escape clause. At their most precise the authors suggest that "a federally initiated voter-registration process is unlikely to raise U.S. participation much more than half the distance between our current rate (55 per cent) and the Anglo-Canadian average of 70 to 75 per cent" (p. 53). This estimate implies that federal registration might increase participation by 7.5 to 10 percentage points, representing (in 1972) some 10 million to 14 million voters. From many points of view such a change would not be trivial.

The quality of the evidence that this book musters in support of its thesis is poor. Its authors seem wholly ignorant of the standard techniques of multivariate analysis. Though many examples could be cited, one will suffice to show where such innocence has led.

Phillips and Blackman note that California "has worked diligently to increase voter registration" (p. 15), and they list some of the ways it has tried to do so. Then they say, "compared to its neighbor Oregon, where efforts to increase registration have not been quite so extensive (my emphasis), California's turnout has not improved" (p. 16). But consider: (1) By the authors' own estimate the registration procedures in both Oregon and California are relatively liberal; one would therefore not expect such differences as do exist to lead to great differences in turnout. (2) California's registration procedures are not more liberal than those of Oregon in all

respects; in 1972, for example, California stopped registering voters more than three weeks before Oregon did. (3) Phillips and Blackman suggest at least twenty factors other than ease of registration that might cause turnout to differ in different jurisdictions, and they control for none of these. All that the comparison does conclusively is to show that the impact on voting of differences in the two states' registration procedures is not greater than that of all other factors combined. And so it goes.

Sloppiness, as well as primitive analytical methods, contribute to the shortcomings of the evidence that Phillips and Blackman present. Consider the following statement: "According to ... [a study conducted by the University of Michigan's Survey Research Center] ... restrictive or permissive requirements make only about a 3 percentage point difference in whether persons regularly vote or do not vote" (p. 14). The finding is misreported. The figure cited applied only to the North. (See Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, The American Voter [John Wiley and Sons: New York, 1960], p. 277). In the South the proportion of those always voting in states with restrictive requirements was 9 percentage points below that in states with moderately restrictive requirements. (No southern states were counted at the time of the study as having permissive requirements for voting.)

At various points in their book, Phillips and Blackman suggest possible consequences of federal registration: increased fraud; invasions of privacy; pranks; inefficiency; "federalization" of the electoral process; "homogenization" and "nationalization" of the major parties; increased effectiveness for trade union leaders in mobilizing their followers; the dissolution of the two-party system; and changes in the socioeconomic composition of the electorate, the balance of power within and between parties, and the features of successful candidacies. Policy makers should welcome a specification and exploration of any or all of these possibilities. How much space do Phillips and Blackman devote to that task? About three pages.

STANLEY KELLEY, JR.

Princeton University

School Finance in Transition: The Courts and Educational Reform. Edited by John Pincus. (Cambridge, Mass.: Ballinger Publishing Company, 1974. Pp. viii, 319. \$15.00.)

This volume of analyses of the opportunities

for school finance reform in the United States is valuable in two respects: it brings together an excellent set of essays on various political, legal, and economic aspects of the school finance problem; and it demonstrates by example a kind of analysis which should be applied to other policy areas.

John Pincus, as editor of this Rand Educational Policy Study, has brought together the most knowledgable students of this subject. Essays by Stephen Barro, by Robert Hartman and Robert Reischauer, and by Charles Benson focus on the alternative school finance systems - including both the revenue and expenditure sides - which might be adopted to overcome present inequities in the availability of resources for public schools. These technical analyses are important, even for the lay reader, because they demonstrate the precision needed to develop policies and programs which will address the complicated issue of equity in diverse economic and jurisdictional situations. The effectiveness of these resources in the educational process is examined by Henry Levin. A fifth essay, by Joel Berke and Robert Goettel, focusing on educational issues, investigates the future of "categorical programs" - special-purpose, often compensatory educational programs - under the new legal decisions seeking to improve equity in school finance.

In contrast to these essays, another group focuses on the political and institutional problems in school finance reform. R. Stephen Browning and David Long write about the courts, and their probable behavior after the Rodriguez decision by the U.S. Supreme Court. Two articles — one by Alan Campbell and Dennis Gilbert, and one by Arnold Meltsner and Robert Nakamura suggest the political obstacles to developing and implementing school finance packages consistent with judicial mandates in different states.

Although all the essays are of high quality, perhaps the most intriguing is David Cohen's chapter on the meaning of equality as reflected in education and social policy more generally. Cohen shows that despite the sophisticated analysis of financial and judicial arrangements, proponents of school finance reform are still quite far from resolving the philosophical problems implied by the drive toward equality of educational opportunity. His dictum that "life is sometimes stranger than art" is certainly true in school finance, a subject which received a great burst of attention from the Serrano decision in August 1971 to the Rodriguez decision in March 1973. Progress was made in raising the consciousness of legislators and citizens concerning equity in public education, but even the experts have found analytical problems very difficult to resolve without leaps of faith concerning important questions, such as the impact of dollars on learning. And so, despite progress at the state level, the subject has become less attractive, leaving many competent analysts, such as the authors in this book, to rethink the problem of developing practical tools for social policy. The experience has been an important one, particularly in terms of the sociology of knowledge, and deserves to be studied by others intending to undertake detailed policy analysis.

This book also is worth reading from another perspective: it tells of efforts for reform, of analysis, of frustration, and of rethinking. In this respect, it is an important addition to the "revisionist" view of the War on Poverty, for it shows that social problems in America are more complicated than they seem to be at first glance and that despite valiant efforts, they will not necessarily be solved by those who set out to do so. This is an important lesson from which we can all benefit, regardless of our area of interest.

MICHAEL A. COHEN

The World Bank, Washington, D.C.

Federal Programs and City Politics: The Dynamics of the Aid Process in Oakland. By Jeffrey L. Pressman. (Berkeley, California: The University of California Press, 1975. Pp. 178. \$10.95.)

Popular politics demands villains, and among the favorites these days are the "best and the brightest," those national government planners whose foreign and domestic policies came to grief in the late 1960s and early 1970s. True, much of this sentiment is transient and will change with time. But at least some of it is more durably grounded in fundamental changes occurring in the structure of the country's politics.

One of these, the changing shape of the federal bargain, is the major concern in Jeffrey Pressman's thoughtful and timely analysis of Federal Programs and City Politics. The setting is Oakland, California, where between 1967 and 1969, Pressman served as a participant-observer in the office of then mayor, John Redding. From this and other sources, including extended interviews with federal administrators, Pressman developed a rich data base which he uses judiciously to assess the increasingly intricate ways of modern federalism.

The federal system, as Pressman notes, has been troubled of late by mounting tensions.

This is not to say strife is pandemic; there is cooperation as well as conflict, and Pressman is concerned with both. But his emphasis is on the latter, and on the sources and the consequences of strains associated with federal aid to cities.

These strains derive partly from the novelty of the situation. Although long a presence in cities, the national government's active concern for urban problems was, until the 1960s, quite modest. During that tortured decade, however, Washington both broadened and intensified its domestic policy concerns. Whatever the substantive merits of these initiatives, they entailed expanded intergovernmental contacts, and increased the occasions for intergovernmental disagreements.

Neither federal nor local officials were fully prepared for this change, and their early contacts, in Oakland at least, were marked by considerable groping. This included efforts to promote better communications through such conventional devices as joint task forces and liaison committees. While these may have facilitated the process of mutual discovery, they by no means eliminated conflict. If anything, they heightened it. "The differences," according to Pressman, "[went] far deeper than misunderstanding or confusion" (p. 137), and these differences were simply made more explicit by improved communications. Further, communications occurred within a restricted framework, "pseudo-arenas" (p. 15) staffed by federal and local officials unable, or if able, unwilling, to confront the political questions separating them.

These political questions, Pressman argues, create enduring difficulties within the federal system. True, intergovernmental relations are often symbiotic since each participant is dependent on the other for important resources city officials for federal funds, and federal officials for local implementation of programs. Even so, these officials confronted each other in Oakland with conflicting organizational perspectives and needs. To justify their existence to Congress, for example, and to obviate attacks from competing federal agencies, national officials needed to move boldly and with dispatch. This was at odds with the needs of local officials who were more measured in their approach. Being on the front lines, and forced to deal with the electorate in more direct and consequential ways, local officials were more likely to "limit their horizons to [programs] which [could] be understood and protected" (p. 127).

Local officials limited their horizons, additionally, since no one of them had broad

programmatic responsibilities. In Oakland, as in other cities, there were multiple jurisdictions, making it difficult to mount "the glamorous and exciting programs" of the sort championed by federal officials (p. 88). This fragmentation, Pressman notes, is a serious obstacle, not only to cooperative federalism, but to the effective workings of local democracy as well. As such it is a national challenge, and he urges federal officials to supplement their substantive concerns with programs to promote "the development of strong and open local institutions." These, he maintains, "would make the city's political system better able to articulate and respond to its citizens' demands, as well as provide potential sources of support for federal programs" (p. 146).

Whether federal officials could achieve this goal is unclear, as is the political wisdom of their attempting to do so. Others before have tried, but failed, to create "strong and open local institutions." The problem, in large measure, may be a national trait, namely a deep and persistent ambivalence toward strong political leadership. Indeed, Pressman himself seems to share this ambivalence, for example in his assessment of revenue sharing - a policy that could strengthen elected local officials, but might threaten important policy minorities (p. 139). As long as this ambivalence persists, and there is no reason to expect it will not, local political leadership will continue to be an uncertain commodity. As a result, tensions between city politics and federal programs will continue, as will the complaints, both from Washington and from the provinces.

RUSSELL D. MURPHY

Wesleyan University

A Little Group of Willful Men: A Study of Congressional/Presidential Authority. By Thomas W. Ryley. (Port Washington, N.Y.: Kennikat Press, 1975. Pp. 198. \$12.95.)

The present work is an historical account, in fascinating detail, of how, in Wilson's famous denunciation, a "little group of willful men... rendered the great government of the United States helpless and contemptible." The filibuster in March 1917 that frustrated Wilson's requested authorization to arm American merchantmen against the German submarine, and "to employ such other instrumentalities and methods" as he might choose against Germany at sea, is another of the dramatic encounters in American history between the executive and

There seems to be little choice but to treat these recurring confrontations as discrete episodes. While the armed ship instance led soon thereafter to adoption by the Senate of its first cloture rule, it is hard to see anything fundamental that might be done to mitigate the constitutional overlap between Congress and the Executive in the conduct of foreign policy. There was no such sequel to the fiasco over the Versailles Treaty, while one is not impressed with either the cogency or permanence of the congressional resolution of 1973 limiting the amount of time that American troops may be committed to battle without the approval of Congress.

An historian, Ryley tells a story; he attempts neither to moralize nor generalize. The political scientist can hardly do better. Clashes over foreign policy between the Executive and Congress are inherent in the constitution. Just now they are increasing in frequency and intensity. Trying though these clashes are, resignation seems the only recourse, tempered by retrospective judgment that vindicates now one and then the other of the two branches, locked forever in uneasy embrace.

EDWARD H. BUEHRIG

Indiana University

Patterns of Recruitment: A State Chooses Its Lawmakers. By Lester G. Seligman, Michael R. King, Chong Lim Kim, and Roland E. Smith. (Chicago: Rand McNally and Co., 1975. Pp. 269. \$8.95, cloth; \$4.96, paper.)

Patterns of Recruitment: A State Chooses Its Lawmakers is an analysis of the recruitment process of candidates for the lower house of the Oregon legislature during the 1966 campaign. The study had its beginnings in a political science graduate seminar at the University of Oregon under the tutelage of Professor Lester G. Seligman. This study adds to the impressive list of works on political recruitment that Professor Seligman has written during the last twenty years.

The literature on political recruitment has been plagued by two major problems: a lack of coherent and commonly accepted concepts which are operationalized (measured) in the same manner over time, and a lack of an integrated theory of the recruitment process. The result has been a plethora of socioeconomic background studies of political elites with little or no supporting political theory

entire recruitment process, including both the causes of certain recruitment patterns and the consequences of those patterns on public policy. Seligman, Michael R. King, Chong Lim Kim, and Roland E. Smith successfully address most of the inadequacies of the political recruitment literature and make an important theoretical and empirical addition to our knowledge of the recruitment process.

Seligman's early work and certainly this latest analysis have been rich sources of hypotheses on the recruitment process. In Patterns of Recruitment, the authors present a coherent model focusing upon five key recruitment elements: (1) from what sociopolitical group are political activists recruited; (2) who are the recruiters; (3) what criteria are taken into the recruitment equation; (4) how are they selected; and (5) to what role is an individual recruited?

The data for the analysis are based upon interviews with both winners and losers in the 1966 primary and general elections for the Oregon legislature. The interviews were administered in three waves: preprimary, postprimary but pregeneral election, and postgeneral election. The interviews elicited basically three types of information: general socioeconomic and political data; data on the process of recruitment (the questions on decisions to run and opportunity and risk are especially good); information about candidate's evaluations and perceptions of their election experiences; and the impact of winning and losing the election.

Unfortunately there are few instances of interval-level statistical analysis, and most generalizations stem from tables reporting bivariate relationships. The entire analysis is beset by the continuing problem of the small number of respondents used in the statistical tests. At one point the authors report several chi squares (without reporting measures of association) with four total respondents. In the only section utilizing partial correlation, the number of respondents used is twenty-one. In a section called "A Causal Analysis of Certification" (pp. 146-151), the amount of variance explained in the dependent variable of each model is not reported, and partial correlation ratios are used for the paths in each model. Generally the statistical analysis could have been much more sophisticated, considering the amount of time and care taken in collecting the data, but this does not seriously detract from the rich set of generalizations the authors reap from their respondents.

Some of the most interesting findings are related to the reasons that people run for political office. Most candidates felt that interest groups were the most important legislative recruiters in Oregon. According to the authors, interest groups have filled a major recruitment role because the direct primary fragmented the importance of political parties. They support Prewitt's findings from the Bay Area city council study, that small primary groups become important sponsors of candidates when there is an absence of political parties. They also discover that most of the respondents perceived little risk in candidacy because, "the outcome is uncertain only in a few elections" (p. 187). Most candidates know when they are going to win or lose, and they know what the costs are going to be to their political, social, and occupational futures.

The loss of an election has little attitudinal impact on legislative candidates in Oregon: "For most candidates, the loss of an election does not alter their political attitudes toward the system or diminish their zest for political activity" (p. 187).

An important distinction is found by the authors among legislative districts and incentives to run for political office. Urban areas generate stronger incentives for candidacy (i.e., more self-recruitment) than do rural areas. Rural unsponsored candidates are a rarity; therefore, a variety of sponsors approach rural candidates, but because political party organizations are diffuse and fragmented in rural Oregon they are not significant recruiters. Primary groups and secondary associations are the most important candidate sponsors in rural Oregon legislative districts.

Although the study is a major contribution to our understanding of the key variables involved in the recruitment process, it has a major shortcoming. The authors did not examine the linkage between the four recruitment patterns which they discover (competitive, majoritarian, rural oligarchic, and rural competitive) and legislative behavior and ultimately public policy. Chapter II is the only part of the book where the linkage between recruitment and public policy is discussed. At no point do the authors analyze the linkage between the recruitment process and Oregon legislative decision making. The central question to ask of their study is: What impact does each of the four recruitment patterns have upon policy preferences and legislative behavior?

Scholars are interested in how political elites are screened and selected because such study can shed light not only on the distribution of power but also on the linkage between elite, constituents, and public policy. Patterns of Recruitment does not analyze this linkage. Therefore, a major reason for studying recruit-

ment is missing from this analysis: how people gain and monopolize political power and what they do with it once they have it. In conclusion, although this book is a step in the right direction, we still need research on both the causes and the consequences of recruitment patterns over time and in a comparative context.

JAMES A. THURBER

American University

Personality and Democratic Politics. By Paul M. Sniderman. (Berkeley and Los Angeles: University of California Press, 1975. \$13.75.)

This book is based on Sniderman's dissertation, which won for him the APSA's E. E. Schattschneider Award in 1972. That this prestigious award was given to a manuscript on politics and personality is a favorable harbinger for students of that field. After all, political psychology continues to move haltingly at best around the periphery of political science, very much like an unwanted guest at a fancy ball. One begins Professor Sniderman's book with the hope that its apparently high quality will help make the study of political psychology persona grata at the political science cotillion.

Are these hopes realized? In very large measure, yes. Certainly one can understand why the Awards Committee thought so highly of Sniderman's work. First, there is the major theoretical problem Sniderman has set himself. He returns us to perhaps the central question of contemporary political psychology: What can we expect psychologically of leaders and followers in a democracy? Do the psychological characteristics of leaders and followers place any limitations on democratic practice? More specifically, are leaders psychologically more "competent" than followers, or are leaders driven by scarred self-esteem and battered self-images of the sort predicted by Lasswell and exemplified by Nixon?

Sniderman not only chooses a major problem for analysis, but he also proceeds in an extremely sophisticated fashion. From the vast array of psychological theories and findings available to him, Sniderman chooses the concept of self-esteem as his focus. He then lays out a motivational version of self-esteem theory (largely psychoanalytic in origin) and a social learning version of self-esteem theory (largely social psychological in origin). Throughout the book he blends these two theories in a most complex and undogmatic fashion, revealing both their limitations and their advantages. Most important, unlike most writers in the field, Sniderman is alert to the configurational

basis of personality. He deliberately avoids simplistic, single-motive theories. For this reason Sniderman is able to show that self-esteem helps knit together other psychological qualities which, in concert, produce important differences in political styles. For this reason, as well, Sniderman is able to dismiss with such success the notion of "democratic personality." As Sniderman shows, there is no single source of personality support for democracy; indeed, the very psychological qualities of openness, flexibility, and rationality deemed part of democratic personality by Lasswellians actually predispose the person toward nondemocratic attitudes in undemocratic regimes. In sum, personality typologies and political typologies should be kept unmixed.

Lastly, there are the findings themselves. After carefully examining large samples of both the mass public and political leaders, Sniderman concludes that neither the motivational nor the social learning conception of self-esteem tells the whole story about American mass/elite psychological differences. The most general application of the Lasswellian argument seems clearly incorrect: in general, elites are not driven by compensatory behavior to recover lost self-esteem. Instead, in the aggregate, political leaders have higher self-esteem levels than the mass sample. Indeed, low self-esteem acts to screen out political stimuli, and therefore reduces political participation. Nonetheless, a substantial proportion of the political leaders studied manifests a syndrome in which low self-esteem combines with obsessiveness and materialistic ambitions. The result, therefore, is that the political elite contains a high proportion of "driven" individuals. More disquieting is the fact that, contrary to the views of Lane and others, the profession of politics does not penalize these low self-esteem compulsives. As Sniderman shows, once into politics, this relatively small group is as successful as the "well-adjusted." In sum, according to Sniderman, leadership in American democracy is generally distinctive for its high level of selfesteem, a characteristic that contributes to the survival of the system. Nonetheless, the elite contains sources of potential disarray. Much to his credit, Sniderman does not find it necessary to please either hardline elitists on the one hand or pluralists on the other.

Nevertheless, there are worrisome aspects of this research. Perhaps most questionable is the data base. Sniderman's work is a secondary analysis of Herbert McClosky's by now extremely well-known mass and elite surveys which were conducted in the late 'fifties. Whether these data tell us much about 1977 is

very much open to question. Sniderman argues that other studies conducted in recent years provide evidence for stability in the relationships he uncovers; therefore, we need not worry. But reassurances of this sort are at best suggestive. Only if the McClosky instrument itself were used in a contemporary replication would our doubts be stilled.

This is not the only hazard of secondary analysis that Sniderman encounters. As he points out in one place (p. 48) the original McClosky data were not gathered for use in a study of self-esteem. Not surprisingly, therefore, despite Sniderman's extremely meticulous scale-construction procedure, the face validity of some of the items used to measure selfesteem is uncertain, at least to me. For example: "When I disagree with people it turns out later that I was right," is no more necessarily a measure of Snidermanian "interpersonal competence" than it is of simple belief in one's own purely intellectual capacity. Nor is "I often prefer to be with people of wealth and good breeding" necessarily a statement of "status inferiority"; "I" could prefer to be with such people because I find them more interesting, intelligent, and enjoyable than others. After all, snobs have rights, too.

The overall status of the self-esteem concept is also a bit worrisome. Sniderman shows quite properly - that, because his study is correlational, the causal links between selfesteem and the many other psychological qualities to which it is related must remain uncertain. He settles for the argument that selfesteem sits at the center of a seamless web of psychological qualities. This rather indefinite conclusion - though undoubtedly responsible makes self-esteem a less important component of political psychology than Sniderman would prefer. Nor is the matter improved by Sniderman's inability to show that self-esteem endures over time as a true personality characteristic must. Nor, finally, is the matter improved by Sniderman's inability to demonstrate the origins of self-esteem, a problem which Stanley Renshon tackles with some success in his work (Stanley Renshon, Psychological Needs and Political Behavior [New York: The Free Press of Glencoe, 1974]). As a result of these lacunae, the concept of self-esteem has a somewhat disembodied quality.

Indeed, of the three components of selfesteem Sniderman identifies, only "interpersonal competence" discriminates strongly between leaders and followers. Since four of the seven items measuring interpersonal competence directly relate to leadership ("I hate to tell other people what to do," "I would rather not have very much responsibility for other people," "I dislike to have to talk in front of a group of people," and "I doubt whether I would make a good leader,") it would be surprising if this were not so. Still, the other two components of self-esteem (personal unworthiness and status inferiority) do play some role. On the whole, however, the tools of the analysis are not entirely adequate to the task.

Sniderman shows that the motivational and learning aspects of self-esteem are related in complex ways. Therefore, any conclusion from this study that viewing self-esteem purely from a learning perspective will solve the problem of designing therapeutic practices for democratic citizenship would be a bit facile. True, the Lasswellian approach seemed to portend hordes of psychiatrists marching about in an orgy of mass psychotherapy intended to spur democratic participation. But the learning approach as Sniderman outlines it does not really substitute anything uniquely its own in place of this silly alternative. Indeed, Sniderman takes pains to point out that political therapy - such as "participatory democracy" - will not really improve self-esteem. But surely such therapy would if self-esteem could be improved merely by social learning coupled with practice. Nor should this be construed as a serious argument against participatory democracy, for that theory does not derive its main justification from theories of personality. Thus, Sniderman does not really supersede Lasswell, but, through precision and craftsmanship, he makes a substantial contribution all his own to the study of personality and politics.

RICHARD M. MERELMAN

University of Wisconsin-Madison

Managing Local Government Under Union Pressure. By David T. Stanley. (Washington, D.C.: The Brookings Institution, 1972. Pp. 177. \$6.95.)

The Unions and the Cities. By Harry H. Wellington and Ralph K. Winter, Jr. (Washington, D.C.: The Brookings Institution, 1971. Pp. 226. \$7.95.)

These small volumes examine the impact of unionism and collective negotiations on employer-employee relationships in municipalities and other local governments. Both are tautly organized and well-written works by highly competent scholars. There are differences in points of view and judgments.

Wellington and Winter observe in their Foreword (speaking of unions) that "few other groups can so profoundly affect the life of a

community" (p. vii), and in their final paragraph refer to "the power the unions will win if we mindlessly import into the public sector all of the collective bargaining practices developed in the private sector" (p. 202). The intervening discussion reflects this point of view. Stanley closes his examination on a different note, i.e., that despite employees' gains, there is "little prospect that the transaction will become overbalanced against management, given the continued functioning of the American political system and the exercise by management officials of a reasonable mix of resolution, ability to listen, decisiveness, labor relations knowledge and good will" (pp. 151-152). Stanley has fewer reservations about bargaining in the public sector.

Wellington and Winter accept the practice of collective bargaining in the private sector but view the public sector as "very different indeed" (p. 8). They assume that competitive forces in the private sector "the discipline of the market" act to limit the impact of bargaining. I believe that this faith in the discipline of the market is not well-founded: Oligopolistic conditions, with a considerable element of administered pricing, are typical of much of the private sector in which bargaining is widespread. Nor are consumers sufficiently well-informed, organized, and powerful to resist effectively administered price adjustments that may result from negotiations.

Wellington and Winter take the position that strikes in the public sector properly may be curbed, although they recognize that not all strikes among public employees have a similar impact. But because such stoppages result in the disruption of services and may produce, or exacerbate, political problems, the authors believe that government "must be in a position to protect itself and its citizens" (p. 73). In my judgment, the issue of strikes by public employees often is overemphasized. While militancy and strike action is growing among public employees, even today the public sector is much less subject to strikes than is the private sector, and many types of work in the public sector (such as the services of billing clerks in the water department) are not important on an hour-by-hour basis as are the services of firemen, police, or prison guards. Wellington and Winter recognize this in concluding that "the emergency dispute problem does not compel a complete ban on strikes" (p. 194).

The authors advocate procedures that call for third-party involvement in search of solutions, including arbitration when the authority of the arbitrator is restricted; and fact-finding, when fact-finders are allowed to attempt mediation. As a last resort, say the authors, the approach of selecting one from a number of possible procedures "will not stop all strikes but has the best chance of reducing their incidence" (p. 186).

Stanley's study concentrates on the impact of unions on the administration of nineteen urban areas, largely cities, but also a few counties, whose populations range from 75,000 to nearly eight million. Based on his study Stanley observes that "employee unions are changing the way local governments are administered" (p. 1). As a result of the steadily increasing rate of change, Stanley believes, "employees appear to be less acceptant and more challenging; ... management is less dominant" and "employee-management relationships tend to become more formal and potentially more hostile" (p. 58). Further, "unions contribute to pressure on local governments to find new and expanded revenue sources" (p. 135), but "in only a few instances have the impressive political power and tactical position of the unions been used in efforts to achieve more effective local tax measures and more satisfactory systems of intergovernmental fiscal relations" (p. 135).

Stanley's judgments will produce mixed reactions among readers. He asks, rhetorically, "what is happening to government achievements under union pressures...?" and answers "not much" (pp. 138-139). He believes that unions sometimes may improve program effectiveness but that "management effort is invariably more tense, more time-consuming, more public than that applied to non-union personnel problems because of the adversary posture of the unions and their generally aggressive attitudes" (p. 140).

While unionism has affected the time and effort devoted to employer-employee relations in the public sector, Stanley gives a warm evaluation of the end result, holding that "strikes have inconvenienced the public and have probably increased government costs, but ... they have also corrected injustices, secured gains for employees, and been a major cause of change in public administration" (p. 144). And, finally, "recent history suggests that ... in the future ... disastrous outcomes are hardly likely" (p. 145).

Since these books were written, many publications have explored labor relations among public employees. Such studies are desirable because the impact of bargaining and its effects on the work of public employees and problems created for local administrators should be understood. Employment conditions in the public sector fail to meet the defensible needs of

workers in many of the smaller cities, towns. and counties of the nation. The well-publicized salaries of New York or San Francisco are not typical. Public workers have been left behind in many instances; they are becoming "less acceptant." The position of Stanley is more nearly in step with the judgments being expressed by many recent authors. My own study of the impact of bargaining in Kansas, where public employee bargaining is just beginning, supports Stanley's view that the early impact of unions is slight. Yet, many city managers and other public officials are afraid of the impact of such bargaining, sharing the reservations of Wellington and Winter. Collective negotiations will undoubtedly create problems for local administrators; these are manageable problems, though the efforts and priorities of officials may have to be reordered. Nevertheless, bargaining is bound to grow, and many more public officials will have to reckon with it. These books are valuable contributions to the literature on unionism and bargaining in the public sector.

GLENN W. MILLER

Wichita State University

The Rise of the City Manager: A Public Professional in Local Government. By Richard J. Stillman II. (Albuquerque: University of New Mexico Press, 1974. Pp. 170. \$8.95.)

While no older than this century, the city manager movement has undergone a variety of distinct phases in its development. Richard J. Stillman II in *The Rise of the City Manager* offers a concise and comprehensive chronicle of this movement that should long serve as an excellent point of departure for further work in this area.

The most useful part of the book is the first three chapters, which trace the rise of the movement from the first well-publicized installation of council-manager government in the Staunton, Virginia, of 1908 to the revitalization of the International City Management Association in the mid-1960s. By the end of World War I, nearly a hundred jurisdictions had councilmanager structures. During the following decade that number would quadruple because the growing complexity of the concerns of public management in the automotive age demanded administrators with technical - usually engineering - competence; and because the notion of "economy and efficiency" in government was given an added impetus by the widespread acceptance of scientific management, or Taylorism, in the private sector. Stillman compares Karl von Clausewitz's Vom Kriege, which provided an intellectual rationale for nineteenth-century military professionalism in Europe, to the scientific management movement, which furnished the twentieth-century profession of city manager with its intellectual and theoretical bases. The profession did not experience a comparable growth spurt until the post-World War II era when an expanding suburbia sought new institutional structures to cope with a new array of public facilities and intergovernmental concerns - not the least of which was insulation from the central city and the problems that the city both had and symbolized.

The historical portion of the book, its first half, is well written and exhaustively documented. While the overall presentation is thoroughly academic in tone, Stillman occasionally writes in a more lively style, as for example in his description (on page 15) of one of the publicists who first gave the city manager concept national prominence: "Richard Childs was the quintessential progressive reformer -aman of means, with a social conscience, a professional background, and extra time to devote to his pet hobby, municipal reform." Stillman's efforts at documentation are not limited to the ample footnotes; he thoughtfully provides a thirteen-page bibliographical essay on his sources as an appendix, which advises the reader that the extant literature is spotty and should be read selectively - all the more reason for first reading Stillman's essay.

The second half of Stillman's work is uneven. Chapter four offers a profile of the city manager that is based upon a 1971 questionnaire sent to a random sample of 11 per cent of the profession and published earlier as a monograph by the ICMA. The following chapter, which compares and contrasts city managers with career diplomats and school superintendents, is academic in the worst sense of the word. Stillman is too skilled a writer not to have a point; however, in this case it is forced. But he rebounds from this minor faux pas with his final chapter, an excellent analysis of the present dilemmas and future directions of the profession.

One of Stillman's more significant findings is that the politics/administration dichotomy, first proclaimed by Woodrow Wilson in 1887 and then reified by Frank J. Goodnow in 1900, is an archaic cross that the modern city manager must still bear. Citing the numerous studies on the role of managers in local politics and the urban policy decision process, Stillman

asserts (on p. 104) that the "acid test of survival for managers is their ability to come to from the public treasury, it followed rationally grips with the complex political realities of the communities in which they operate." Unfortunately, the persistent fictional doctrine of the total separation of politics from administration under the council-manager structure both strains the city manager's relations with his council and obscures the manager's valuable contributions to the politics of the community. There is no easy solution to this dilemma. The best city managers may well be the best hypocrites - always proclaiming their neutrality in political issues while at the same time working quietly to resolve those issues to the advantage of their community. The definitive book on the history and problems of the city manager's profession remains to be written; but Stillman's work is undoubtedly a leading contender for that accolade in the interim.

JAY M. SHAFRITZ

University of Houston at Clear Lake City

From Poor Law to Welfare State: A History of Social Welfare in America. By Walter I. Trattner. (New York: The Free Press, 1974. Pp. 276. \$9.00.)

This simplistically written book is intended primarily for historians and social workers who wish to obtain some perspective on the magnitude and character of the problems with which they are dealing. But it may also be of general interest to those concerned with an interpretive history of social welfare policies in the United States. The author claims that most social welfare histories are accounts of various chronological periods of specific developments within the field such as child labor reform or housing reform and that his study assimilates numerous earlier studies to provide a broad overview of American social welfare. This book, however, does not satisfy that claim nor is it unique in that respect, although it does attempt to provide a general synthesis of accounts.

Two main trends are emphasized throughout the book. The first is the early belief that the only cause of poverty is individual weakness. The influence of the classical economists and the general laissez-faire philosophy that public relief tended to demoralize recipients and depress the standard of living in particular was based on the belief that accumulating wealth was a moral virtue and that dependency was a vice. If destitution was the individual's fault and most of the needy were recipients of help that public aid was a cause of pauperism and thus inherently bad. If aid was to be bestowed it should be given to public institutions where careful control could be enforced. The Protestant ethic, which demanded benevolence of the rich but also hard work and morality of the poor, complemented the belief that the individual had the power to achieve economic success through his own efforts.

This emphasis on individual weakness and the visitation of God's wrath on the erring person gave way to a focus on environmentalism or the belief that one's economic and social surroundings affected the way one lived. From this new focus came discussions about helping delinquent children to lead productive lives by transplanting them to acceptable environments. Furthermore, society started believing that a dependent person needed its assistance and was not inherently evil. Michael Harrington's popular statement in The Other America highlighted this vicious circle of poverty which brought self-supporting families to the dependency level and kept them there

The second point was reflected in the attitude of President Franklin Pierce to Dorothea Dix's appeal to Congress to appropriate ten million more acres to the states to help pay for the construction and maintenance of mental hospitals. Pierce vetoed the measure, saying in part: "If Congress has the power to make provision for the indigent insane . . . it has the same power for the indigent who are not insane," and thus all the nation's poor (p. 62). Harry Hopkins in Spending to Save illustrates the same sense of fear that came with federal government programs that dealt directly with the individual and did not rely directly on the state and local government for action. The flexibility of federalism worked against a single legal code affecting social welfare matters throughout the nation and continues to do so.

According to Trattner, the Depression answered the vexing question of whether private or public agencies should be responsible for relief giving. Voluntary charity simply could not cope with the situation, and only public agencies could deal with the collapse of the economy and mass unemployment. Although President Hoover adhered to Pierce's earlier thinking of self help, such thinking was doomed to national centralization through such programs as Social Security and the War on Poverty.

The book concludes abruptly, telling little about the 1960 and 1970 eras and ends on a somewhat syrupy plea for an understanding of social ills and the poor.

FRANK T. COLON

Lehigh University

Black Republicans: The Politics of the Black and Tans. By Hanes Walton, Jr. (Metuchen, N.J.: Scarecrow Press, 1975. Pp. 217. \$8.00.)

In this work Professor Walton seeks to trace the impact of racial conflict on factional cleavages within the Republican party in the South after the Civil War, and particularly to locate those forces leading to the decline of the black-and-tan faction of the party. He charges that "although the Negro issue has occupied a central place in the development of national party organizations and in the formation of political alliances, its effect upon intra-party cleavages has been dealt with in only a peripheral and superficial manner" (p. 2). Walton's charge is startling to those of us who learned so much about race and politics in the South in this period from the works of the late V. O. Key, Jr. and the scholars he inspired to further research. Nothing in Walton's book challenges these writers' interpretations. But in bringing together a mass of hitherto scattered information on factional in-fighting within the southern Republican states and local parties after the Civil War, Walton adds depth to our knowledge of the decine and fall of biracialism in the GOP.

Black Republicans contains three major sections. The first reports the attraction of free blacks to the Republican party at its inception, and the mixture of idealism and opportunism that moved white leaders of the national party to champion the cause of emancipation and racial equality. The second and major section is a detailed narrative, for each of the eleven southern states, of struggles between the blackand-tan and lily white factions in southern Republicanism from the 1870s to the 1920s. Walton has prodigiously combed fugitive sources as well as published materials in order to give a minute account of state and local Republican factional struggles. These struggles, he finds, were fought with varying weapons and somewhat different objectives in the eleven states. In Louisiana, for example, the two factions largely resorted to legal actions in pursuing control of the state party machinery and convention delegations; in Texas, however, the factions primarily sought mass support at the ballot box. For each state, Walton carefully and sometimes vividly reconstructs the strategies and tactics of the black-and-tans and lily whites and reveals the strengths and weaknesses, of tactical fighter, bomber, surveillance, and

of factional elites. A final chapter links the fate of the black-and-tans to Republican national convention politics up to the Hoover period, when the lily whites were finally victorious and southern blacks began their exodus to the Democratic party.

The reader closes Walton's book rather stunned by a mass of names, dates, and events, yet with a deep overall impression of the often scurrilous measures used by both factions, the ineptitude of many of their leaders, the venality of national Republican party figures and their insensitivity to the plight of southern blacks in this period. Here is a portrait of American party politics at its worst. Yet political scientists are apt to come away from Black Republicans somewhat unsatisfied. Walton's study is primarily narrative and presents almost no systematic data analyses that might sharpen our knowledge of the similarities and differences in the two factions, nor does the author apply theoretical approaches that might relate his research to some broader framework of understanding. Considered as history, Walton's work is careful and thorough, yet his concentration on details of the intraparty battle leave the reader with a feeling that the possible effects of some broader historical forces remain unexplored. Walton seems not to rank Populism as a factor of great significance to the black-and-tan decline although, in view of the importance accorded to it by Key and other scholars, he might have been expected to consider it at greater length. And while he is undoubtedly right in highlighting the effects of white supremacist attitudes on the withering of the blackand-tans, he ignores the flowering of other ideological trends after the Civil War - e.g., the rise of social Darwinism and the economic gospel of individualism and competition - that might help explain the disappearance of white Republicans' zeal for racial justice. A full-scale analysis of the transformation of the Republicans from a biracial to a lily white party remains for the future, yet Walton's book will be an indispensible resource in any such analy-

DOUGLAS GATLIN

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U.S. Tactical Airpower: Missions, Forces, and Costs. By William D. White. (Washington, D.C.: The Brookings Institution, 1974. Pp. 121. \$2.50.)

William White's book grapples with the problem of escalating costs in the procurement transport aircraft for the military services. In current 1975 U.S. dollars, the P-40 Fighter cost \$54,000 when it became operational in 1940. According to White, the F-15, by contrast, will cost \$9.8 million per copy in 1975, excluding research and development costs. Economies of scale no longer apply. In the 1940s, thousands of aircraft were made by assembly-line methods and the cost per unit fell as the number produced increased. Now much smaller quantities are produced, with aircraft largely hand-crafted. Fewer aircraft can be procured, therefore, because each is so costly.

White's principal argument is not that the skyrocketing trendline for aircraft costs has been affected by pervasive price inflation in the U.S. economy. Rather he attributes the cause of rising costs to a peculiar American procurement strategy of maximizing unit capability so that each tactical aircraft is a multipurpose platform. The optimal aircraft, in this view, is both fighter and bomber combined, able to perform close-air support and deep interdiction missions, and capable of all-weather and night operations. White, a colonel in the U.S. Air Force, traces this procurement strategy ultimately to U.S. airpower doctrine which gives priority to defeating an opposing air force in air-to-air combat and in bombing its bases of supply. For tactical aircraft to survive in the hostile environment over an opponent's home territory as the drive for air superiority is conducted, heavy demands are placed on aircraft performance and survivability of the aircraft and crew.

The reader should note that the author makes a choice when he pursues the line of argument that rising costs for tactical aircraft are due to American procurement strategy. An alternative explanation, which he observes only in passing, would explore how general price inflation has affected tactical aircraft procurement as well as civilian consumer purchasing power for other far less sophisticated hardware. His own argument would be more persuasive if he had tried to persuade the reader initially to accept his basic assumption about the reasons for the increasing costliness of tactical aircraft. If he had laid the trendline for the decreasing real value of the dollar alongside his trendline for aircraft procurement costs, he might have been able to demonstrate some interesting correlations. His argument, however, might have suffered from the comparison.

Let us assume that aircraft costs are up because price inflation resists any correction. We are still confronted with the problem of what to do about burdensome aircraft costs. White may have misidentified its sources, but

he has surely addressed an important problem which seemingly defies a solution. The solution probably lies outside the subject of White's book and, for that matter, even beyond the defense budget. Unfortunately, economists have not yet found an antidote to price inflation. White also misidentifies the solution by implicitly arguing that a change in airpower doctrine away from an emphasis on air superiority in an adversary's home air space and toward close-air support for the ground battle will cause less expensive special-purpose aircraft to be built. Undoubtedly, if we give up our traditional interest in air superiority, savings will result, but then we will be giving up the means of prevailing in a conflict. The lessons of the Yom Kippur war are still being debated, but if the Israeli Air Force had emphasized the close-support mission to the detriment of air superiority, its losses to Arab surface-to-air missile systems probably would have been even greater than they were.

White does not inform the reader of alterations in design engineering which can effect savings. He is more intent on changing mission emphases. And yet if missions do not change because of the bureaucratic inertia of the military services for which traditional roles and missions are as touchy and fundamental as ever, it would have been more realistic to identify changes in the technical parameters of aircraft, metallurgy, ordnance accuracy requirements, and other ways of reducing the cost of aircraft systems. The elaborateness of Air Force ground support facilities offers opportunities for savings. The staggering \$1 billion cost of nuclearpowered aircraft carriers has stimulated a search for alternatives.

The reader must wait for another book, or for White's next book, to learn how the budget for tactical air power forces can be brought under control.

JACK M. SCHICK

Washington, D.C.

Local Administration and Politics in Modernising Societies: Bangladesh and Pakistan. By Najmul Abedin. (New York: Oxford University Press, 1974. Pp. 458. \$14.00.)

The title of this book may mislead readers not familiar with governmental structure on the subcontinent because, as the author points out in the introduction, it is a study of subnational rather than local government. In addition, the relationship with local politics is given only marginal treatment.

Professor Abedin, now at Chittagong University in Bangladesh, obviously prepared most of this book, which was printed in Dacca, as a study of one nation and then made allowances for the emergence of Bangladesh. The manuscript was completed in 1973 and in no way anticipates the Bangladesh military coup of 1975.

The book deals heavily with prepartition influences on district government and relies heavily on secondary sources. As such, its greatest contribution is an historical analysis of the differences between Bangladesh and Pakistan and of the role and function of districts which were units of the provincial governments in Pakistan prior to 1972 and remain such today. In Bangladesh they have become arms of the central government. Chapter I, "The Evolution of District Administration." is an excellent summary of the wide range of material available àbout pre-British and British India. Here we have recounted the conflicts between Cornwallis and Munro reforms, Whigs and Utilitarians, separation and concentration of powers, and Zamindari and Ryotwari land tenure systems. Unfortunately, the discussion of the "political-social environment" in Chapter II is not of the same depth, reflecting obvious differences in the quality of source material. Administrative and social traditions are termed authoritarian and paternal in both nations, but religion is held to dominate political life more completely in Pakistan, while Bangladesh exhibits a stronger tradition of political participation.

The real focus of the study is the District Officer (Collector, Deputy Commissioner, Magistrate), and the author has a hard time merging his basic conclusions about the individuals who hold these hallowed jobs. He argues that they are authoritarian and paternalistic, representing an elitist tradition (p. 53); that they have declined in general intellectual ability, with serious consequences for district administration (p. 186); and "that the District Officer, whose authority remained almost unchallenged during the early part of this century, is now facing a serious challenge to his authority even from within the administration" (p. 359). In this sense, Professor Abedin reflects a conflict that has plagued both scholars and practitioners of local government in Pakistan and Bangladesh since 1947 – a theoretical desire for democratic institutions which is constantly offset by a traditional respect for authoritarianism. The end result is constant criticism of a governmental system dominated by elitist services existing hand-in-hand with a national reluctance to substitute any other form.

The book also demonstrates another continuing problem well known to political scientists who have worked in Pakistan - the extreme isolation of the academic community from the real world of government. Although major official documents are cited, a definitive analysis of the complex interaction between the provincial departments, particularly the local government departments, and district administration is lacking. No mention is made, for example, of the efforts to develop a Local Councils Service that went on in both wings of Pakistan during the 'sixties. The author evidences familiarity with available materials about East Pakistan-Bangladesh, but his knowledge of West Pakistan-Pakistan materials is limited, if the footnotes and the bibliography are valid measures. Consequently, a major portion of the book deals with what is now Bangladesh. Still, it provides a valuable summary of the role of the District Officer in British India and, to a lesser extent, in pre-1972 Pakistan. The book offers perhaps more than the average reader will want to know about land revenue administration but is superficial in its treatment of Village AID and the Works Programme and contains only a limited summary of local politics, the activities of local councils, and the Basic Democracies experi-

The most penetrating portion of the book for readers searching for an understanding of the Bengali political system is the author's "Concluding Observations," a four-page section which is not, for the most part, an outgrowth of the earlier text. Here he makes the observation that local leadership began to make its existence felt during the 1960s, that during that time the Basic Democracies system encouraged a fusion of official and nonofficial agencies, and that communication between the bureaucracy and society, particularly rural society, was increasing. He also argues that because the BD system was an electoral college which served the political interests of an authoritarian regime, the local councils were severely undermined as effective units of local government.

These are hypotheses with which I agree, but to test them will require more than the predominantly historical account presented in this study. Unfortunately, that level of research is still not possible in Bangladesh.

GEORGE M. PLATT

Wichita State University

Agricultural Initiative in the Third World. By the Agribusiness Council. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. vi, 210. \$16.50.)

Formed in 1967, the Agribusiness Council is a nonprofit organization of corporate and foundation members including, among others, United Brands, H. J. Heinz, Exxon, IBM, and Ralston Purina. The Council's expressed objective is "to aid in relieving the problem of world food supply through increased agribusiness investment in developing nations" (p. ii). In short, the Council seeks to locate investment opportunities for its members. In February, 1974, the Council organized a symposium of "Science and Agribusiness in the Seventies," and this volume consists of sixteen papers originally presented at the London conference.

At least four often mutually exclusive themes compete unsuccessfully for dominance within the book. First, several papers are devoted to a consideration of applied agricultural science's contribution to food production. These chapters generally reflect an optimism about the potential for dramatic advances expected in food output. The fact that technology cannot be isolated from its political, social, and economic context does not receive the attention it deserves here.

A second theme is that a reduction in birth rates is necessary to prevent massive famines in the underdeveloped world. The fact that poverty creates high birth rates, which in turn create poverty, is mentioned only in passing. There is no demonstrated familiarity with the rapidly burgeoning literature on socioeconomic determinants of fertility. Perhaps foreign agribusiness is not sympathetic to radical economic redistribution as the key to declining fertility.

Third, the discovery of the small farmer by the World Bank and the Inter-American Development Bank (IDB) comprises another group of essays. Both Antonio Ortiz Mena, president of IDB, and Montague Yudelman, director of Agriculture and Rural Development at the World Bank, recognize the necessity for reforms in land ownership and tenancy. Ortiz Mena believes this would create consumer demand for fertilizer and machinery. Neither paper sufficiently examines the resistance to such basic structural transformation on the part of internal elites and external allies.

Fourth, it is argued that if only they are given the investment incentive, multinational agribusiness corporations can solve the world food problem with their vision, "know-how," technological packages, and superior management judgments. In a section entitled "Agribus-

iness in Developing Nations," various schemes for attracting agribusiness investment receive attention. Firms have apparently hesitated to share their expertise because of "uncertainties about the future of agrarian reform and taxation policies" (p. 93).

Finally, Lester Brown of the Overseas Development Council and David Hopper, president of the International Research Center in Ottawa, contribute papers on "The Emerging Policy Issues." Hopper's upbeat conclusion is that food output can be tripled with proper financing and good will.

I found myself rather impatient with the assumption in this book that increases in food production will mean that hungry people will have enough to eat. The narrow perspective generally found here excludes the real possibility that food problems are rooted in a maldistribution of the actual means of food production. Brazil, and specifically the Industrial Development Institute (INDI) in the state of Minas Gerais, is touted as having successfully utilized the Council's skills in 1972. According to a contribution by Dr. Francisco Noronha, the Secretary of Commerce, Industry, and Tourism, INDI had promoted major agribusiness development by attracting considerable outside investment. There is absolutely no evidence in the book, however, to suggest that starving Latin Americans are eating better as a result or ever will; exporting beef to hamburger chains in the United States would seem to be of marginal value to the peasants. This raises the troubling question of whether the commercial objectives of multinational agribusiness in the Third World cheap labor, low-priced crops, and tax advantages - have anything in common with the needs of hungry people.

Although a few of the selections, notably Brown's and Yudelman's, stray from a strictly agribusiness perspective, an obtrusive editorial commentary consistently trumpets a food-for-profits orientation, at times leaving the impression that the more thoughtful papers are functioning as camouflage for the Council's ultimate goals.

After noting that many countries, to their detriment, have discouraged private investment, the Council observes that "Perhaps the strongest message of this international conference was to the policy makers of these latter countries." To them, the business participants were saying: "We can be of help, but only under conditions that provide us a fair economic return for our efforts." The book provides substantial unin-

tended support for declining this offer.

GARY L. OLSON

Moravian College

Gladstone and Radicalism: The Reconstruction of Liberal Policy in Britain 1885–1894. By Michael Barker. (New York: Barnes and Noble Books, 1975. Pp. 308. \$17.50.)

During the late nineteenth century the British Liberals transformed themselves from a party of laissez-faire into a party of economic intervention and social reform, a change that culminated in the great reforming government of 1905-15 which laid the foundations of the welfare state in Britain. Barker details a moment in this transformation, the period between 1885, when the Radicals were a minority group in a Whig-dominated party, and 1892, when the party as a whole fought an election on a radical program, which included Church disestablishment in Scotland and Wales, electoral reform, reform of the land laws and local government, and measures to control conditions of employment in industry. The book also deals (unavoidably) with the issue of Irish Home Rule, and with the evolution of the Liberals from a parliamentary caucus to a nationally-organized party.

It emerges that these three political developments were interconnected. The conversion of Gladstone and the bulk of the Liberal leadership to Home Rule largely alienated the Whig elements in the party, including the landowning peers. This immediately changed the political complexion of the parliamentary Liberals: in the longer term, the radical cause was promoted still further by the withdrawal of Whig financial support and the party's increased reliance on its national organization (the National Liberal Federation and its Scottish counterpart), which was aggressively radical in outlook and moreover contained vocal Welsh and Scottish contingents. The final lurch to radicalism was also brought about by Irish events: the Parnell scandal in 1890 made Home Rule less attractive as an election issue, and Gladstone decided to fill the platform out with domestic reforms.

Barker's originality lies mainly in his reassessment of Gladstone's role in this transformation. The latter is often portrayed as unenthusiastic about radical reforms and preoccupied in his later years with the Irish question. Barker destroys this portrait and brings out the full ambivalence of Gladstone's character: on the one hand, the landowner and churchman who believed in the aristocracy's natural right and duty to govern the country; on the other, the committed social reformer who supported the aims of trade unionism and responded sympathetically to the striking dockers in 1889. This last reaction was again a result of events in Ireland, where the united action of the peasants against their landlords persuaded Gladstone that combination was the only effective way for the poor and helpless to obtain justice.

The book is written for a specialist audience and assumes a fair knowledge of the period's political history on the part of the reader. It begins and ends abruptly, without attempting to extract any general themes from the detailed discussion of political events which are the book's staple. This concentration of subject matter also means that no attention is given to general currents of political thought, or to the economic conditions which form the background to many of the events described. Within its chosen limits the book is both scholarly and readable, but it is rather less than a complete account of the emergence of a "new liberalism" in nineteenth-century Britain.

DAVID MILLER

University of East Anglia, England

Labour, Race, and Colonial Rule: The Copperbelt from 1924 to Independence. By Elena L. Berger. (Oxford: Clarendon Press, 1974. Pp. xiv, 257. \$19.25.)

Among metal mining industries in Africa, the copper mining industry of Zambia is only exceeded in size and value of output by the gold mining industry of South Africa. It was established by British, American, and South African mining companies during the 1920s. The record of resulting relationships between the local subsidiaries of those companies, their workers, and successive territorial governments, before and after the birth of Zambia in 1964, is richly documented by archival materials, company files in particular, which have been available for scholarly research in Zambia.

Indeed Zambia's industrial enclave is renowned among students of labor history and urbanization in newly developing countries. It has even given rise to a distinctive tradition of sociological scholarship sponsored by the former Rhodes-Livingstone Institute, its Zambian successor, and the University of Manchester. Recently economists and political scientists have also mined nuggets of knowledge in these hills. Elena Berger's work, a superb history of industrial relations on the colonial era "copperbelt," meets an obvious need while it complements a number of other studies. Other writers have used materials that appear in Berger's

account for diverse analytical purposes. Her book may now be read as a reliable and systematic guide to the documentary record itself.

What is there for the political scientist particularly to note in a history of industrial relations under colonial rule? To begin with, the employers were large international firms, and since there is an ongoing debate about how progressive or reactionary such firms may have been during the colonial era, students of that question would do well to read this book. They will find that the companies, the colonial government, and the African workers favored racially exclusive unions in opposition to the white miners' union, which wanted a racially united union, a closed shop contract, and a policy of equal pay for equal work. Careful readers will discover that sound trade union principles may lend themselves to reactionary uses in colonial situations. In colonial settler societies all industrial relationships are distorted by the mechanisms of national or racial domination. This is not exactly news. But succinct and definitive studies, like Berger's, ought to be read by those who venture to make broader assessments of the relationship between capital and labor in ethnically divided communities.

Increasingly, students of multinational enterprise may seek precedents for effective action by international labor organizations in their confrontations with transnational corporations. Dr. Berger has recorded an instructive instance of international labor action that rescued the inexperienced and factious African mineworkers' union from the brink of a catastrophic defeat in the mid-1950s. Similar actions may help to secure the rights of labor in numerous countries today.

Finally, Berger shows that public authorities in colonial Zambia (then Northern Rhodesia) repeatedly relied upon the mining companies to initiate and implement policies of broad social and political significance. For example, from 1954 onward it was public policy to abolish the industrial color bar. Public authorities, however, spinelessly shifted the burden of carrying out this policy from themselves to the mining companies. True to form, the colonial government tried to get a cheap industrial solution for one of its big political problems. To be sure, the companies went along, but resentfully and with self-serving conditions that were detrimental to the public weal. In brief, the cheaper immediate solution entailed excessively high long-term social costs: in this case, a "dual" or racial wage system that could only be abolished at great national expense to the detriment of sound policies for balanced national development.

This lesson is relevant to studies of the planning process in all countries which depend upon private enterprise to effectuate major public policies. It is surely relevant to newly developing countries which depend upon the operations of transnational firms. It also warns us to be wary of the political doctrine of corporate social responsibility.

RICHARD L. SKLAR

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Israel, The Korean War and China: Images, Decisions and Consequences. By Michael Brecher. (Jerusalem: The Jerusalem Academic Press for the Institute of Asian and African Studies, Hebrew University, 1974. Pp. 148. \$6.50.)

Professor Brecher has written a detailed analysis of a nonevent, and a minor nonevent at that: Israel's failure to establish diplomatic relations with the People's Republic of China. But this little book is not to be dismissed: though its substantive content is of exceedingly narrow interest, its method on the other hand is of very general interest indeed. Brecher has a model (he prefers "research design") which is sufficiently structured to be presented in diagrammatic terms. The first element of the model is the "objective" operational environment, consisting of (A) external factors: the global, subordinate (i.e., Israel-centered), other subordinate (i.e., regional, per se) dominantbilateral (i.e., Israel-USA) and (other) bilateral, systems; and (B), the internal factors: military capability, economic capability, political structure, interest groups and elite competition. This objective world is perceived (through a "communications" layer) by the decision-making elite, whose attitudinal prism is formed by images centered on a prior selection of the salient "objective factors." For example, elite actors directly concerned with U.S.-Israel relations will accord priority to the "dominant bilateral relations" factor, and their perceptions will be shaped by that particular facet of the prism. This entire psychological environment will then in turn shape elite conduct, as manifest in the formulation of "strategic" and "tactical" decisions in four major "issue areas": military-security, political-diplomatic, economic-developmental and cultural-status. Finally there is the implementation of decisions by the "service structures" to which the various elite members have access, or in which they have control. The outputs in turn feed back into the operational environment . . . and so on.

Some will dispute the validity of Professor Brecher's model on epistemological grounds,

and some may reject his implied distinction between the realm of objective facts and the "attitudinal prism" through which these facts are perceived. Others may object to his subordination of the purely bureaucratic element in decision making (in the model decisions are mechanically implemented by "structures," and not reinterpreted to fit bureaucratic preferences). But the best argument for the model is Brecher's own use of it to tell his tale, and explain the course of events.

Briefly the facts are as follows: on January 9, 1950 Israel extended de jure recognition to the PRC, being among the first non-communist countries to do so. This "strategic" decision to use Prof. Brecher's terminology - was the logical concomitant of a higher-level strategic decision, Israel's neutralist stance. Chou En-lai responded warmly to Israel's cabled statement of recognition. But Israel made no move to establish diplomatic relations. On June 20, 1950, the Chinese DCM in Moscow approached his Israeli counterpart on the subject. Five days later, before a reply could come from Jerusalem, the Korean War began. The war and its tensions soon forced Israel to abandon its preferred policy of "nonidentification." At the UN, Israel consistently supported the U.S., while reiterating its recognition of the PRC's legitimacy. Another opportunity came after the Korean War, and it was missed again. By April 1955 it was too late: at Bandung the Chinese found the Arabs, and by May 1955 the Chinese flatly rejected a belated Israeli approach on the establishment of diplomatic relations. There matters have rested ever since.

Brecher's very detailed account carefully differentiates between the various protagonists on the Israeli side, and their different perceptions of diplomatic opportunities and diplomatic costs. We follow events as the protagonists saw them, or at least as they said (or wrote) that they saw them - in the framework of the "research design" whose structure is also the structure of the book. The result is a most instructive case-study of collective decision making. For example, at one point Israel was seemingly deterred from establishing diplomatic relations with China by a warning from Under Secretary Bedell Smith that American and American-Jewish opinion would react very negatively. Then we discover that this opinion was convened at a meeting solicited by the Israelis themselves, in pursuit of instructions from Jerusalem to "consult" with the Under Secretary. As a protagonist pointed out to the author, any approach to the State Department was bound to elicit a negative reaction. Had a real decision been made in Jerusalem, the

ambassador would have been instructed to explain Israel's policy and not asked to consult the State Department on its formulation. This explanation induces us to reconsider the images that guided the conduct of the actors in Jerusalem, and to reconsider the validity of their explanations of Israel's diplomatic defeat. It becomes apparent that a shorter-term Euro-and American-centered view of the world prevailed over a minority longer-term and Asia-conscious view. The State Department merely provided ammunition for a battle already decided.

Here as elsewhere, Brecher's model is actually useful as an explanatory tool, even in the presence of what is seemingly all the evidence on a bit of microhistory. This alone makes it a rara avis.

EDWARD N. LUTTWAK

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Interdependent Development. By Harold Brookfield. (Pittsburgh: University of Pittsburgh Press, 1975. Pp. 234. \$8.95.)

This is an important book for political scientists. As its author states: "This is not a book about development. It is a book about ideas about development..." (p. ix). As such, it at least touches upon most contemporary thinking about economic development; but more importantly, in so doing, the work may well realize what must be its author's main purpose — namely, to stimulate thought which, when considered collectively, will provide theoretical constructs useful to the solution of the many problems within which the economic development crisis is embedded.

As the book's title suggests, the author rejects the stale "have-have not" approach to economic development and thus, most of the theory and research based upon that approach. Instead, he defines development as "...the whole process of change brought about by the creation and expansion of an interdependent world system" (p. xi). While he realizes that this definition, as well as the approach it implies, "...is so broad as to be meaningless..." (p. xi), it does allow for a maximization of objectivity such that "development is therefore positive and negative, according to one's goals..." (p. xi).

Given this point of departure, Professor Brookfield introduces relevant thinking from various disciplines as well as from various ideological perspectives within disciplines. The ideas analyzed range, for example, from those of Joseph Schumpeter and Francois Perroux through those of Albert Hirschman and Gunnar Myrdal to those of John Friedmann and Brian Berry. In addition, the ideas of those who have had a more direct impact upon public policy formulation, e.g., W. W. Rostow and Raul Prebisch, are examined with special care. Finally, the ideas of the Marxists, both old and new, are explored, not as a body of useless dead-end thinking, as is so often the case, but as a creative and dynamic, albeit limited, part of development theory.

It is in this last area of relevant thinking that Brookfield is most open to criticism, not because of what he covers, but because of what he does not cover. I am amazed that there is not one mention made of Paul Baran throughout the entire volume. Baran's writings, especially his The Political Economy of Growth, are seminal to two decades of neo-Marxist development theory, including much of that discussed by Brookfield, and have influenced many non-Marxist development theorists, including some of those discussed by Brookfield. Further, Baran's contributions in the area of development are based upon the argument that development is indeed interdependent. Brookfield should have devoted at least some space to indicating why he feels Baran's ideas are irrelevant to the issue at hand.

In his treatment of "ideas about development," Brookfield relies heavily upon several concepts, the appropriate consideration of which should stimulate relevant and useful thinking. Brookfield is clearly too sensitive to the complexities of the economic development crisis to establish a model for the achievement of this purpose. Instead, throughout the book, he develops several dimensions upon which he obviously feels future theory and research ought to be based. In relation to development, these dimensions include: (1) its historical nature; (2) its spatial nature; and (3) its structural nature. While these are superficially very general categories, within the context of "ideas about development," Brookfield disassembles each of them and suggests how each might be reassembled, leaving the final decision concerning reassembly, however, up to the empirical results of future research based upon one or more of the dimensions. It should be added that even though the dimensions indicated constitute the unifying principle of Brookfield's effort, he does not ignore those dimensions upon which perhaps too much of the extant theory and research about development are based.

A final note: Harold Brookfield is a geographer with both academic and field experience. As a result, he brings a refreshing view of reality to a body of thought which thus far has been introduced to political scientists primarily by economists. Thus, assuming that none of the above remarks have been sufficiently motivating, reading this book should at least broaden the relatively limited perspective from which most political scientists view the economic development crisis.

PHILLIP ALTHOFF

Kansas State University

Legitimacy and Ethnicity: A Case Study of Singapore. By Peter A. Busch. (Lexington, Mass.: D.C. Heath & Co., 1974. Pp. 157. \$14.00.)

Peter Busch is interested in the Lipset theory that economic development breeds legitimacy, and in exploring the effects of racial attitudes upon legitimacy. In 1970 he and his wife administered 3,316 questionnaires to Singapore secondary school pupils, and he conducted several intensive interviews as well. Utilizing linear regression coefficients and multiple correlations he finds that many more Malays (especially poorer ones) feel inferior than do Chinese. Among Chinese, there is strong correlation between a feeling that Malays are inferior and that they are advancing more slowly than Chinese, but perceptions by Malays that Chinese are advancing actually seem to ease their inferiority feelings a bit. Those respondents (Chinese and Malay) who feel their race is inferior, and who feel that Malays are advancing more slowly than Chinese, also tend to score lower on the legitimacy scale. Significant numbers of poor respondents score high in legitimacy, however. Malays who score high often do not believe that racial equity exists, but do believe that the government wants such equity. Those Chinese who believe the Malays are making progress are more apt to want multiracial neighborhoods. Those in nonintegrated neighborhoods are more allegiant to Singapore than those in mixed neighborhoods. Years in school tend to weaken legitimacy feelings, and strengthen racial prejudice. Although Professor Busch does not mention it, his conclusions do not match Singapore's orthodoxy; they suggest that mixing neighborhoods and indoctrinating school children does not necessarily produce allegiance or reduce racial prejudice.

Of course, one should not take any of this too seriously. The author does not reproduce the questionnaire or any numerical breakdown of responses (e.g., what percentage of respondents scored high on legitimacy?); the distribution curve seems to be quite narrow (there

might be a preponderance of high scores, and little variation in responses among various groupings, so even very minor differences become statistically significant). He gives no information about the isomorphism of his sample, save for a comment that the elite English school portion of it is overweighted with wealthier pupils; in the absence of more extensive categorical grouping it is impossible to tell how subsets of the sample responded (does legitimacy decline with grade level in nonelite schools, or among those most racially prejudiced?). The R² in his tables (the proportion of variation explained by all the variables in the table combined) is often very small (e.g., all six variables in this table together can only explain 30 per cent of the variation in response to these questions). His more elaborate "policy equations" (p. 106) yield little information about the reliability of the parameters or the estimating equation itself (e.g., the standard error); there is no way to identify which of the thirty-seven variables are the most significant, His legitimacy scale is based on two questions: whether you would take a much lower-paying job if the government asked you to, and how willing you are to fight and die for Singapore. Yet he defines legitimacy as a feeling that political institutions are right and proper even when coercion, fear, and immediate material incentives are absent. Coercion and incentives are not absent in Singapore, even for jobless pupils; he did not ask whether the respondents would be willing to fight even if there were no coercion to do so, and a person answering "no" to these questions is not necessarily expressing disapproval of political institutions.

The findings seem, if anything, to point to a need to publicize more strenuously the economic advances of both races, to keep neighborhoods segregated and morally integrated, and somehow to reform English-stream schools. Busch instead calls for winning over cultural leaders, uplifting the poor, and inducing transactions that cross racial lines; the relevance of his findings to these prescriptions does not register with me. There is perhaps some very qualified support for Lipset here (though Busch does not focus it), but little to conclude about the relationship between ethnicity and legitimacy. He does ask (but not answer) some interesting shotgun questions about legitimacy: does it aid stability, how is it transmitted, how do you adapt socialization so as to create it, whether a person morally integrated into a neighborhood might thereby accord legitimacy to the state as a sort of overflow effect, how people separate state and government in their legitimation, etc. A journal article would have sufficed to present the data, and might have forced the author to focus more sharply on some conclusions.

ROBERT E. GAMER

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Political Change in Britain: The Evolution of Electoral Choice. By David Butler and Donald Stokes. 2nd Edition. (New York: St. Martin's Press, 1969, 1974. Pp. 500. \$19.95.)

In 1969, David Butler and Donald Stokes published the first edition of Political Change in Britain. Based on data collected in three national surveys (1963, 1964, and 1966, with additional reference to 1959), the volume combined the schools of electoral analysis of the University of Michigan's Survey Research Center and the detailed historical work of Nuffield College, Oxford. This tour de force won the Woodrow Wilson Award of the American Political Science Association as the best book published in political science in 1969.

To issue a second edition of the work that is not merely an updated gloss merits additional critical praise. Butler and Stokes have extended the study through 1970 by adding several new surveys: The initial panel study was re-interviewed in 1970; a completely new set of respondents was examined in 1969 and again in 1970; and an additional group was surveyed in 1970. The second edition combines all the surveys into a comprehensive account of British electoral behavior. In doing so, Butler and Stokes have covered a period of Tory as well as Labour rule and the entry of a new generation of voters into the electoral market. There is little reason to doubt that their findings are of general significance, reflecting more than the peculiarities of a specific time frame.

A review of the second edition must serve at least two purposes. Not only should it comment on the additions but it should take the opportunity to delve more deeply into specific elements of the authors' arguments. In the second edition, Butler and Stokes have not only provided new data but they have modified the volume's structure. The "replacement" of political generations is now given the same emphasis as the analysis of party alignment and short-term flux. In their treatment of "replacement," they have elucidated a phenomenon of crucial theoretical importance. Political science has been very slow to examine the consequences of the general population growth that has characterized the world in the postwar years. It would be very surprising if the increases in the relative size of the elderly and

the differential birth and death-rates of social class, ethnic, and sex groupings did not have important political consequences. Butler and Stokes have demonstrated that they do, and they have shown how to use demographic change characteristics as explanatory variables. This is illustrated particularly well in their definitive solution to the theoretical puzzle of the "Working Class Tory."

The authors have also amplified several of the "gems" of the first edition. The explanation of the symmetrical national vote swing and the exploration of the absence of issue coherence among the electorate are retained and joined to an updated analysis of the pattern of vote switching and a critique of the argument linking electoral changes to fluctuations in the economy. Each of these is a standard for the analysis of crucial theoretical issues as well as a description of British voting behavior and public opinion.

The study rests on an "open framework," which seeks to encompass numerous foci in the analysis of electoral change. It is this "open system" that gives the volume its encyclopedic quality and its immense utility as a data resource. At the same time, it provides an air of theoretical confusion. Two small but significant examples reflect ambiguities in choices of explanatory and descriptive variables: in the second edition, the "pattern of housing" emerges as a significant explanatory variable for persistence and change in voting behavior. There is, however, no discussion as to why it "emerged" in 1970 but was of little importance in the first edition, and, therefore, there is no attempt to assess its theoretical consequences. Apparently in response to earlier criticisms, in this edition, Butler and Stokes use the standard polling definition of the working class. As they note, the categorization into classes is crucial to the analysis, but they neither provide a theoretical justification nor explore the implications of that decision.

A fundamental difficulty associated with the use of an "open analytic framework" is that it is not conducive to the elaboration of interrelated explanatory propositions. As a consequence, the theoretical significance of the empirical findings is diminished. Both editions emphasize two major elements of long-term change: (1) a cleavage in the oldest age cohort based on religious ties and (2) the growth of class-based politics. The second edition also reports the "paradoxical finding" that young voters are less likely "to see politics as an arena of class conflict" (p. 411). Butler and Stokes use this "aging of the class alignment" to account for the declines in voter turnout and

the increases in voting flux present in the late 1960s. Their attempt to explain these changes uses several explanatory factors: e.g., general growth in affluence, change in generational life-styles; changes in the appeals of party leaders. While these factors are not mutually exclusive, they derive from different theoretical positions and have different theoretical consequences: Will Britain's recent economic travail awaken notions of class conflict among the young? Will changes in the goals and actions of the party leaders effect greater class and party loyalties? Do the changes evident in the new generation indicate the wave of the future? Butler and Stokes provide no answers. Their concern with detailed descriptions precludes the elaboration of theoretical arguments.

Furthermore, the complexity of their framework makes it particularly difficult to join arguments found in the volume. A potentially fascinating response to the problem of the aging class alignment is to link it to the massive growth of new voters in the late 1960s. If, as Butler and Stokes argue, new voters are most prone to weak political loyalties and to voteswitching, can we account for the apparent decline in class-based voting by the difficulties inherent in socializing large numbers (the products of the postwar population growth and changed electoral laws) and to new voters' tendency to be vote-switchers? If so, will there be a return to the traditional electoral divisions as they "settle in?" Will they be socialized by the traditional political parties or new competitors? Will they remain a relatively undifferentiated political generation? There is neither an attempt to interrelate the phenomena nor to respond to the particular issues.

The answers to these questions will tell us much about the future of British politics but also about central theoretical issues. For Butler and Stokes to have sought to answer them would not have required them to don the garb of prophets, as they imply. Rather, it would have demanded the abandonment of their vague "open analytic framework" for the elucidation of precise theoretically interesting arguments. Their diversity of approach naturally results in a vision of highly complex change.

ALAN S. ZUCKERMAN

Brown University

Latin America: The Struggle with Dependency and Beyond. Edited by Ronald H. Chilcote and Joel C. Edelstein. (New York: Halsted Press of John Wiley & Sons, 1974. Pp. 781. \$24.75, cloth; \$9.50, paper.)

In an introduction, the editors outline the

role of foreign economic factors in shaping domestic political and social structures in Third World nations, warning the leaders of the developing nations that natural resources are not abundant enough to provide the North American consumption level for as little as half the world's population, even allowing for technological innovation. Then other scholars, each dealing with one Latin American nation, give detailed critiques of underdevelopment in Argentina, Chile, Cuba, Guatemala, Brazil, and Mexico. Three of these countries - Argentina, Brazil, and Mexico - have significant industrial sectors in regional and world terms, yet the progress of industrialization in these nations has not lifted them from developing to developed in either the economic or political spheres.

From the standpoint of scholarship, the outstanding nation study of the six is on Cuba, by Donald W. Bray, political scientist, and Timothy Harding, historian, both at California State University at Los Angeles. They manage to chronicle the successes and failures of the Castro revolution without omitting setbacks, unlike those specialists on Cuba who portray Castro favorably at the cost of accuracy. Bray and Harding capture the significance of the basic changes in Cuba during the past sixteen years without either apologia or polemics.

Bray and Harding see the Committees for the Defense of the Revolution, which exist in every neighborhood of every town, as representing positive mass support for the Revolution. Unfortunately, they do not see the CDR as threats to the privacy and thoughts of Cubans as individual human beings. They correctly perceive the Cuban government's professed rejection of meritocracy, a point missed by many writers on Cuban politics. That is, the Castro regime rejects the notion that those endowed with greater intelligence should live better than others, though in fact the opposite happens in practice. The authors also point out that the Cuban Revolution firmly anchored the Third World struggle in the Western Hemisphere.

James D. Cockcroft, a sociologist at Livingston College, Rutgers, in analyzing Mexico, defines Mexico's mixed public-private economy as state capitalism, with the government controlling a higher percentage of major corporations than do private Mexican investors. Foreign capital remains the key junior partner, though President Luis Echeverría in recent years has expropriated new industries ranging from tobacco to motion pictures. Cockcroft cites several million Mexicans with marginal living standards — with rural Mexicans being more impoverished than urban residents — as a shortcoming of land reform programs. Despite such failures of the ongoing Revolution, he found that the aroused alienated groups were fragmented and that enough of the dissidents had been co-opted into the system to help the power elite sustain that system.

Brazilian sociologist Thetonio Dos Santos finds his country, one of the largest nations in the world both in land area and in population, not to have risen above underdevelopment despite its economic vigor. Even with energetic industrialists, Brazil retained its rigid dependence on the agrarian-export sector. Its industrial elite after 1937, aware of the weaknesses of other classes, knew how to mobilize its own class to defend its interests and to manipulate the government. Industrialization meant not only substitution of imported manufactured goods by locally produced items, but also the creation of new commercial relations characterized by importation of machinery and intermediary products. Foreign capital retained control over the machines and intermediary goods, giving Brazil a dependent industrialization. Dos Santos traces the failure of nationalism to evoke successful political radicalization, resulting in military rule since 1964.

One of the nations studied, Argentina, escapes the population explosion bedeviling most Third World nations, yet it continues to suffer inflation, violence, and political chaos. With an annual increment in population of only 1.5 per cent, Argentina cannot blame a baby boom for its triple-digit inflation in 1974 and 1975. To document the poor political and economic leadership, Juan Corradi, a sociologist at New York University, gives an historical overview from the nineteenth century through the return of Juan Perón in 1973, told in terms of class conflict.

Italian and Spanish immigrants in the 1880s kept Argentina ethnically European, free of the problem of integrating large numbers of Indians into the national mainstream as in many other Latin American nations. After some decades of middle-class power, 1930 is cited by Corradi as the year the landed upper class returned to political power under the imperative to industrialize. He categorizes the new industrial bourgeoisie as politically impotent and traces the power gains of organized labor under the Peron dictatorship. Argentina went from militarism to populism. Corradi found that after 1955 foreign capital was the powerful political factor. He de-emphasizes the decisions of Argentine leaders themselves in the failure to expand the economy without running the printing presses, which gave Argentina runaway inflation based on currency not backed by any increased

productivity.

Susanne Jonas, a staff member of the North American Congress on Latin America, a private research agency, deals with Guatemala. She found that during the colonial era, Spain put Indian lands into huge estates owned by a few Spaniards, a pattern which has carried over into modern times. With the loss of British Honduras, Guatemala lost mahogany exports to Britain. Coffee became a major crop despite investments by United Fruit Company in banana production. In the 1960s, United Fruit sold its banana plantations, shifting to the less risky marketing and processing of bananas for export sales.

The Guatemalan government in 1968 took over installations of the International Railroads of Central America after the corporation became insolvent. This rescue operation cost the Guatemalan treasury \$31 million in government loans and unpaid wages. Jonas found Guatemalan industrial technology to be capitalintensive rather than labor-intensive, using machinery to do a job which might have employed dozens of workers, to keep production costs competitive in world markets. Industrialization failed to alleviate serious unemployment. The value added from manufacturing more than tripled between 1950 and 1969, but industrial employment remained at 11.5 per cent of total employment from 1950 to 1964.

She summarizes Guatemalan violence as police-state law and order. Both leftists and rightists have murdered many opponents. Jonas contends that the rightist actions were indiscriminate whereas the leftist actions were discriminate. She admits guerrillas kidnapped third-party victims and killed diplomats, including U.S. Ambassador Gordon Mein and West German Ambassador Count von Spreti. The rightist White Hand machine-gunned a beauty contest winner and a congressman in a wheelchair. Yon Sosa guerrillas shot thirteenyear-old track stars. Political bloodshed has been too extensive to draw up a complete list of victims and motives. Jonas found that marginalization of the lower classes vielded institutionalized unrest. The government cannot make necessary reforms and thus continues to rely on force.

Marcelo Cavarozzi of the Di Tella Institute of Buenos Aires and James F. Petras, at State University of New York, Binghamton, cover Chile. They analyze the presidency of Marxist Salvador Allende sympathetically and that of Christian Democrat Eduardo Frei critically. The Christian Democratic policy was to achieve or to seek economic development through copper production in mixed public-private corpora-

tions with the government being the senior partner. Frei's gradual pace is contrasted unfavorably by the authors with the radical changes of Allende. But they do not include in the comparison the world's worst inflation, at the runaway rate in the increased cost of living of one per cent per day, resulting in a takeover by right-wing generals who have suppressed all political freedoms. Over all, this volume provides useful background for those seeking political perspective for Latin America's economic underdevelopment.

MARVIN ALISKY

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The Last Days of United Pakistan. By G. W. Choudhury. (Bloomington, Ind.: Indiana University Press, 1974. Pp. 239. \$10.00.)

G. W. Choudhury is eminently qualified to write a book on the events surrounding the loss of East Pakistan in 1971. The author is a Bengali who for many years taught at Dacca University (now in Bangladesh) and later served in a number of capacities in the Pakistan government. When the curtain came down on the Bangladesh tragedy, Choudhury was a special counselor on constitutional affairs to President Yahya Khan. Since those fateful days, he has made his home in the United States, where he is professor of political science at North Carolina Central University. Choudhury identifies with that element which wholeheartedly supported the original Pakistan movement. Unlike so many Bengalis, he did not succumb to provincial passions, and this more recent of his many books is an important personal account of the final chapter in the quest for a semblance of Muslim unity in South Asia.

In examining Bengali nationalism, Choudhury is very critical of all those Pakistani leaders who from the inception of the state, ignored every opportunity to ameliorate East Pakistan's grievances. Political inferiority and economic disparity fueled Bengali emotion, and the Islamic religion could not solidify a multiethnic and territorily divided state. The author comments that

a federal union can be strengthened by giving cultural freedom and autonomy, but Pakistan's attempt to impose uniformity where diversity was desirable had unfortunate consequences. Every attempt made by the Pakistan government to foster a cultural uniformity based on Islamic culture in East Pakistan produced a sharp reaction; the Bengalis began to look more and more to West Bengal [in India] for cultural affinity and bonds (p. 11).

Choudhury is emphatic in the argument that Pakistan's leaders played into the hands of those Indians who were waiting for an opportunity to fracture the Pakistan design. In the culmination of their self-fulfilling prophecy, Pakistanis who had always insisted India would sever East Bengal from Pakistan as a prelude to destroying the entire state, did virtually nothing to preserve their tenuous unity.

The crucial period for Choudhury is the Ayub Khan decade (1958-1969). Although he admires Ayub the man, the author asserts that Ayub was no statesman and hence was easily misled by his "friends" as well as his foes. Choudhury analyzes the decisions to precipitate a war with India in 1965, to accuse Mujibur Rahman of treason and try him in the infamous Agartala Conspiracy case, and finally, to celebrate the regime's "success" during the Ayubian development decade. In each instance, Ayub was misguided or deliberately misinformed by his advisors, explains Choudhury. Moreover, after viral pneumonia struck him down and left him physically and mentally weak, there was a concerted effort to have him replaced. Choudhury would have us believe that Ayub could have managed the storm that blew up around him if he had endeavored to satisfy Bengali demands. But Ayub could not take the measure of the situation, and his ignominious demise heralded the eventual breakup of the Muslim state.

Yahya Khan, Ayub's successor, was given one last chance to salvage the Pakistan dream. Choudhury apparently was more intimate with Yahya than with Ayub. He therefore tends to be somewhat apologetic when describing his role in the unfolding scenario. Yahya, he tells us, tried to preserve the unity of Pakistan while politicians like Zulfikar Ali Bhutto and Sheikh Mujibur Rahman were "playing unscrupulous roles" (p. 163) and seemed not to care what happened to the country so long as their personal ambitions were gratified. But Choudhury's attack is not only directed at Mujib and his Awami League followers, or Bhutto and his Pakistan People's Party, or indeed, even at those members of the military junta whom Yahya was compelled to accommodate. Yahya might well have come to terms with Mujib, says Choudhury, had it not been for the Bengali economists and some foreign economists "financed by the Ford Foundation" who "were the loudest in making extreme demands, and they were, to a large extent, responsible for the failure of the Dacca dialogue" (p. 168).

Choudhury condemns the atrocities committed by the Pakistani army against the

Bengali population but he also feels that some display of military power was essential given Indian machinations. Moreover, the Soviet Union's decision to favor India in its running dispute with Pakistan is an added feature in the drama.

The Last Days of United Pakistan makes fascinating as well as informative reading. Although it must be categorized as a largely subjective account, it represents the first inside interpretation of Pakistan's most trying hour. It cannot be the last word on the subject, however, and one can only hope that others with first-hand knowledge will see the necessity of rendering their versions.

LAWRENCE ZIRING

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The Labour Party and the Struggle for Socialism. By David Coates. (New York: Cambridge University Press, 1975. Pp. 257. \$14.95, cloth; \$5.95, paper.)

Professor Coates examines the role of the Labour party in the creation of a socialist Britain. The author thinks of socialism as involving common ownership of the means of production, distribution, and exchange. He analyzes the history of Labour party programs and policies advocated and implemented with a view to assessing the Party's potential for attaining this socialist goal. Basically Coates argues that if Labour's past is prelude to the future, then parliamentary socialism will prove to be an inadequate vehicle for the creation of a socialist society and should be replaced by revolutionary socialism. The author hopes that this analysis will initiate further debate about whether or not the Labour party has been, could be, and should be socialist.

The main body of the book focuses on the least controversial aspect of this debate: historical analysis of the extent of socialism advocated and implemented by Labour leaders and enumeration and evaluation of the obstacles which precluded further socialist progress. The first four chapters examine the early Labour party, the Labour Governments of 1945-1951, the Labour party in Opposition 1951-1964, and the Labour Governments of 1964-1970. This chronological and detailed analysis of the statements and policies of various Labour leaders is followed by chapters on "The failure of the socialist promise," "The weakness of the Labour Left," and "The exhaustion of a tradition," which summarize in more general terms the author's analysis of the obstacles to socialism and recommend action, such as the formation of a revolutionary socialist movement, which socialists should take in the future. The inventory of obstacles to socialism assessed by the author includes not only long-standing institutions such as the civil service and perennial problems such as the balance of payments, but also contentious policies within the Labour party itself such as the commitment to parliamentary socialism and Labour's failure to mobilize a radicalized working class.

Coates apparently believes that the controversial element in his analysis is not so much his interpretation of Labour history as it is (1) his projection that Labour's past indicates that the Party will never be capable of implementing socialism as he defines it, and (2) his recommendation that the parliamentary route to socialism should be abandoned in favor of revolutionary socialism. These controversial conclusions are at variance with the opinions of authors such as Eric Heffer and Ken Coates who disparage revolutionary socialism and argue that socialist change can still be achieved via parliamentary action by the Labour party. (See Eric Heffer, The Class Struggle in Parliament [London: Gollancz, 1973] and K. Coates, "Socialists and the Labour Party," in The Socialist Register 1973, ed. R. Miliband and J. Saville [London: Merlin Press, 1974], pp. 155 - 178.

In summary, the author does a good job of demonstrating that based on its past record, the present-day Labour party is unlikely to be able to implement socialism as Coates defines it. Although this conclusion seems to be neither very original nor very controversial, it is perhaps a necessary step in any analysis of the Labour party's socialist potential, for Coates claims that revolutionary socialism will not gain acceptance until it is clearly demonstrated that present Labour party trends will not lead to socialism.

The major weakness of the book lies in the author's failure to support his claim that the "revolutionary Left need not always fail" (p. xi). Coates is hopeful about the possibilities of forming a revolutionary socialist movement because the Labour party's "total domination" of working-class political loyalties is fragmenting, thereby allowing the growth of a revolutionary socialist Left. He does not, however, explicate strategies which would or could be used by revolutionary socialists in coping with British economic problems.

can only form after the parliamentary road to socialism has been demonstrated as inadequate. I feel that Coates's rebuttal begs the question, since it provides no indication of how revolutionary socialism would deal with concrete problems.

LYNDELLE D. FAIRLIE

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The Chinese Red Army: Campaigns and Politics since 1949. By Gerard H. Corr. (New York: Schocken Books, 1974. Pp. 192. \$8.95.)

The People's Liberation Army (PLA), the armed services of the People's Republic of China, is one of the key institutions to be considered in the analysis of politics in contemporary China and of the socioeconomic transformation of that nation. The importance of the PLA has been reflected in the work of long-standing merit of such China scholars as Chester Cheng, Ellis Joffe, John Gittings, and Ralph Powell. Since the time of the Cultural Revolution in the late 1960s, research and analysis focusing on the PLA has received additional impetus resulting from the new levels of involvement in China's political life by the Army. Among these more recent efforts are the work of William Whitson and of Ying-mao Kau. By and large, however, these writings tend to be addressed to scholars concerned with other aspects of Chinese politics rather than to students of other societies or to the general reader. A book which satisfied these broader needs would undoubtedly be welcome.

Gerard Corr, a British journalist with experience in Asia, claims, rightfully, that his book, The Chinese Red Army, "is not for the expert." It is not likely, however, to suit the needs of the nonexpert either. If it is intended as an overview of the Chinese Army since 1949, it fails to treat the all-important nation-building role of the PLA as an economic work force, as a cultural and educational vanguard, as a training ground in technical skills, and as an integrative and politicizing environment for its personnel. Corr does devote one chapter to the history of the tensions between the Army and other elements of the political system, such as the Communist party, but the bulk of the book is concerned with the four major post-1949 "campaigns" of the PLA: Tibet; Korea; the Indian border; and the Soviet Border. Aside from Korea, however, these have been very limited وسيمطلائين أداستينا عما بالاستاد يبكط بالما استثبارا

political context of military activities without detracting from the military emphasis. The result is usually an inadequate treatment of either aspect, and Corr's book is no exception. The narrative is superficial and tends to confuse evidence with conjecture.

The principal identifiable theme of the book is that the four military campaigns were restrained and were justified on the grounds of historical claim or national security. Corr is attempting to counter the cold war image of China as militarily aggressive. The aim has merit, and is consistent with the revisionist trends in recent studies of China's foreign policy. Unfortunately, Corr's effort is not convincing on two counts. First, he does not cite any sources. There is perhaps no need for footnotes in a general work of this type, but one would expect that the bibliography would reasonably lead the reader to reputable books in the field. Yet the section on the Korean War, for instance, makes no mention of the classics on that subject by Alan Whiting, Alexander George, or I. F. Stone. Secondly, Corr's credibility in attempting to look at China's military activities "as they may have been viewed at the time from Peking" (p. 10) is weakened by his persistent use of racial stereotypes: "the men who fill the ranks of the PLA today ... have a quick, intuitive intelligence without being overendowed with imagination" (pp. 25-26) and "Mao ... anxious for quick results, with his patience, like that of most of his fellow Hunanese, burning on a short fuse" (pp. 47-48). I would suggest that it is precisely this kind of "pop" psychological evaluation which has been at the root of cold war views of China, and of foreign policies based on those views.

Since Corr fails to provide an adequate treatment of the Chinese army, the general reader must still rely on scholarly works. Among the most readable, although it is unfortunately several years out of date, is John Gittings's *The Role of the Chinese Army* (New York: Oxford University Press, 1967).

PIERRE M. PERROLLE

Wheaton College, Massachusetts

European Parliament Digest, Volume I: 1973. Edited by Mary Edmond. (Towota, N.J.: Rowman and Littlefield, 1975. Pp. 218. \$22.50.)

The full reports of the proceedings of the European Parliament for 1973 cover 2119 pages. Here they are reduced to 194 pages plus an introduction and two indexes. The latter are

by far the most valuable features of the book. Every speaker and his contributions to the debates are listed (the only one omission I noted was that Mr. John Brewis's speech on page 116 is not recorded). The main topics discussed in the Parliament are covered in a separate index. Scholars wishing to study the parliamentary record of individual speakers, or of groups of speakers, will for the first time be able to do so in English without having to plough through the full reports of that body. It is difficult, however, to think of many alternative uses for this volume. Although a library which takes the "European Hansard" will find this digest a useful, but expensive aid to research, it is no substitute for the complete transcripts themselves.

The speeches are clearly and judiciously summarized. The print is handsome and the format of the book easy to use and easy on the eye. Most librarians, however, could probably spend \$22.50 more usefully on other reference books concerning the activities of the EEC institutions.

D. J. WENDEN

All Souls College, Oxford

Armies in Revolution. By John Ellis. (New York: Oxford University Press, 1974. Pp. 278. \$10.95.)

This book bases a successful revolution on the "militarization" of a political movement and the "politicization" of its military arm (p. 15). A movement must be militarily disciplined to win the revolutionary war or takeover - yet. paradoxically, it must also be politically democratic to assure a "social revolution" along popular lines. The paradox is resolved by an "authentic people's army" (p. 250): Its consensual base affords enough military cohesion yet permits enough political responsiveness for the revolution to succeed, as evidenced by the Chinese Civil War (chap. 8). Conversely, a "regular army" that gains cohesion through control from above rather than consensus from below can still win the military takeover but at the cost of popularly guided social transformation afterwards, as in the English Civil War. the French Revolution, the Prussian Reforms, and the Russian Revolution (chaps. 2, 4, 5, 7). But without a cohesive army, not even a takeover can succeed, as shown in the Paris Commune (chap. 6). People's war, claims Professor Ellis, is thus the only way to make revolution effective as well as responsive.

Since this pattern is nearly a truism, the anomalous case - America's War of Indepen-

dence (chap. 3) - may be more instructive than the pattern itself. Here, as the author himself points out, a military revolt succeeded with an army that was neither militarily ready nor politically responsive. He attributes this success to the "personal greatness" of George Washington (p. 249); but this reason remains outside his theory even though it could apply to other cases too (e.g., Cromwell, Lenin, Mao). An alternative reason may be lack of cohesion in the regime's forces as well as in the rebel army; hence, not the level but the ratio of military discipline between rebel and regime factions may be crucial to revolutionary victory. Stated this way, the author's thesis easily covers the American case: the exception helps us to refine the rule.

However refined the account may be, its relevance seems to be timebound. The writer himself grants that "technological parity" between the ruling and rebelling forces is more likely in past than in present societies. Yet social factors may thereby be more crucial to past than to prospective revolutionary victories. Specifically, the successful occurrence of revolutionary wars in premodern states might derive from military discipline and political solidarity, but the nonoccurrence of such wars in modern states could be due instead to the regime's technological superiority. The book does not check for this possibility insofar as its design does not include states at varying plateaus of development. The work thus remains a history rather than a theory of revolutionary outcomes.

A dilemma for this and other revolution studies involves the subject and object of analysis. The object is verification of, in this case, the hypothesized effect of military strategy upon revolutionary success. Yet the subject all but eludes verification. For revolution is definitionally vague - and indeed is not defined by Ellis – thus precluding agreed-upon cases (e.g., Prussia, America) and periods (e.g., does the China case end in 1949?). The subject is also empirically rare (for Ellis, n = 7), thus precluding reliability tests. Revolutionary wars may be informationally fraught as well, since the highly charged political atmosphere may impose dual meanings upon such writings as memoranda and memoirs (Ellis's primary sources). Since the subject-object dilemma is unresolved, this book is an illustration rather than a verification of the Ellis hypothesis.

The work's prime asset is its fusion of military science and social science insights. From social science comes an awareness of the impact by "grass-roots aspirations" upon the choice of strategy; from military science comes a sensitivity to the impact of strategy, whether

regular or guerrilla war, upon the outcome of revolution. The apparent assumption is that progress in revolutionary studies hinges on both the "militarization" of social science and the "socialization" of military science. In making this fusion, the author becomes part of that movement within academe — intellectual ecumenism — whose task is to airlift analytical tools and insights across disciplinary boundaries. The importance of that task, I think, overshadows the book's departure from the rigorous methodological standards now emerging in social inquiry.

CRAIG MCCAUGHRIN

University of Pennsylvania

Pluralism and Party Transformation in Lebanon: Al-Kata'ib 1936—1970. By John P. Entelis. (Leiden: E.J. Brill, 1974. Pp. 227. Guilders 68.00.)

Professor Entelis has produced a careful and provocative study of Lebanon's largest political party, the Kata'ib (LKP), better known as the Phalanges. It will be of interest not only to students of Lebanese and Middle Eastern politics but also to political scientists concerned with modernization, fragmented political cultures, and consociational democracy. It takes on added significance because of the important role the Kata'ib has played in the Lebanese civil war, in which 60,000 people may have perished by the end of 1976. But the Kata'ib's aggressive behavior in this crisis, especially at its beginning, which has contributed to pushing Lebanon toward de facto partition, casts a certain irony on some parts of the author's analysis.

The thesis of this study is that the LKP "is the most important and influential political organization in the country dedicated to preserving the system's essential physiognomy while concurrently encouraging meaningful forms of evolutionary or adaptive social change" (p. 9) and that it has achieved considerable success in both of these objectives. Entelis supports his thesis ably, but accepts perhaps a shade less critically than he might have done the party's self-image and its own analysis of its role in Lebanese society and politics. The LKP is important but not as decisively important as he suggests. Certainly, its efforts to preserve "the essential physiognomy" of the system have proven counterproductive and its attempts to encourage "meaningful ... evolutionary" social change without needed political reform have come to naught.

To be sure, the LKP became a major actor in Lebanese politics during and after the 1958

crisis, and one of the most interesting parts of this book is the description of how it finally became part of the Lebanese establishment. But even after 1958 its political power remained limited. The Party held only between four and nine seats in the ninety-nine-member Chamber of Deputies. It showed no clear trend toward increasing its electoral strength: in 1960 it won six seats; in 1964, four; and in 1968, nine. In the last elections before the civil war (and after the completion of this study), 1972, the LKP won only seven seats. Similarly, the percentage of LKP winners among LKP candidates has also fluctuated over these four elections, with 86. 44, 100, and 63 per cent, respectively. As for participation in government, while the LKP held one or two portfolios in most, but not all, of the cabinets of the 1960s, this limited level of representation declined somewhat in the 1970s: there was no Kata'ib member in the Sa'ib Salam cabinets of October 1970 through April 1973; and it is significant that a key provision in one of the early - and abortive attempts to head off the civil war in summer 1975 was that the Kata'ib be excluded from the government. Thus even up to the collapse of normal government (and contrary to the statement on p. 149) the Kata'ib was not to be automatically granted cabinet membership. Furthermore, one could make a case that other political organizations such as the army, the president and his circle, the Maronite church, the banking and business community, certain traditional notables and their clienteles, and other "parties" such as Camille Chamoun's National Liberal Party or Kamal Jumblat's Druze-socialist coalition were as important as the Kata'ib, if not more so, in system preservation. One may not be prepared to agree, therefore, that the LKP was "the single most powerful independent political force in Lebanon" (p. 213). It is more important to realize that its power was military more than political and that the scope of its influence was confined to the Maronite community. Even there it was not unchallenged, as the continuing importance of rival notables like Camille Chamoun, Suleiman Frangieh, and Raymond Eddé indicates.

As for the LKP encouraging social change, Entelis convincingly documents how its liberal intelligentsia was very much in tune with the social democratic ideology of General Fuad Chehab's regime (1958–1964). During that relatively stable era and the first part of the regime of Charles Helou (Chehab's successor and protégé), it was possible for the LKP to try to project a new, nonsectarian, integrative image. Although the author does not mention it, the "integration-through-modernization"

strategy of Chehab was also very good politics for the Kata'ib: it enabled the party to consolidate its gains at the expense of the other leading Christian politicians, especially Camille Chamoun, who had been defeated in the 1958 civil strife. But as Chehabist stability withered away in the late 1960s because of corruption, financial instability, and above all the consequences of the Six-Day War, the Kata'ib reverted to its primordial Maronite nationalism.

The emergence of an armed, militant Palestinian movement in 1968 created an imbalance in the delicate Lebanese equation. Although the Palestinians had no interest in interfering in Lebanese politics, their activities against Israel brought massive reprisals which in turn led to Christian discontent and ultimately politicalsectarian conflict. The Palestinians served to catalyze Muslim-left wing discontent, and the Kata'ib saw the "essential physiognomy" of the system - i.e., Christian dominance threatened. And, as Entelis observes insightfully, the new threat was a powerful stimulus for development of the party and especially its militia. The LKP developed increasingly not as a "positive system challenger" but as the main instrument of "static system maintenance." Entelis dwells mainly on the "positive" aspects of Kata'ib behavior - and understandably so. since much of his research was undertaken during the LKP's social-democratic phase. But in his final chapter he recognizes the trend toward parochial exclusivism, and he concludes by declaring what a tragedy it would be if "this progressive and forward-looking party . . . were to regress . . . in its stated objectives of achieving an enduring modernization, democracy, and stability" (p. 215). Indeed, "tragic" is not too strong a word to apply to the LKP's policy, especially evident from the spring of 1973, of using violence to try and subdue the Palestinians. This policy, based as it was on a fear that the Palestinians would infect their Lebanese Muslim and Arab nationalist supporters with discontent at traditional Christian domination, became a self-fulfilling prophecy. While the LKP undoubtedly strengthened its position within the Christian political scene by its military prowess in "defending" Lebanese Christian nationalism, its (and the author's) hopes of the Party serving as an instrument for modernizing the Lebanese state and integrating the whole Lebanese polity, including the Muslims and the left, seemed to have been virtually destroyed by the end of 1976. Ironically, the Kata'ib by that time appeared to have become a major force working in behalf not of "static system maintenance" but of carving a "pure" Maronite nationalist enclave out of the old

pluralist Lebanese republic.

While Entelis's thesis emphasizes the liberal and integrative aspects of the LKP, it is to his credit as a scholar that he does not gloss over evidence pointing in another direction. Thus, readers will find material which helps explain the LKP's regression to primordial exclusivism. The chapter on belief system and ideology formation is particularly enlightening in this respect: in it (e.g., on pp. 78–82) we see clearly a deep racial and religious prejudice against Arab Muslims.

Professor Entelis certainly is right in arguing that the Kata'ib has brought structural coherence to the Christian community, and with it more power than the traditional notables and their clienteles could muster against rising class and sectarian opposition to the system. Unfortunately, however, this uneven political development appears to have helped destroy the delicate sectarian proportional representation formula which held the state together since independence. An all-embracing "Christianized" Lebanese nationalism may be just as artificial and hard to develop as the harsh, assimilative pan-Arabism which the author condemns. If preservation of the essential physiognomy of the Lebanese system requires some modernization through improved efficiency and redistributive capacities it also seems to depend on minimizing communal political institutionalization. The only institutionalization that will save Lebanon from disintegration is that which originates from a nonsectarian basis - and the Kata'ib evidently cannot escape its sectarian identity.

Whether or not one accepts Entelis's interpretation of the Kata'ib's development or its functions as part of the Lebanese political system, his arguments are always stimulating because they draw creatively upon modernization and pluralist theory. Furthermore, the core of the book — Part II which deals with the LKP's history, ideology, structure, membership, and electoral-government performance — is very well written and constitutes a significant contribution for students of political parties in the Middle East and elsewhere.

MICHAEL C. HUDSON

Georgetown University

Backward Toward Revolution: The Chinese Revolutionary Party. By Edward Friedman. (Berkeley: University of California Press, 1974. Pp. 237. \$12.75.)

Although its data are drawn from the Chinese case, this book addresses an issue of

general importance in the field of revolutionary politics and social change. As the title suggests, Professor Friedman argues that modern revolutions, and modern revolutionaries, succeed only to the extent that they draw on the past in building a new future. In peasant societies such as China, Friedman believes, progressive ideologies and modern organizations are not necessarily the most effective instruments for enlisting popular support in the countryside. Instead, he stresses the ability of radical elites to develop revolutionary ideals and political organizations which tap the basically conservative and "backward" views of the peasants. In Friedman's view, "mythic return and modern advance" (p. 220) are complementary rather than antagonistic forces for change.

The case example selected by the author for the exploration of this provocative thesis is the Chinese Revolutionary Party (CRP). Founded by Sun Yat-sen after the failure of the Second Revolution of 1913, the CRP represented a retreat from the principles of a Western parliamentary party to an organizational form more appropriate to China's quest for unity against Western imperialism. The "super-Leninist" (p. 212) structure of the CRP was a throwback to China's secret societies in the sense that it projected an image of total commitment and sacrifice which resonated more strongly among China's population than the competition and careerism encouraged by the Western-type parties. More importantly, the CRP initiated a movement "backward toward revolution" through its alliance with "social bandit revolutionaries" (p. 144), such as the White Wolf, who roamed the North China countryside in the 1910s. Though not altogether isolated from national political affairs, the White Wolf drew his major strength from an appeal to the religious needs of the peasantry and their desire for renewing of family and community ties. In the face of "an atomizing war of all against all" (p. 137) in the countryside, the villager recruited into the guerrilla bands of the White Wolf did not "see himself as rebelling," so much as "trying to defend what has been and is rightfully his" (p. 222). In the eyes of the peasants, revolution was "a restoration" (p. 120), "a return to the mythical starting point" (p. 121).

Friedman's discussion of the White Wolf is a major contribution to current reinterpretations regarding the nature of peasant participation in revolutionary political movements. A major premise of his argument — that "people in the countryside who had been acting for centuries on millennial, religious, and magical notions did not suddenly undergo a transvaluation of val-

ues" (p. 131) - is a valid criticism of works which superimpose Western concepts of social change on the convulsions that have affected China and other peasant societies. It is difficult to believe, for instance, that in the absence of full-scale industrialization and mechanization of agriculture, China's peasantry has undergone such fundamental transformations as the elimination of religion and the erosion of belief in the supernatural. Indeed, Friedman's analysis challenges the common assumption that radical change in peasant belief systems is a necessary concomitant of economic progress and mass politicization. The movement to an egalitarian society in China's countryside was not antithetical to peasant folklore traditions and thus did not require the total psychological metamorphosis suggested in William Hinton's Fanshen (New York: Random House, 1966).

Unfortunately, Professor Friedman's sensitivity to peasant psychology is not matched by an equally perceptive analysis of rural social structure. As is so often the case in studies of this sort, the peasantry is treated as an undifferentiated and largely unchanging mass. Friedman does not relate the insightful discussion of peasant religion to rural class relations. Nor does he consider the institutional structure of religion and its relationship to the position of China's landlord/gentry class. Most importantly, Friedman's suggestion that the popular appeal of the White Wolf in the 1910s paralleled the CCP's mobilization of the peasantry in the 1940s neglects significant changes that occurred in north China in the interim thirtyyear period. His hypothesis linking the two movements are interesting but cannot be taken seriously without an analysis of sociological data drawn from village case studies.

Finally, I must criticize Professor Friedman's arguments which, while treating the CRP as an organizational precursor to the CCP, consider the progressive views on party organization articulated by Sung Chiao-jen - the architect of the 1912 Kuomintang - as irrelevant to subsequent Communist theory and practice on the Party. To be sure, Sun Yat-sen's support of a highly disciplined and tightly knit organizational structure presaged Communist efforts at party-building. Yet, several important features of the CRP - most notably, Sun Yat-sen's patriarchical leadership style and the emphasis on brotherhood as a basis of party member relations - were strongly criticized by the Chinese Communists as early as the 1920s. Indeed, the CCP elite have persistently opposed organizational characteristics associated with the secret society and social bandit traditions. On the other hand, Sung Chiao-jen's commitment to a party organization structured around political issues, as opposed to personal cliques, anticipated the Communist effort in the Rectification Campaign (1942–1944) to depersonalize cadre relationships and establish ideological beliefs as the basic associational principle within the CCP. Contrary to the Friedman thesis, the formulation of a viable revolutionary party in China has involved the introduction of organizational structures and practices which were not within the framework of established traditions, and thus required a major transformation of attitudes and behavior. From an organizational perspective, the thrust of revolution was not "backward" but decidedly forward.

In sum, Backward Toward Revolution is a valuable study which should provoke considerable discussion and debate. In light of the current drive in China for the mechanization of agriculture, Friedman's emphasis on the basically conservative orientation of the peasantry deserves serious deliberation.

LAWRENCE SULLIVAN

Wellesley College

Vote, Clivages Socio-Politiques et Developpement Regional en Belgique. By André-Paul Frognier, Vincent E. McHale, and Dennis Paranzino. (Louvain: Vander, 1974. Pp. 149. F.B. 280.)

This book brings together two studies, each in its own way analyzing the relations between social reality and the vote in Belgium. They have also in common, that, starting from a sound theoretical background, they make use of refined mathematical tools, which once more proves to be a fruitful combination.

In the first study Frognier tries with success to create an image of the distances between the political parties as they were perceived by the voters in 1968, in the three Belgian regions (Wallonia, Flanders, and Brussels). The representation of these distances in a multi-dimensional space allows him to identify the dimensions as the cleavages, classic in Belgian sociopolitical life. As a verification, the distances between parties are compared with the flows of votes between these parties, and the results are confirmed. The method is inspired by the studies of Rokeach and of Converse and Valen, but use is made of a specific type of factor analysis: the analysis of correspondences.

McHale and Paranzino pay attention to the impact of regional developmental change on the transformation of the cultural-political opposition in Belgium. Instead of the classic hy-

pothesis of the integrative functions of economic development, they postulate the idea that socioeconomic diversity and unbalanced growth may result in a resurgence of ethnic, regional, linguistic, and other subnational concerns which may threaten continued stability. They show that the growing political importance of linguistic differences and regionallinguistic parties in Belgium in the postwar period is in large part an outgrowth of persisting developmental imbalances between Flanders and Wallonia. Some of their results, however, confirm the hypothesis about the effects of cross-cutting conflicts too; thus, they find that the "Socialist-Catholic" factor, expressing especially socioeconomic conflict in Wallonia and religious conflict in Flanders, is negatively correlated in both regions with the variable expressing regional-linguistic conflict. They established also a relation between blancs and the divided attention of the voters for two cleavages at once. Both these kinds of hypotheses seem to be complementary in the Belgian context, since developmental change has not only increased the political salience of linguistic differences but it also loosened old political ties leading to opportunities for new forms of electoral support.

Both these studies are important contributions to knowledge of the Belgian political system, and they also provide better foundations for comparative research.

A. VAN DEN BRANDE

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Soviet Works on Korea: 1945–1970. By George Ginsburgs. (Los Angeles: University of Southern California, Far Eastern and Russian Research Series, 1974. Pp. 168. \$9.50, cloth; \$6.50, paper.)

This unique bibliography of more than a thousand Russian-language works, with the titles in English translation, provides perspectives and facts often not available in Western monographs. The thoughtful and succinct preface by Professor George Ginsburgs of Rutgers University Law School, Camden, paints a vivid background and categorizes major Societ research on Korea. Professor Ginsburgs, in addition to compiling this comprehensive source, suggests useful research questions, such as: why hasn't North Korea joined Comecon, what is the impact of industrialization, etc.

Professor Ginsburgs charts a close correlation between Soviet political output and changes in the closeness of the Soviet-Korean relationship. The recent increase of publications in broader fields such as history and economics, however, suggests that the number of titles should remain relatively substantial.

This well-written and cohesive secondary source is essential for students of Korea who are able to utilize Russian-language monographs.

ROBERT R. SIMMONS

University of Guelph, Ontario

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Comparative Policy Analysis: The Study of Population Policy Determinants in Developing Countries. Edited by R. Kenneth Godwin. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 333. \$17.00.)

Some twenty years ago the usual stance in demography was that one could predict the consequences of alternative population policies but that, as a neutral scientist, one had no right to advocate any of them. Neither position is widely held today. With the repeated failures of population forecasts to predict at all accurately, responsible demographers are likely to restrict their statements about the future to thoroughly hedged broad generalizations. And during the same two decades, paradoxically, many in the discipline - and outside it - began to offer increasingly precise exhortations to everyone from heads of state to individual parents. This transition to advocacy showed all too often that few in the population field had ever thought about what is meant by "policy" or how one evaluates it. Some ten years ago, when political scientists started to show an interest in population policy, many demographers welcomed their intervention, even though initially it was typically expressed in throat-clearing compilations by generally young men as ignorant of demography as they were unsure about the trend of their own discipline.

This is perhaps the fifth such compilation, and eventually one's good-natured tolerance is strained. The thirteen contributors to the volume comprise a lone professor of sociology (also not a demographer), three government bureaucrats, three private counterparts, and six academic political scientists, most of them at a junior level. To make a book out of offerings from such diverse sources would require strong editorial guidance, but the editor, an assistant professor at Oregon State University, failed to exercise the control that might have made a coherent work out of this almost random agglomeration.

The papers range from original and stimulating to pedestrian and derivative. They are not linked by anything more than the broad topic

stated in the subtitle, and the variation in style and mode of analysis is not mitigated by any visible overall structure. As one sees from the footnotes, this little group continues along its well trod path, citing from one another and the same few key demographers (Judith Blake and Edwin Driver) or administrators (in particular, Berelson). But they have not yet evolved a common base from which to advance, nor even a common language. In the last chapter before an appended bibliography, Bruce Shepard reviews some of the book's points and notes again the two dozen meanings that its contributors, among scholars generally, give to "policy." If the book had ended with no more than a consistent typology of usage, that would have been a minor achievement; instead, most of the authors struggle visibly to define differently – the concept they are supposed to analyze.

The longest paper in the book, by Warren Ilchman, although poorly organized, is stimulating and worth reading. Its discussion exemplifies how an intelligent outsider can benefit any discipline - for example, by seeing practices that have become routine for the stupidities that they are. As one instance, Ilchman has compiled a table showing the principal results of some thirty-four studies to test the thesis that persons of higher education have fewer children on the average (and thus, implicitly, to test also the policy recommendation that to reduce a population's fertility one should raise its educational level). Of the thirty-four researchers, thirty-four neglected to ask questions, or even to speculate, on how the content of the education might have affected fertility behavior. Not surprisingly, American women who attended Catholic colleges several decades ago have had more children than one would anticipate from the general thesis, and the same seems to be the case in the Middle East with men who had an advanced Islamic education. Beyond the primary skills, "education" has a moral content that in its direct and indirect effects can be either antinatalist or pronatalist, or a confusing combination of both.

The best paper, not fortuitously, is also mainly negative — Patricia Anglim's generalizations from her work in Africa. She makes four main criticisms of the typical rationale of population programs: that antinatalist policies are judged by their supposed eventual economic benefits but seldom by their immediate political costs; that the power of the political leaders whom would-be policymakers court is often less than that of the anonymous government bureaucracy; that the simple "technological mastery" of contraception does not

ensure the successful realization of antinatalist policies; and that hopes for such an achievement, even if otherwise realistic enough, typically gloss over the fact that the transition from traditionalism to modernity will last generations. It is a comment on the state of the art that such commonsensical points, when forcefully expressed and aptly illustrated, must be rated as excellent.

WILLIAM PETERSEN

Ohio State University

Race vs. Politics in Guyana: Political Cleavages and Political Mobilization in the 1968 General Elections. By J. E. Greene. (Kingston, Jamaica: Institute of Social and Economic Research, University of the West Indies Press, 1974. Pp. xvii, 198. \$3.50, paper.)

The use of electoral politics for forging an effective national unity in a "new" country wracked by profound ethnic and religious divisions would appear to be almost a contradiction in terms. One would suppose that to the degree that free party competition exists and to the extent that the parties reflect the ethnic cleavages in the society, open rivalry among them would exacerbate disunity and be a cause, or at least a forerunner, of political violence. The grave history of ethnic rioting and violence in Guyana prior to Independence would seem logically to augur such a direction.

Nevertheless, it is a conclusion of this excellent study of the politics of Guyana, based on a systematic examination of the electorate following the 1968 elections in that country. that just such a phenomenon has emerged there. Radical cleavages are deep and lasting in Guyanese society; they are reflected clearly in the identifications which the electorate make of the principal parties, each one corresponding quite closely to distinct ethnic groups in its electoral support; each party openly employs appeals to ethnicity for electoral purposes. Yet the result has not been the disintegration of the national society, nor an increase in communal tensions, nor paralysis at the centers of government. How can this seeming anomaly be explained?

This book suggests, quite convincingly, that the system of party organization developed in Guyana since the time of the mobilization for the 1968 elections is a principal explanatory factor. The system of proportional representation introduced for that and subsequent elections obliged the parties to create more or less permanent organizational cadres in all electoral districts, thereby raising the immediate visibili-

ty of all the parties. The rapid mobilization of the new mass electorate which had taken place for the elections of 1953 under the aegis of the Jagan-Burnham People's Progressive Party had resulted in a highly participant political culture; the subsequent break between Burnham and Jagan and the creation of a two-party alignment in which the ethnic component of the two leaders' respective constituencies became the prominent distinction between them served, apparently, to heighten political awareness and participation. The author suggests that the creation of a third party (United Force, UF) clearly representative of the white (principally Portuguese) merchant and capitalist class and appealing as well to other non-East Indian and non-African ethnic minorities, completed the ethnic "coverage" of this participant culture. The personal styles and ideological idiosyncracies of the two great Guyanese political rivals the pragmatic "smartman" Forbes Burnham and the seemingly ingenuous Cheddi Jagan have apparently contributed as well to a continuing interest and participation in the party process by the population at large.

The racial appeals for electoral purposes are not the result of an ideological line stemming from the party leadership. On the leadership level the parties are quite consciously interracial, and their ideologies are firmly nationalistic and antiracist. Professor Greene shows how the Guyanese parties, as electoral organizations, reflect significantly the racially divided character of the electorate; yet they seem to be providing a kind of political mobilization which balances ethnic pluralism and separateness with effective integration into a national system with sufficient self-confidence to display considerable independence in its domestic and international postures.

In this monograph report on an empirical study of the electorate of 1968, the author is methodical and careful in his presentation of the data and is lucid and professionally cautious in his interpretations and conclusions. As a profile of the attitudes and behaviors of an electorate in a "new" nation, the book is informative, very well organized, methodologically elegant, and cogent. In addition, and for all its brevity and the modesty of its pretensions, the study never loses sight of the larger questions of politics which are posed so dramatically in the new nations of the Caribbean, faced with the challenges of national integration in the context of cultural and ethnic pluralism. The book is a solid contribution to the understanding of the possible role of party and party organization in the working out of these challenges in one particular nation. The

concepts and methods used by the author lend themselves comfortably to the tasks of comparative studies in the Caribbean and elsewhere. It is a welcome additional example of the high standards of responsible and relevant scholarship we have come to expect from the Institute of Social and Economic Research at the University of the West Indies.

ROBERT W. ANDERSON

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Willi Münzenberg: A Political Biography. By Babette Gross. (East Lansing: Michigan State University Press, 1974. Pp. 337. \$12.50.)

An important aspect of certain political movements is the scope they give to individuals who would otherwise be confined to restricted fields of action. This enlargement may proceed along social, cultural, or geographic dimensions; while the individual experiences hitherto unimagined opportunities, the movements gain capable individuals. Revolutionary movements perform this function, at least during their "heroic" periods, and of course Marxism and Leninism have been no exception.

An outstanding but neglected figure who exemplifies this general process at work in the international Communist movement during its Heroic Age (ca. 1915-1935) is the German publicist and politico-cultural impresario, Willi Münzenberg (1889-1940). Born into humble circumstances in Thuringia, poorly and sporadically educated, he had seemingly found his predestined niche when, at the age of fifteen, he was apprenticed to a barber. Yet Münzenberg became, in turn, the leader of the Communist Youth International, inventor of the "Communist Front," master organizer of intellectual fellow-traveling, and an outstanding propagandist, first for the Soviet Union and then against Hitler, particularly at the time of the Reichstag Fire and Trial. This range of activity was possible not only because of Münzenberg's abilities but also because of the existence of an international movement and an organization, the Comintern, and it flourished as the expression of an international outlook. Significantly. Münzenberg's effectiveness declined when the Stalinization of the Comintern deprived him of the ideological and organizational flexibility he required; he had remained autonomous of the KPD apparatus, even though he belonged to its Central Committee.

Münzenberg was the prototypical popular-front participant. While maintaining his own convictions, he found common ground with those of different political opinions. Relatively infrequently did he deceive his collaborators

about the objectives of their actions (more often about the nature of his own financial and organizational resources). It is part of the disappointment of this book that the one flagrant instance of such deception which is described (pp. 220-221) is left unanalyzed.

In historical perspective, Münzenberg's striving for personal and organizational autonomy made him an increasingly anomalous figure. He lost control of youth affairs because he wished them to remain autonomous of the party; his work flourished in the NEP period because it was useful, and it survived the sectarian "third period" because of its organizational resources and loose ties to Moscow. Eventually such independent spirits were fed into the Gulag machine. Münzenberg seemed to have escaped this fate by not returning to Moscow, but died, probably at Soviet instigation, fleeing southward in France in 1940.

His particular genius expressed itself chiefly in two directions. He sensed that the modern "progressive" intelligentsia would respond to someone who did for it the tedious work of mobilizing its good will; and he was a master propagandist, capable of reaching the apolitical masses with modern journalistic techniques and of appealing to intellectuals with book and cinema. Münzenberg's genuine enthusiasm for intellectual products and packaging, his organizational talent, and his internationalist and socialist convictions made him the perfect master of his many-ringed circus.

Fortunately, the fascination of Münzenberg's career comes through in this disappointing book (I did not have access to the 1967 German original; the English-language edition is marred by gaucheries of translation, such as "fire-raisers" for arsonists [p. 248), and editorial lapses, such as misdating the Fourth Cominterm Congress to 1923 [p. 134]).

Babette Gross, who was Münzenberg's companion from 1922 on, although she remains an almost anonymous figure in this book, has unwisely decided to write a "political biography." She would have done better to have written in vivid detail from her unique perspective. For Münzenberg's early life, including his antiwar activities, Swiss period, early meetings with Lenin, and almost all of his youth work, she relies on a few older sources, including Münzenberg's memoirs, but gives neither a brief summary nor a fully rounded picture. It is typical of Gross's lack of depth that despite Münzenberg's complex relationship with his father, she remarks on the occasion of the father's accidental shotgun death that "it did not seem to have made a great impression" on 13-year-old Willi (p. 12).

Even as a close observer and participant, she gives us superficial political summaries, sprinkled with digressions (such as the description of a Comintern spy school, with which Münzenberg seems to have had no connection, on pp. 264-265). What were Münzenberg's views of the important German and Soviet officials he knew? How did he view his causes, the intellectuals he managed, the campaigns he organized? How did Münzenberg, who was not an intellectual, and spoke no language other than his native German, get along with Rolland, Einstein, and the rest? Is Gross right that after 1933 Münzenberg's work shifted from pro-Soviet to Anti-Hitler? To these and many other questions, this book gives no answers.

Nevertheless, by virtue of the unique materials which Gross does provide, her life of Willi Münzenberg makes valuable reading for anyone interested in the early Communist movement or (as with her account of the Berlin premier of *Potemkin*) European political-intellectual history during World War I and the two decades following.

HENRY KRISCH

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Military Regimes in Africa. By William F. Gutteridge. (London: Methuen & Co., Ltd., 1975. Distributed by Barnes & Noble, New York. Pp. 195. \$12.75, cloth; \$7.00, paper.)

William F. Gutteridge is the doyen of African military studies. In the early 1960s, when only a few scholars were concerned with the military in the Third World, Gutteridge published his pioneering Armed Forces in New States (London: Oxford University Press, 1962) and Military Institutions and Power in the New States (New York: Praeger, 1965). These general works were followed by a more specialized study of The Military in African Politics (London: Methuen, 1969), and now by Military Regimes in Africa. In addition to these major works, Gutteridge has written numerous papers, articles, and short monographs on related subjects.

Military Regimes in Africa is really an updated and revised edition of The Military in African Politics. Whereas this earlier volume was concerned primarily with the origins and causes of military intervention, the present work evaluates the character and conduct of African military regimes. As Gutteridge explains in his preface, the purpose of this book was "to write a sequel" to The Military in African Politics which would "take up the threads of the first work and at the same time

concern itself more with performance in power than with the seizure of it." It is altogether fitting that Gutteridge should now address the behavior of military regimes. In the first place, a continued preoccupation with the causes of military coups in Africa would have added little to what is already known. And so the author's introductory chapter merely adapts - without specific attribution - Claude Welch's general classification of eight factors that have contributed toward African coups. Second, the proliferation of military regimes in Africa during the last decade provides ample cases and time-depth for a retrospective analysis. Finally, the shifting of research interest from causes to consequences is consonant with the general state of the art in contemporary African military studies.

The heart of the volume consists of six case studies of military rule in Ghana, Dahomey (now Benin), Nigeria, Zaire, Uganda, and the Sudan. In each case Gutteridge sketches the basic features and trends of national and international politics. Rejecting "elaborately structured analysis" and "essentially meretricious" categorizations, the author prefers to stress the uniqueness and novelty of each case. The result is an interesting collection of brief historical essays which unfortunately contributes little to our understanding of the performance of African military regimes. Many of the biases and imbalances of Gutteridge's earlier work have been carried over into this volume. Nigeria and Ghana, which together claimed 66 of the 102 pages of case-studies in The Military in African Politics, continue to dominate Military Regimes in Africa. In fact, they both occupy the exact number of pages in this new volume, thirty and thirty-six pages respectively, and far outweigh the forty-nine pages allocated to the other four cases. There is no apparent justification for this imbalance; indeed, the duration of military rule in Dahomey (Benin), Zaire, and the Sudan constitutes a prima facie case for comparable attention. The distribution of attention among the six cases was obviously determined more by the author's personal inclinations and infatuations than by their intrinsic interest.

A more serious objection can be raised regarding the absence of any admissible criteria for the selection of cases. Gutteridge admits in the preface that each of his six cases was selected as "significant, if only because it tended to emphasize the unique nature of the particular situation." Presumably then, each case simply cumulates novelty but yields no comparative perspective. This methodological dilemma notwithstanding, Gutteridge does

manage to draw some conclusions from the "unique" particulars. These conclusions, however, amount to no more than a distillation of the conventional wisdom that (1) the performance of African military regimes has not been very different from civilian governments; (2) military regimes are essentially alliances between the ruling officers and the civil service or bureaucracy; (3) civilian-military distinctions are blurred because the army and bureaucracy are conservative, order-loving elites who treasure their mutual self-interest above all else; and (4) military regimes have tended to succumb to the intractable problems of governing new states rather than master these conditions.

Military Regimes in Africa therefore is more a synthesis than a singular contribution. It reflects, reinforces, and recapitulates current views instead of reassessing them. Like all other general studies of the political role of the military in Africa, this volume is based on the case-study approach, but the absence of a comparative analytical framework is especially deplorable. The high cost of such a slender volume of such prosaic rendition will only irritate the reader who expects more from William Gutteridge.

JOSEPH P. SMALDONE

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Struggle in the Andes: Peasant Political Mobilization in Peru. By Howard Handelman. (Austin: Institute of Latin American Studies, The University of Texas at Austin, 1975. Pp. xvii, 303. \$10.00.)

In his introduction to the study, Howard Handelman, Assistant Professor of Political Science at the University of Wisconsin-Milwaukee, informs us that his purpose "was to study the role of the village campesinos in the national political system," and to determine "how and under what circumstances the peasantry may become an effective participant in Peruvian politics." On page 46 (at the bottom of footnote 24) he indicates that "A major objective of this study will be to probe the attitudes of sierra villagers to ascertain whether or not they are inclined toward cooperation and to see how such attitudes might affect their potential for political mobilization." In spite of a number of months diligently spent collecting data on the views of community leaders and surveying the existing literature and periodical sources, I am forced to conclude that the author has failed to achieve any of his objectives. Let me go into some detail.

The first part of the book (chapters I to 6) covers the historical background of the period that Professor Handelman had chosen: The invasions of large landholdings by peasants employed by them (in various capacities) and/ or living in nearby communities, which occurred in Peru between 1960 and 1964. It is important to keep this period in mind when one analyzes the second part of the book. These first six chapters, while presenting a competent survey of the literature already available on the subject, fail to provide any information not previously published. To be fair, the author scrupulously lists his sources and gives credit to those who preceded him in the discussion of peasant movements in Peru. But obviously, honesty alone does not provide the raw material of which scholarly books are made. Furthermore, his survey of the existing literature, while competent, is not comprehensive enough. Professor Handelman deals at length in chapters five and six with two individuals who played an active role in the events he describes. Yet, he makes no reference to their writings, such as Genaro Ledesma Izquieta's Complot, which deals with some of the same episodes covered in the book, and Jesus Veliz Lizarraga's El Peru en la Cultura Occidental and Principios Fundamentales de Aprismo. Finally, and perhaps more importantly, the author fails in this first part to provide a comprehensive vision of the Peruvian political system in the period under study, the type of vision that the nonspecialist reader needs in order to place the specific subjects of Professor Handelman's study in the systemwide context.

The second part of the book - chapters 7 to 9, as well as appendices I and II - contains the presentation and analysis of the field data collected by the author. It consists of the information obtained by interviewing the leaders of forty-one peasant villages in the central and southern highlands of Peru; the dwellers of twenty-one of these villages had participated in land invasions during the period. under study, and the inhabitants of the other twenty had not. The author tells us that the universe might total 6,000 such villages. With the help of university students from the areas being surveyed, the author interviewed the leadership group at joint meetings, often attended by other dwellers. He sought information on level of socioeconomic development, contacts with the outside world, intravillage solidarity, political mobilization, and political attitudes. Since it is apparent, at least to me, that the second part of the book is supposed to be the author's original contribution, the data

ought to be analyzed at length and the "fine print" read carefully.

As with the first part of his study, Professor Handelman honestly discusses some of the weaknesses of his survey. He points out that he is interviewing village leaders of 1969 to investigate events that had occurred in 1960-64, although many of these leaders had not led during the period under study. Having dismissed the possibility of surveying a representative sample of village dwellers, the author arbitrarily assumes that the views of the leaders coincide with those of the villagers, both in 1969 and in 1960-64. By employing joint (even mass) interviews, he made it difficult for individual leaders to express dissenting opinions, thus converting villages into homogeneous units. The survey was conducted in some villages before and in others after the announcement of a new agrarian reform program by the military regime, a fact that undoubtedly affected the village leaders' perception of the political system. Finally, the author tells us (p. 268) that the questions dealing with political orientations, internal solidarity, and political efficacy were open-ended and "thus, the criteria for assigning a particular response to a rank were necessarily subjective." The assignment was handled by him and his two assistants who, apparently, were not the same individuals throughout the study. Thus the reader is told in Appendix A that the core of the "objective" data is in fact highly "subjective."

The statistical handling of the already questionable set of tables also deserves some comments. Assuming that the views expressed by village leaders were uniformly shared by their constituents and ignoring all the other difficulties listed above, we must note, nevertheless, that the author's measurement of the strength of the relationships is based on small sample sizes (at most forty-one cases). When samples of this magnitude are utilized, the appropriate measures of statistical significaNce should accompany the values of tau-c provided in the text. In the absence of any discussion of statistical significance I am unable to place much reliance on Professor Handelman's conclusions, which are based on tau-c's varying in magnitude from .063 to .409 and averaging .264. (I gratefully acknowledge the advice of my colleague Linda Powell on this subject.)

When one combines the inadequate presentation of the data with the weaknesses listed above, the inescapable conclusion is that Professor Handelman's evidence is extremely shaky and does not provide his opinions and observations with the backing that he appears to take for granted. I do not believe, for instance, that

on the basis of this data, he has successfully disproved the theories advanced by Samuel P. Huntington and others regarding the political involvement of the rural lower class. Nor has he proven the views he advances in the second part of the book, and particularly in chapters 8 and 9 regarding the mobilization of the Peruvian peasantry and its relationship to the entire polity.

The book also includes an epilogue which attempts to update the discussion of the political role of the peasantry in terms of the measures taken by the military government that took power in October, 1968. It discusses the characteristics and possible consequences of the agrarian reform program decreed by the military leadership in June, 1969. Unfortunately, since the manuscript was completed in 1972 and has not been further updated (although it was published in 1975), most of the facts presented in chapter 10 have already been published and in some cases superseded by more recent events. So have many of the comments presented by the author regarding the military government and its policies toward the rural areas.

Intellectually, one issue that appears throughout the book deserves some comment. Professor Handelman keeps raising the question of legitimacy of peasants' land invasions and other political actions. Whether the author recognizes it or not, he leaves the reader with the impression that peasants' political moves enjoy a high level of justified legitimacy. To put it quite simply, the peasants are the "good guys" in the Peruvian struggle for power. I'd like to suggest that the peasants are motivated by the same factors that moved mestizos and whites, both now and throughout history. Their claim to land and to a share of political power is based on as weak (or as strong) grounds and claims as those held by white and mestizo landowners, military officers, bureaucrats, and industrialists. Property relationships have been altered more than once in many parts of the world; what evidence is really there to claim that ownership rights of Indians or peasants are more legitimate than those of the colonizers, particularly when one is familiar with the precolonial history? The conception of legitimacy hinted at by Professor Handelman should have been explicitly discussed, since it affects not only some of the authors's hypotheses but his perception of events as well.

Finally, at a different level, I want to question once more the unfortunate habit of writing comparative politics in a mixture of languages. The author feels compelled to use Spanish words and phrases on nearly every page, as if they had a meaning of their own which can not be conveyed by their English equivalents. Next to them, to prove that this is not the case, he gives us the translation, thus unnecessarily increasing the length, and cost, of the book. The volume is also lengthened by the needless repetition of facts and thoughts; examples can be found in information provided on footnote 28, p. 131, and repeated on p. 135, as well as with the facts described on footnote 54, p. 242, and repeated on footnote 17, p. 251.

I am regretfully forced to conclude that Professor Handelman's book confronts us with the work of an honest observer who contributes virtually nothing to his field.

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Divided Nations in a Divided World. Edited by Gregory Henderson, Richard Ned Lebow and John S. Stoessinger. (New York: David McKay Co., 1974. Pp. 470. \$12.50, cloth; \$5.95, paper.)

A world that is divided into innumerable territorial entities called nation-states, each claiming an exclusive right over a domain, is a fact of international life with which students of world politics must begin. The book under review is a path-breaking enterprise which initiates a systematic investigation of the phenomenon of political fragmentation and reintegration of nation-states in a divided world. The book is grouped into two parts and eleven case studies of six "divided nations" including Germany (John H. Herz), Korea (Gregory Henderson), China (Nathaniel Thayer), Vietnam (Le Thi Tuyet), Cambodia and Laos (Bernard K. Gordon) and Mongolia (Harrison E. Salisbury) as well as of five "partitioned countries" including Ireland (Richard Ned Lebow), India and Pakistan (Craig Baxter), India, Pakistan, and Bangladesh (Rounag Johan), Ruanda-Urundi (Warren Weinstein), and Palestine-Eretz Israel (Gidon Gott-lieb). "Divided nations," according to the editors, are countries that have been arbitrarily separated into two or more political units from the outside and "partitioned countries" are the case of political fragmentation that results from internal causes of ethnic, linguistic, or religious conflict among the subgroups in the political

A very brief, almost too cursory, introduction is provided by one of the editors, John Stoessinger, in the capacity as director of the seminar sponsored by Ralph Bunche Institute on the United Nations of the City University of

New York, for which most of the papers contained in the volume were written. The concluding chapter of the book, written by the remaining two editors (Henderson and Lebow), provides a useful and stimulating theoretical overview of the book which is intended more to raise questions and hypotheses than to provide answers. Henderson and Lebow suggest, for instance, that the "relations between divided nations change as a function of (a) the degree of stability and legitimacy of each divided state; (b) relations between each divided state and its respective superpower; and (c) relations between the superpowers themselves" (pp. 438–439).

Henderson and Lebow break down the process of political fragmentation and reintegration of the nation-states into four separate stages. These are (1) initial division, characterized by intense hostility between divided units; (2) middle-term division, noted for declining hostility between units; (3) rapprochement, marked by the creation of intergovernmental linkages and of formal consultative machinery; and (4) unification, resulting in a kind of loose federation initially and involving free movement of persons, extensive economic cooperation and some symbolic gesture like adoption of a common flag and national anthem. In light of the subsequent development in Vietnam, which led to a forceful takeover of the south by the north, the paradigm of political fragmentation and reintegration suggested in the books seems limited to its validity and usefulness. Other less sweeping as generalizations are more convincing and seem accurate as hypotheses. For instance, Henderson and Lebow argue that the divided nations "are likely to possess greater freedom of action and seek improved relations with each other," as they "develop internal strength" and as the level of "hostility between their respective superpower backers decreases" (p. 439).

The book contains useful data on each country and on all twelve cases under review. In an attempt to explain the degree of "hostilization" between the component units, an interesting set of data are displayed on the background characteristics of these countries. These background variables, fourteen of them in all, include such common attributes of a country as the length of historical experience, degree of economic development, the population and resource distribution, as well as some unusual attributes like the regime stability and ideology, proximity to great powers as neighbors, ethniclinguistic-religious homogeneity, the number of refugees. Unfortunately, the measurement of these interesting variables is based on a subjective criterion, rather than on an objective standard. Some actual statistics, such as percentage of the dominant group in the population as a measure of ethnic homogeneity, for instance, would have been far more useful and valid than the judgmental scoring of ethnicity based on a scale with a range of \pm 5.

The case-study materials contained in the book are generally historical and descriptive, geared to the need of those outside the area specialization. The book is by no means a theoretical breakthrough in the study of the process of political fragmentation and reintegration of the nation-states in the international system. The concluding chapter, in my opinion, should have been placed at the beginning to provide an overarching framework for the remaining chapters of case studies to follow. As a result the book suffers from the lack of theory-guided perspective that could have tied the material together. In fact, the style, focus, coverage, and quality of the essays vary and the accuracy as well as the value of some of the case studies may be questioned. It is not clear, for instance, why the countries of Cambodia and Laos are lumped together as divided nations and why Pakistan-Bangladesh and India are partitioned countries.

The volume nevertheless represents a fascinating story of the making, the breaking, and the remaking of political entities in the world the landscape of which continues to change. The study makes one aware of the importance of an act of "bounding" territory — whether in the developed countries or in the newly emerging countries, often with an arbitrary drawing of the frontiers following the departure of the former colonial powers of the West.

YOUNG WHAN KIHL

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Chinese Historiography on the Revolution of 1911. By Winston Hsieh. (Stanford, Ca.: Hoover Institution Press, 1975. Pp. 165. \$9.00.)

This valuable monograph, composed of an illuminating historiographical essay and an extremely useful bibliography, is of much greater interest to students of modern China than its highly specialized topic might suggest. One cannot arrive at an adequate understanding of political development in modern China without first coming to grips with the revolutionary movement which led to the 1911 Revolution and beyond. There are still relatively few Western scholarly examinations of this period, and, as Professor Hsieh demonstrates master-

fully, one must approach the Chinese language studies with caution. Interpretations of the revolution by both Chinese Nationalist and Communist historians (whom Hsieh labels Orthodox and Neo-Orthodox respectively), on the whole faithfully follow the changing political and intellectual needs of the ruling elites. The first school magnifies the role of Sun Yat-sen and his associates while the other seeks to compress history into Marxist class categories, although valuable compendia, documentary collections, and monographs have resulted from their labors. Both schools fit squarely into the grand tradition of Chinese historiography which valued intellectual orthodoxy above all else and viewed scholarship as an ideological tool.

Professor Hsieh brings to his analysis the cool iconoclasm which is the hallmark of the historiographer's craft. Despite the shortcomings of earlier studies of the revolution, enough of the picture has emerged over time to enable contemporary scholars to pose the questions which their Chinese predecessors were inhibited from exploring. Here the monograph makes its greatest contribution. Professor Hsieh offers us a rich menu of suggestions for research topics, including the development of ideology and political institutions, local politics, the relationship between the Manchu reforms and the revolution, the interaction between traditional and modern modes of rebellion in China and much more. The broadminded student of post-1949 Chinese politics cannot fail to recognize the family resemblance between many of these topics and his own concerns. Political scientists specializing in China, then, should view this book as an open invitation to join historians like Professor Hsieh in a continuing effort to deepen our understanding of this seminal period in modern Chinese history.

STEVEN I. LEVINE

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Reform and Revolution in Asia. Edited by G. F. Hudson. (New York: St. Martin's Press, 1973. Pp. 310. \$16.95.)

The eight essays of this collection concern Asia, but as a group they do not focus upon "reform and revolution." They are, rather, "special studies" of "aspects of the Asian political scene" (pp. 9, 24).

This work has nine parts. The first are an editor's introduction and a chapter on prerequisites to Asian modernization (the latter by Werner Klatt). There follow two essays on China (by W. A. C. Adie and by Ralf Bonwit),

and one each on Japan (Richard Storry), India (W. F. Crawley), Pakistan (H. Khuhro), and Indonesia (L. H. Palmier). It concludes with an essay by the editor on Asia in the 'seventies.

Klatt's topic raises immediate problems, for it assumes that modernization is a rather concrete goal and that progress toward that goal can be assessed. The terms are familiar: industrialization, economic development, gross domestic product. To Klatt's credit he is sensitive to issues of land and agriculture, asserts that politics deserves the "commanding position" in change (p. 53), and recognizes the risks of foreign advice (p. 30). He has difficulty assimilating China to the text, writing as if about Asia ("foreign contribution to the investment programme ... is invariably essential") and then acknowledging that China has had little foreign assistance (and hence a "much reduced rate of growth" [pp. 36-37]). Klatt repeats the conventional but distorting assertion that China spends 10 per cent of her annual income on defense (p. 35) and judges that Chinese agricultural planning "has been faulty in the extreme" (p. 52). Klatt's commitment is to reform rather than revolution, but through "well-timed economic, social and political 'inputs' " to overcome "tensions"; enriching the few by merely raising the economic growth rate, he asserts, can have a "palliative effect" but "cannot avert ultimate disaster" (p. 56). Thus new policies must be sought, but not in the Chinese experience: "China's prerequisites for modernization are unique, and thus make its political, social and economic pattern unsuitable elsewhere in Asia" (p. 52).

Adie's essay traces Chinese arguments on education from 1949 to 1970. His theme is the contest between full-time study ("less but finer," elitist, professional, specialist, Liuist) and half-work half-study ("more but worse," political, generalist, Maoist). The topic is important, and Adie asserts his argument deftly. He is drawn into an unproductive excursion, however, as he takes up the alleged "special quality in Chinese society" of repressed aggression against figures of authority, and likens themes in "the propaganda of Maoism" to Nazi themes during the Third Reich (p. 63).

Adie's assessment turns on one perception and one predisposition: He sees the role of China's army, from 1964, to have been increasingly influential and even controlling ("usurped the powers of the constitutional Party Committees," "ultimate control rested with Army men"), though with the final — and contradictory? — insight that "the real effect of the guiding role given to PLA is to disperse and demilitarize it" (pp. 95–96).

His predisposition is to modernization and specialization. China may devise "further solutions" to the conflict between egalitarianism and specialization, but this juncture witnesses "the inevitable restoration of power to regular teaching staff and to managerial bureaucracy" (pp. 94, 96).

Adie's viewpoint is best captured by his closing remarks on the army's role as model, an orderly horde:

The values transmitted to the younger generation are meant to be those of a group whose development was arrested at the stage of adolescent revolt, idealism and fantasies of assimilating the world, as described by ... Jean Piaget.

... Piaget's remarks on ... a mode of thought proceeding from the concrete to the concrete without elaboration of abstract ideas ... call to mind many passages of Mao Tse-tung's theoretical writings, while his description of the intellectual egocentricity of adolescence... not only fits the mentality of Red Guards as revealed in their publications, but may be extended by analogy to the collective mind of emergent nationalist states (p. 96).

Bonwit — "Mao's Continuing Revolution" — is the most useful essay in the volume, an exegesis and interpretation of texts on central themes. Written in July, 1970, it asserts that PLA control is decisive and that "outside the PLA, there appears to be no properly constituted Party authority below Central Committee level" (p. 142). Thus it can be read not only as a carefully argued example of one school of analysis, but also as an illustration of the dangers in interpretation of a fluid and inaccessible situation.

Storry's brief, inconclusive entr'acte on the Mishima affair locates it politically, with grace and effect

The next three essays survey the post-World War II political histories of India, Pakistan, and Indonesia. Leaders, parties, and interest groups march in the quest for "control." Each essay, succinct and felicitously worded, is an exercise in compression. On the other hand, the authors have little scope for argument. Social and economic facts, if not reducible to shorthand. are never explored. Miss Khuhro, for example, loses an important opportunity to consider underlying change which preceded the December 1970 victory of Z. A. Bhutto in West Pakistan. And Mr. Palmier tells us that the "sober economic plans" of the Suharto government called for encouragement of "foreign investment that the country desperately needs" (p. 263) with no intimation of the issues of source, quality, consequence, and terms which that prescription necessarily implies.

G. F. Hudson concludes that the three main types of political system in Asia are democracy, communism, and military dictatorship, and that "there is no government in the world which is quite secure against communist takeover" (p. 274). And he adds that "if Asians want political democracy as a condition of development, they can look to India for a model; if their concern is with advanced technology and high standards of living they can look to Japan; in China they can only find utopian ideology and a still backward economy which painfully drags itself upward with 'politics in command'..." (p. 299). Of course, this was written in November 1971.

Romanization of Chinese names is inconsistent; Hudson repeatedly refers to Soga Gakkai [sic]; and the index is a shamble of errors, both of spelling and page reference. This is the American version of Publication No. 7 of St. Antony's College, Oxford, published in 1972 by George Allen and Unwin under the same title, but appears simply to reproduce the errors of the original.

BRUCE D. LARKIN

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The Politics of Massacre: Political Processes in South Vietnam, By Charles A. Joiner. (Philadelphia: Temple University Press, 1974. Pp. 346. \$12.50.)

Charles Joiner of Temple University claims his book is highly critical of every party to the war in Vietnam. In view of Joiner's early association with Vietnam through the Michigan State University Advisory Group and his subsequent diligence in studying this country, such a declaration of critical intent deserves attention. Unfortunately, Joiner's critical detachment is sometimes more a posture than a method and does not compensate for his lack of a comprehensive, consistent perspective.

Part of the problem is that the book consists mainly of articles on loosely related or overlapping subjects published between 1963 and 1970 and reprinted evidently without revision. Three chapters or sections of chapters were coauthored with Guy Fox, John Donnell, or Roy Jumper. Not surprisingly, the book has a problem of continuity and lacks a steady focus. The impact of the author's criticism is further vitiated by his clear preference for non-Communist government in Vietnam. This preference does not necessarily require support for any of the non-Communist regimes that

have collapsed in South Vietnam, nor is it inherently incompatible with critical analysis, but in this instance it leads to sympathetic "balanced" criticism of the Second Republic and unalloyed aversion to the Communists.

The principle theme is that the internal political process in South Vietnam always has prevailed over the external forces which have sought to shape it. Hence, it is futile to "explain" Vietnamese politics in terms of external forces, and the proper subject of study is the process by which Vietnamese have sought to re-establish legitimate institutions. This is hardly a startling argument, but it bears repeating as a corrective to the simplistic image of Vietnam as a passive arena of Great-Power politics. For Joiner, however, the "internal political process" is mainly the politics of the now-defunct Republic. This may be the natural consequence of his experience and sources, but it leads to a surrealistic picture nonetheless. The fortunes of non-Communist political groups are traced in tortuous detail, as are the elections of 1967, while the revolution remains little more than background noise. A brief section on "The Organization Theory of Revolutionary Warfare" - actually an uncritical "critique" (Joiner's word) of Douglas Pike's Viet Cong and assertions that the insurgents were in some way part of the process, do not adequately compensate. Pike's "organization theory" (which inspires Joiner, in a lapse of analytical detachment, to recommend that Saigon turn the "organization weapon" on the insurgents) merely prevents him from seeing how Communist effectiveness was contingent upon inequities that Saigon elites sought to preserve, as Jeffrey Race convincingly demonstrated in War Comes to Long An. The view of both Saigon politics and revolutionary politics, and of the intersections between them, is from the limited vantage point of Saigon.

Heterogeneity is held to have been the principal cause of political fragmentation, disunity and powerlessness of the Republic. Joiner provides a great deal of useful detail in support of this view. Certainly, the reliance of Saigon elites on particularistic bases of support was a major factor keeping the Republic's politics in a state of petty, debilitating contention. But Joiner does not successfully explain why the PRG succeeded where the RVN failed. To argue, as Joiner does, that the NLF and PRG were held together by indoctrination and organizational complexity, or that "Northerners" imposed discipline on unruly "Southerners," whereas the Republic did not do these things or did them less well, only begs further questions. Very different conclusions might have been reached had Joiner appreciated that Saigon elites represented narrow interests within their claimed constituencies and had he not ignored the importance of class cleavages.

One of the curious consequences of his insistence upon the primacy of heterogeneity is to portray all Vietnamese political movements as equally plagued by it. This is valid in the sense that they all had to operate in the same environment, but not in the sense that they were equally unable to do anything about it. Joiner's vision of Vietnamese society overlooks the strong continuities which overlay the heterogeneity and cannot come to terms with the fact that of all modern Vietnamese political movements, the Communist Party was the most successful in strengthening these continuities by establishing institutions on the presumption of regional, sectarian, sexual, and ethnic equality. The assertion that "perhaps nothing is truly 'national' about Vietnamese politics" (p. 98) and the labeling of the evanescent cliques and factions that orbited about Saigon from 1954 to 1975 the "nationalists," in addition to being contradictory, betrays a shopworn, ideologically inspired ignorance of what Vietnamese communism is and what it has achieved. Yet in fairness it should be said that knowledgeable students of Vietnam, reading with circumspection, may find in this book some useful insights and information on the politics of the non-Communist experiment.

WILLIAM S. TURLEY

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The Pattern of Sino-American Crises: Political-Military Interactions in the 1950s. By J. H. Kalicki. (New York: Cambridge University Press, 1975. Pp. 279. \$17.95.)

Dr. Kalicki's study of Sino-American crisis interactions attempts to contribute to the quest for nomothetic statements about international crises by moving in the direction of comparative inquiry for the purpose of generating hypotheses and theory about the general pattern of crisis behavior. This commendable analytic goal is pursued by what is termed a "comparative case study" approach focusing on the single Sino-American dyad over time, rather than comparing crisis-prone dyads comprised of varied participants. Given the prominence of the Korean conflict, the two Indochina episodes, and the two Taiwan Straits confrontations in any conventional inventory of international crises, this happy choice of cases should have facilitated the search for behavioral regularities while permitting the analyst to hold

constant a number of potentially contaminating factors intrinsic to a cross-sectional design. Unfortunately, however, the author fails to develop an explicit framework for comparison across cases, so that what emerges is a series of "historical narratives" detailing the peculiarities of each successive crisis; different variables are observed in each case, with the result that a rich contextual account of the unique characteristics of each crisis is provided, but the reader must sort this mass of detailed information and read between the lines in order to discern commonalities in the record of interactions. Had the narrative histories been probed in a systematic manner with a nonelastic category system (by jettisoning juxtaposition in favor of controlled comparison), perhaps the conclusions would not have appeared so theoretically weak, descriptively trivial, and often tautological. For example, to learn that communications problems are aggravated by the nature of the channels (p. 72) and that "the exercise of ambiguity ... can result in increased confusion over intentions" (p. 86) is not to acquire wildly exciting information or perceptive insight into crisis phenomena.

More useful, perhaps, are the conclusions which emerged from Dr. Kalicki's cumulative diachronic perusal of the record of Sino-American interactions throughout the entire period. The author's unfortunately brief (10 page) examination of the total chronological sequence of interactions in the 1950s suggests rather convincingly the extent to which past dyadic exchanges served as a prologue to future interactions. That is, each crisis set precedents for future crisis behavior and each party to the dyad developed through experience a patterned response-set or institutionalized behavior repertoire which, the author contends, enabled the two superpowers to manage each successive disruption more effectively by anticipating the other's behavior. The primary thesis stemming from this observation is that the crisis behavior of Chinese and American decision makers became increasingly sophisticated over time; as a result of shared experience in dealing with each other in crisis situations, both sides learned to become more flexible, creative, and capable of crisis management. While some readers may regard the conclusion that the "learning curve of US-PRC crisis handling" grew at an exponentially increasing rate as hyperbole (since most learning grows at a decreasing rate, resembling a diminishing returns curve), the evidence that internation learning occurs under conditions of chronic crisis constitutes a valuable insight and promising hypothesis for future empirical research. A set of corollary hypotheses also deserving of further research may be derived as well: that crisis conditions tend to promote the clarification of national goals and interests, and that rather than increasing the probability of war, crises may stimulate compromise and agreement between conflicting parties.

This book is likely to elicit differing reactions from readers, depending on their methodological and epistemological predilection. Diplomatic historians will probably find the presentation balanced; the interpretation of events is an orthodox one, but the contribution of revisionist works is occasionally acknowledged. Some readers will undoubtedly be disturbed by the book's tendency to employ reified concepts and teleological reasoning, by the occasional use of normative adjectives for descriptive purposes, by the alacrity with which motivations and intentions are attributed to actors on the basis of mere assertion, or by the tendency to vacillate without warning between treatments of crises as independent and as dependent variables. Students of comparative foreign policy should appreciate the sensitive treatment given to the linkage between domestic and international politics, but will be inclined to look askance at the way in which variations in systemic and idiosyncratic factors are underplayed. Theoretically inclined readers in search of empirically verifiable hypotheses are certain to find this book unrewarding when compared with the methodologically sophisticated and encyclopedic coverage provided by Charles F. Hermann's International Crises: Insights from Behavioral Research (New York: Free Press, 1972). And traditionally minded students of contemporary international affairs will be distressed to discover that an assessment of the legacy of Sino-American interactions in the 1950s for current relations between the two states is not made. Nevertheless, despite some apparent deficiencies, The Pattern of Sino-American Crises, which is based on the author's doctoral dissertation at the University of London, addresses a complex subject in a highly readable manner. One would hope that Dr. Kalicki will pursue some of the promising ideas expressed in the present study in subsequent work.

CHARLES W. KEGLEY, JR.

University of South Carolina

Revolutionary Transformation in the Arab World: Habash and his Comrades from Nationalism to Marxism. By Walid Kazziha. (New York: St. Martin's Press, 1975. Pp. 118. \$8.95.) The Arab Nationalist Movement has not received much attention in the West, largely because it has been overshadowed by political movements such as Nasserism and parties such as the Baath. Nevertheless, the ANM, as it is abbreviated, is worthy of notice. One regime in the Arab world, that of South Yemen, grew out of the ANM, and two important, albeit numerically small, factions of the Palestinian movement, the PFLP and the PDFLP, can trace their origins to the ANM.

Walid Kazziha, presently a professor of political science at the American University of Cairo, brings to his subject one invaluable asset: for eight years, he was an active member of the ANM. His book is not, however, a personal history, nor is it a theoretical or ideological tract. Instead, Kazziha sets himself the modest goal of describing the evolution of the ANM as a political phenomenon in the Arab world.

He tells us that starting from an early preoccupation with the loss of Palestine, the ANM gradually evolved toward Nasser's vision of Arab nationalism and unity, to the point where the ANM nearly lost its separate identity. In 1964, however, the ANM took a new turn, this time to the left, as a younger generation of party militants began to press for the inclusion of socialist concepts in the ANM ideology. Here the author is able to draw on first-hand information to clarify the nature of the debate that eventually split the ANM into more and less radical factions.

The author also sheds light on the organizational structure of the ANM, describing its evolution from a cell-based movement in its early years to a looser structure by the 1960s. As the ANM spread to various Arab countries, it adapted itself to local conditions. The author provides useful descriptions of the accommodations made in Jordan with some of the traditional political forces, and in the Gulf states, where the ANM remains an important political force today, especially among the large expatriate Palestinian community.

Kazziha has done well what he set out to accomplish. The book does not claim to provide a general theory of political movements, nor does it rely on concepts derived from the social sciences. The result, apart from the pretentious and misleading title, is a modest and useful case study, particularly welcome because of the dearth of books on the topic and the moderate, nonpolemical tone adopted by the author.

WILLIAM B. QUANDT

University of Pennsylvania

The Military in the Third World. By Gavin Kennedy. (New York: Charles Scribner's Sons, 1975. Pp. 368. \$17.50.)

This book opens a new set of questions relevant to the study of the military in the Third World. The now voluminous literature in this area began with often normative and relatively data-free analyses of the reasons for increased intrusion of the military into the politics of developing states. In the intervening years we have seen a greater interest in the role of the armed forces after their attainment of civil power and the employment of more quantitative data and methods. Professor Gavin Kennedy also analyzes military intervention, utilizing case studies from the Middle East. Africa and South-Southeast Asia, but in addition he brings to the reader's attention a number of less-discussed issues.

The most interesting issues include the context of violence in which the military operates, the reasons for the nonintervention of the armed forces, and the economic impact of military expenditures. The first chapters assess violence against patterns of ideology and political structure in the Third World. After noting the prevalence of violence in the area (he concludes the book by stating that violence is endemic in the world and "the normal means of settling human conflict" [p. 335]), the author argues that the intervention of the military is a consequence of violence rather than the cause of it. Another chapter reviews the fascinating question of why the military has not intervened or been depoliticized in what are now a minority of Third World countries, presenting for observation the examples of Turkey, Israel, and Mexico. Given the virtual ubiquitousness of military interference in civil government in the Third World, this question is as crucial as the earlier queries about why coups took place. Noting the importance of strong, charismatic, and legalistic leaders in the three case studies, the author goes on to argue that: "Where civilian governments fall and the military succeeds this expresses the crisis of civilian government and not the inherent pathology of the soldier" (p. 154).

The last half of the book is an overview of subjects too little discussed in the literature, i.e., the politics of defense budgets; the relation of defense expenditures to growth, welfare, and production; arms races and production and their relationship both to the economics of the developing world and to the issue of disarmament in the region. Numerous charts are presented on expenditures, types of military equipment, economic and social data on individual

countries, suppliers and production of armaments, etc. In his analysis the author seeks to correlate economic and social data with military expenditures. While cautioning the reader about the unreliability of the data, Kennedy is not prepared to argue that military expenditure and production have a necessarily deleterious impact upon development, welfare, and industrialization in Third World countries.

It is in this area of analysis that I find Professor Kennedy the least persuasive. It is true that his cross-national data do not show necessary injurious relationships to various aspects of growth and development from military expenditures and production; furthermore, high-growth states have also spent high percentages of their budgets on their armed forces. This type of analysis, however, begs the question of whether there would have been even higher GDPs, more welfare, and higher levels of industrialization without such attention to the military. To a degree these problems with cross-national data are ameliorated by the extensive case studies describing the economics of war in such individual countries as Nigeria, Israel, Egypt, Pakistan, and the neighbors of Vietnam. While the reader may question Kennedy's general tendency to discount the harmful effect of the military, he does bring considerable data and careful reasoning to bear in his confrontation with many accepted arguments.

I recommend this book highly to both the general reader and specialist. The country or area expert may quarrel with details of specific case studies; the reader may be put off by Kennedy's tendency toward asides, and some overstatement; and I myself was unable to accept some of the conclusions drawn from the cross-national analysis. Nevertheless, the book asks many of the right questions and includes most welcome compilations of data.

FRED R. VON DER MEHDEN

Rice University

Le fédéralisme soviétique: Ses particularités typologiques. By Theofil I. Kis. (Ottawa: Éditions de l'Université de'Ottawa, 1973. Pp. xiii, 191. \$4.95.)

This study of federalism in the U.S.S.R. is not directly concerned with the Soviet nationalities problem, nor is it an attempt to understand the actual political relationships between the men in charge of the major federal units and the central authorities in Moscow. It is, instead, an examination of the concept of federalism as it developed in the U.S.S.R. and is

expressed in Soviet constitutional arrangements. Professor Kis's basic thesis is that Western scholars should not dismiss Soviet federalism as a mere sham but should accept it as a unique form of federal system based on theoretical principles which, though generally couched in traditional terminology, differ radically from those underlying the federal systems of the West. The most important difference is that Soviet federalism, unlike others, was never intended to provide decentralized administration or local autonomy but rather was conceived as a means for moving eventually toward a fully centralized, unitary state.

Although the author asserts that his approach is a "normative-structural-functional" one, his work is overwhelmingly historical and legalistic in the most traditional sense. The first half of the book is devoted to an examination of the development of the concept of federalism in Russia. Kis provides an interesting and concise survey of some of the prerevolutionary Russian attitudes toward federalism and shows that the generally antifederalist views which prevailed among the non-Marxist Russians were matched, for different reasons, by strongly antifederalist attitudes on the part of Marx, Engels, and Lenin. He then presents the reasons that Lenin and his associates decided to alter their attitude on this question during the years preceding the formation of the U.S.S.R. in 1922-1924. Essentially Kis sees Lenin as having accepted federalism as a temporary compromise designed to alleviate dangerous nationalities pressures, to prevent the breakup of the multinational Russian state, and to facilitate a gradual reintegration of the nationalities into the kind of centralized state which Lenin, as a Russian and a Marxist, preferred. Stalin, an advocate of maximal centralization during the debates on the constitution for the U.S.S.R., overtly accepted Lenin's formula but thereafter reverted to his initial preference in practice.

Most of the second half of the book is devoted to an examination of the "typological and Constitutional particularities of Soviet federalism" in the period since 1944 when the Stalin Constitution of 1936 underwent a theoretically important modification which allowed the union republics to conduct their own foreign relations or at least to maintain institutions ostensibly intended for that purpose. Although the author clearly recognizes that a number of features of the Soviet system, including the centralized Party structure, the role of the central authorities in economic planning and foreign policy making, and the overwhelming strength of the Russian republic

within the federation, contradict in practice the Soviet claims concerning the sovereignty, autonomy, independence, and right to secession of the union republics, he nevertheless insists on examining each of the provisions of the Constitution as well as the theoretical and legal arguments put forward by various Soviet authors related to these claims. For most readers, including many specialists on Soviet politics, much of this analysis will inevitably seem rather tiresome and futile. It may be somewhat more interesting for scholars engaged in the study of comparative federal systems and constitutions; but even for them, Kis's insistence that Soviet federalism is a unique phenomenon, and his tendency to avoid discussion of actual political interrelationships between the center and the union republics will restrict the book's usefulness.

Finally, the book contains a very brief discussion of some of the implications for Soviet federalism of Khrushchev's attempts at economic decentralization and of the debate among Soviet academic specialists both at that time and after these reforms were abandoned concerning the desirability and speed of the future "coming together" and "merging" of the peoples of the U.S.S.R. Kis finds that while there are profederalists and antifederalists, most of the Soviet theoreticians whose works he has examined do not take an extreme position and that Soviet theory on federalism has generally tended to stagnate in recent years. One could feel more confidence in this assessment if the author had examined more of the Soviet periodical literature on the subject and had included more materials which have appeared since the mid-1960s, particularly the many Soviet works which were published in connection with the fiftieth anniversary of the U.S.S.R. in 1972.

DAVID L. WILLIAMS

Ohio University

Political Regime and Public Policy in the Philippines: A Comparison of Bacolod and Iloilo Cities. By Howard M. Leichter. (DeKalb, Ill.: Center for Southeast Asian Studies, 1975. Distributed by the Cellar Book Shop, Detroit, Michigan. Pp. 165. \$7.00, paper.)

Professor Leichter argues in this monograph that regime type (oligarchy or polyarchy) has been the determining factor in the adoption of differing public policies in Bacolod and Iloilo, two Philippine cities. Machine politicians in Iloilo City, having governed since 1955 and lacking an independent economic power base,

more intensively exploit office-based resources than their Bacolod City counterparts. In Bacolod City, by contrast, a homogeneous oligarchy owning sugar centrals and large tracts of sugar acreage dominates politics. Political pressures and expediency in Iloilo affect policy in several ways: expenditures on General Administration (i.e., the Office of the Mayor) comprise a larger percentage of total city expenditures than in Bacolod and absorb a larger percentage of mayoral supporters in the local bureaucracy; budgeting is less orderly (as indicated by an excessively large number of appropriations ordinances); fewer permanent civil service eligibles are in the police force and in various policy areas; and there is less continuity than in Bacolod between past and current expenditures.

By "politicizing" protective, administrative, health, and other services to broaden their base of political loyalty, Iloilo mayors and councilors demonstrate a different decision-making style than their Bacolod counterparts. The latter, homogeneous, are able to depoliticize some policy areas such as zoning where policy is made by a combination of government, professional, and civic leaders on the City Planning Board, rather than by the City Council. Since continued elite rule in Bacolod has never been questioned, factional conflict (measured by the number of different factions or parties represented on the city council and by the number of ordinances between 1960 and 1970 receiving negative votes) more fully expresses itself. In Iloilo, by contrast, the political elite is less secure, and consequently seeks to suppress interfactional conflict.

In order to substantiate his argument, Leichter assembles an impressive array of longitudinal data on different policy areas (e.g., regulatory, distributive, extractive), and carefully distinguishes among different types of public policy and the conditions which influence them. Detailed historical analysis shows, for example, that Bacolod is more active and progressive in its use of zoning ordinances for several reasons, such as an active planning board that is successful, in part, because of elite homogeneity. By comparison, the City Planning Board in Iloilo responds largely to crises (e.g., a fire downtown in 1965), often is undercut by the city council, and functions intermittently. Bacolod, unlike Iloilo, has a comprehensive zoning policy governing land use and urban planning.

Leichter's effort to show the importance of regime type is not entirely successful. Because regime type, an aggregate variable, is never measured directly, it remains unclear what specifically is more or less important in influencing policy. For example, we do not know how explicit political factionalism within the city council affects the number of appropriations bills passed. Leichter argues that Bacolod, ruled by a cohesive oligarchy, has a more rational budgetary process - that is, it passes fewer ad hoc appropriation bills than does Iloilo. And yet Iloilo has less overt political conflict and factionalism in the city council than Bacolod because, according to Leichter, the Iloilo political machine is threatened by discord. In arguing this, Leichter ignores a possible alternative explanation that factionalism in the Bacolod City Council makes passage of appropriations ordinances difficult, regardless of the rationality or irrationality of passing any particular appropriation.

A second weakness of Leichter's monograph is that his analysis of policy change rates does not link fluctuations in revenues, some of which are tied to particular expenditures, to changes in those expenditures. Without such analysis, we do not really know whether and how politics in Bacolod and Iloilo affect incremental budgeting.

Third, Leichter carelessly interprets his data on regulatory ordinances. He concludes that the undifferentiated elite in Bacolod passes fewer such ordinances. Yet prior to 1955 Iloilo was also ruled by an undifferentiated elite (the Lopezes and Ledesmas); even then, however, Iloilo consistently had more regulatory ordinances than Bacolod (Table 19).

Despite these oversights, Leichter's monograph is original, stimulating, and occasionally provocative. It should be useful to Filipinists and policy analysts alike.

THOMAS C. NOWAK

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Political Corruption: The Ghana Case. By Victor T. LeVine. (Stanford: Hoover Institution Press, 1975. Pp. 169. \$7.50, cloth.)

In late 1975 Ghana's most recent (Anin) commission of enquiry concluded: "The devout have vanished from the land. Corruption is endemic throughout the whole society." Employing the ample documentation on corruption provided by several regimes, Professor Victor LeVine has written an extremely careful, thoughtful, and valuable study. He first develops a model of the nature and processes of political corruption and then proceeds to delineate and test for the nature, extent, and consequences of Ghana's political corruption.

LeVine provides a brief political history of

Ghana, a structural model for studying political corruption, several chapters which test for the levels, patterns, and pervasiveness of political corruption, an assessment of the critical contributing causes of corruption and its probable consequences, and, lastly, an attempt to compare Ghana's corruption and its effects with political corruption elsewhere.

LeVine's analytical model starts with a narrow definition of political corruption and postulates (1) the "core process - involving individual office-holders and political goods, resources, and transaction networks; and (2) the "extended process" - entailing the possible creation of a culture of political corruption and a corrupt informal polity. A culture of political corruption exists where networks of corrupt relationships become so ramified and systematized that they strongly influence and parallel the formal political system. These are extremely difficult concepts to test empirically; an affirmative finding involves measures of the level, pervasiveness, and impact of political corruption and a cutoff point beyond which one could say a culture of political corruption and informal polity exist.

LeVine employs a variety of measures (e.g., the value of corrupt transactions as a percentage of budgets), but ultimately he must render qualitative assessments. LeVine documents the growth of political corruption through increases in the number and value of losses to the government noted annually by the Auditor-General (although he does not give loss value as a percentage of the rapidly expanding public revenue) and the findings of commissions of enquiry under the Nkrumah regime itself. He then details some of the conclusions of the forty-plus commission of enquiry reports initiated by the successor military regime to discredit the overthrown Nkrumah government. These include a report on Nkrumah himself and reports on five institutions which, LeVine submits, were set up for legitimate purposes and "turned out to be agencies for institutionalized political corruption" (p. 30) - an expression with which it is possible to differ. This institutionalized corruption clearly characterized NADECO, set up to collect contract kickbacks, but was somewhat less so for the Ministry of Trade, whose last several ministers (but not the bureaucracy as a whole) became incredibly corrupt in distributing import licenses; it was also less true for the Guinea Press, which was legally and openly government-subsidized, even if it employed political criteria in hiring personnel. LeVine also details the more fragmentary evidence then available on political corruption in the post-Nkrumah period and concludes that it is diminished somewhat in scale. More recent findings show that Prime Minister Busia (1969-72) became quite corrupt in office, as did some of his ministers.

It is impossible to do justice to LeVine's extensive and thoughtful presentation of evidence. In analyzing the values supportive of political corruption through interviews of twelve officials found to have been corrupt, he finds that these officials expressed a high level of cynicism regarding uses of public authority, a high level of political efficacy in manipulating power and influence, a narrow and parochial sense of obligation (self and extended family), and no feelings of guilt. He demonstrates the transactional networks of persons involved in corrupt dealings with interview evidence, the extent of corruption among top Nkrumah regime officials through inquiry findings of their assets, and, in fascinating detail, a wholly corrupt bureaucracy with the enquiry report on the football pools.

LeVine concludes fairly that while a culture of political corruption certainly existed, it had probably not developed into a full-blown corrupt informal polity, such as in Morocco and Sukarno's Indonesia (pp. 71-78).

LeVine assesses the contributory causes in terms of (1) traditional contexts and the effects of colonialism (i.e., market economy and cash nexus), (2) the "new men" seeking political power, and (3) the vast expansion of government/bureaucratic activity, which increases the scope for corruption. LeVine disagrees with much of the literature regarding the possible benefits of political corruption generally, and in particular in Ghana's case, and details the tremendous costs in terms of wasted resources, reduced government capabilities, and political instability.

Several criticisms can be made. The section on political history contains a number of avoidable factual errors: e.g., the Kulungugu assassination attempt occurred in August 1962, not 1964 (p. 28); in 1956, we are told, CPP election support had "shrunk" to 57 per cent (p. xx), but it was officially 56 per cent in 1954; Nkrumah did not appoint many followers to the upper ranks of the civil service, in contrast with public corporations. More important, I think that LeVine's analysis of the role of the CPP "new men" in politics under Nkrumah as a cause of corruption fails to make some important distinctions; the borrowed notion of a "bureaucratic bourgeoisie" is employed without adequate dissection; and there were differences in the corruptability of the "socialists" and the CPP "old guard" who were

parties to the intra-CPP conflicts, as the asset reports make clear. An analysis of those who were not corrupt and the struggles against corruption would have provided a more dynamic political explanation of why the forces tending toward corruption prevailed.

Nevertheless, LeVine's study of political corruption is an excellent contribution to our understanding and to approaches to analysis. For those who care about countries like Ghana, this is a painful subject. And LeVine's study is all the more welcome for the absence of easy cynicism and a hypercritical tone. It is graced with realism and restraint.

JON KRAUS

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Underdevelopment in Kenya: The Political Economy of Neo-Colonialism 1964—1971. By Colin Leys. (Berkeley: University of California Press, 1974. Pp. 302. \$10.50.)

It is a privilege to review a book as stimulating as *Underdevelopment in Kenya*. Colin Leys has undertaken in this succinct volume the demanding task of analyzing the way in which colonialism generated underdevelopment and how and why the process of "neo-colonial development" is being perpetuated.

Many scholars, following upon Frantz Fanon, have equated African bureaucracies with an emerging bourgeoisie arguing that members of the bureaucracy use their positions to build an economic base as a new class. In contrast, Professor Leys analyzes the still fluid class structure in Kenya in light of Marx's Bonapartist model as set forth in The Eighteenth Brumaire of Louis Bonaparte. Thus Levs notes how the alliance among (a) foreign capital. (2) an African auxiliary bourgeoisie of manager/partners in multinational corporations and in the higher ranks of the public sector and (3) an African petty bourgeoisie of merchants and farmers provides those conditions which ensure that no single class dominates the state apparatus in its own interest. The leadership thereby retains the autonomy of class that characterized the Kenyatta regime.

While "Kenyatta and Mboya were not tools of foreign capital," Leys argues, "... they were collaborating closely with it..." (p. 221). The auxiliary bourgeoisie was not, therefore, a comprador bourgeoisie in the classical Marxist sense.

The exigencies of this alliance, however, required that the Kenyatta regime depend upon the civil service, parastatal bodies, police and armed forces all of which were staffed with

reliable Kikuyu. To offset this dependence, the Kenyatta regime made concessions first to one then to another element of the ruling class alliance. Such behavior involved a succession of what appeared to be contradictory measures. Foreign capital was ritually humiliated by the rhetoric of economic nationalism but welcomed back in partnership with the auxiliary bourgeoisie, thereby permitting "... a steady expansion of foreign ownership of productive assets in Kenya, and thus of the political power of foreign capital" (p. 208).

At the same time, we are told, the power of peasants and urban workers has been progressively curtailed and neutralized. Continued survival of the peasant mode of production, encouraged by the state, enables employers to keep wages low by deducting from the price of labor power that which the family earns from its own garden. Preservation of the peasant mode also guarantees the supply of agricultural commodities at low prices relative to the manufactures which must be bought, and provides for the absorption of an increasing portion of the adult population (pp. 170-173). Meanwhile, the tripartite agreement among the state, Kenya Federation of Employers, and the Kenya Federation of Labour deprived organized labor of the right to strike, thus further institutionalizing labor passivity.

Leys suggests that a neocolonial pattern of "associated dependent" development characterizes the Kenyan political economy. This pattern emerges in those ex-colonies "... where the transition from colonialism to independence permitted the relatively efficient transfer of political power to a regime based on the support of social classes linked very closely to the foreign interests which were formerly represented by the colonial state" (p. 27). The Kenyatta regime's decision to purchase the mixed European farms in the "white highlands" at full market value, to finance this purchase by loans, and to sell the land to landless Africans on an individual basis represented an official endorsement of capitalism and as such was "the linch-pin of the transition from colonial rule..." to neo-colonialism (p. 63). Dependent capitalist development also implied the granting of legal monopolies that ensured a central role for the state. Legitimized during the colonial period were monopolies of access to capital, roads, labor, most profitable crops, credits, schools and other services that benefitted European settlers. In this way the monopoly nature of neocolonial development would be preserved to meet demands from the African petty bourgeoisie for "fixed shares of specific markets, public loan funds or publicly guaranteed

commercial credit, fixed suppliers, and fixed prices" (p. 155).

But this neocolonial pattern of dependent development is not static. Among the contradictions to which Leys points is the latent although growing conflict between the auxiliary bourgeoisie and foreign capital as well as between African traders and foreign manufacturing interests (pp. 158-159). The Bonapartist strategy cannot be expected to continue indefinitely, especially once Kenyatta is dead. Perhaps the most essential contradiction of neocolonialism, however, is that underdevelopment "implies limits to growth and a growing polarization of classes as the exploitation of the masses becomes more apparent" (p. 271). It is this contradiction which still requires further exploration.

LYNN KRIEGER MYTELKA

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The Communist Parties of Western Europe. By Neil McInnes. (London: Oxford University Press, 1975. Published for the Royal Institute of International Affairs. Pp. 209. \$16.00.)

Neil McInnes is European editor for Dow Jones, which is famous for its averages, *The Wall Street Journal*, and *Barron's*. But McInnes's book not only lacks the superficiality characteristic of deadline journalism. It also avoids the failings of the typical study of nonruling Communist parties available from front-line academics.

To confirm this second point, I surveyed my university's library holdings on West European communism, published in English since 1950. Of twenty-two books, I judged only seven as at least tentatively "comparative" while no more than two reflected the author's conviction that generalizations about communism deserve empirical support. Thus it is with a sense of professional admiration and scholarly enthusiasm that I register Neil McInnes's book as a landmark study of West European communism. It is richly comparative and solidly based on empirical research.

Even the general organization of the book facilitates cross-national analysis, with the chapters focusing in turn on the electoral and organizational strengths of the parties, the characteristics of their membership, their structure (both on paper and in fact), their ideological and programmatic orientations, and the apparent goals and hard realities of alliance politics in pursuit of national office. McInnes frequently includes in his analysis the character-

istics of non-Communist parties, e.g., with regard to party factionalism, membership militancy, and party financing; these comparisons enhance our understanding of a particular Communist party by placing it in its broader national context. Most students of communism have treated their subject in isolation and consequently have attributed to a particular Communist party characteristics that instead may be generic to the political culture of the whole society.

McInnes assembles data on membership strength for twenty-three Communist parties, trend data on the electoral fortunes of fifteen parties, the most recent election returns for eighteen parties, and additional data for several parties on the size of their associated trade unions, circulation of the party press, membership turnover, party finance, age and SES characteristics of party members, voting support by region, sex, and occupation. There is ample detail on party organizational structure and an array of minicase studies on leadership succession, party factionalism and decision making, and party relationships with Moscow. The narrative materials, all competently integrated with the data, are based largely on original sources from the parties and from the national press (in French, Italian, German, Spanish, Dutch, and the Scandinavian languages).

The author has succeeded in imparting to all these materials a conceptual coherence that is unique to the study of nonruling communism; nor is it often found in the more broadly focused studies in comparative politics. A critical measure of an author's conceptual ability is his propensity for clear generalization and the formulation of testable hypotheses. And in the case of McInnes's densely packed 200 pages of text, I have counted approximately 30 major generalizations and 23 hypotheses with clearly specified variables. Some examples:

The electoral and organizational strength of West European communism is primarily a function of *political* rather than socioeconomic conditions (pp. 69-70).

"Responsiveness to electoral performance is increasingly a feature of West European communism..., even if the Leninist structure of the parties sets limits to that responsiveness" (p. 34).

"The practice of the West European CPs is electoralist, parliamentary, revisionist" (p. 181).

The programs and ideological orientations of the parties are less an expression of "forceful social criticism" than they are of "an archaic and incoherent populism" (p. xi).

The percentage of working-class members in the major Communist parties is inversely related to the extent of advanced economic development (pp. 60-61).

Declining electoral support increases factionalism within the party (p. 34).

Divisions within the international Communist movement are more likely to affect the stability of the party organization than the strength of Communist electoral support (p. 30).

The generalizations and hypotheses proposed by McInnes are in turn integrated within a more general conceptual framework that is meant to explain and predict Communist party behavior in the larger West European context. McInnes identifies three power centers or sources of loyalty and competing demands: a conservative party bureaucracy, a more radical and utopian-oriented core of working-class supporters, and the foreign policy interests of the Soviet Union. The frequent conflicts and rivalries among these three centers explains why the parties cannot be understood as monolithic or as "organizational weapons." It also explains the frequent inconsistencies within party programs, between ideology and behavior, and the various patterns of behavior across the spectrum of the nonruling Communist parties (e.g., as when support for Soviet foreign policy proves incompatible with an enlarged electoral following, or when revisionism for the purposes of political alliance jeopardizes the support of militant workers).

Undoubtedly there are problems in so ambitious an undertaking. McInnes does not explain how he reaches the conclusion that "no more than 20 per cent of communist workers" (p. 150) constitute the proletarian hard core that represents one of the three power centers, nor does he explain precisely how this particular center exerts its influence on and interacts with the party bureaucracy, or how its size and orientations may vary over time. Perhaps more important, a broadly based cross-national approach to the study of any political phenomenon seems to dictate an illustrative and narrative style that often proceeds with haste and sometimes sacrifices the accuracy of detail for the satisfaction of generalization.

This may be to say only that it is impossible for one book to be all things to all readers. But a measure of McInnes's accomplishment is the conceptual utility that derives from his rigorously comparative and empirical methodology; his book helps us to organize the findings of studies in the field that exemplify alternative methodologies — including the noncomparative, case study, and more impressionistic ap-

proaches to understanding nonruling communism. And, by itself, McInnes's book is unquestionably a major contribution to the field. It also is an example to students in comparative politics of how to be truly comparative.

THOMAS H. GREENE

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Suffragists and Liberals: The Politics of Woman Suffrage in Britain. By David Morgan. (Totowa, N.J.: Rowman and Littlefield, 1975. Pp. 184. \$12.50.)

All Liberal and Labour Governments in Britain this century have grappled, to some degree or other, with the thorny problems of electoral reform. The present Labour Government, short of an overall majority of seats in the Commons on the basis of just 39 per cent of the votes in the October 1974 election, faces renewed demands for electoral reforms to achieve a more equitable relationship between votes and seats. As the Labour Ministers consider whether or not to flirt with some changes in the voting system they could do worse than read David Morgan's Suffragists and Liberals, which catalogues the agonies of the Asquith Liberal Cabinet as it faced the problem of how to respond to the demands for woman suffrage some sixty years ago. The book complements Morgan's earlier Suffragists and Democrats (East Lansing: Michigan State University Press, 1972), which dealt with the politics of woman suffrage in America.

There is no shortage of studies of the woman suffrage campaign in Britain, but for the most part they have concentrated on the suffrage movement and its leaders, rather than on the politicians they faced. As Morgan points out, the campaign has been written up "as though the politicians were a static 'given'" (p. 3). Morgan has attempted to fill this gap by presenting a detailed account of the interparty and intraparty machinations in the 1910-14 period, based upon the papers of leading Ministers and Opposition leaders. He has produced, in effect, an account of how women failed to persuade the Government to grant them the vote in the last years of "Liberal England," rather than an account of how they eventually were enfranchised after the 1914-18 war had revolutionized British politics and society.

Morgan's detailed analysis of the considerations that motivated the politicians' attitude to the suffragists serves as a timely reminder that with electoral reform more than most issues, questions of principle (is this proposal "a good thing" in itself?) are likely to be subordinated to considerations of short-term partisan advantage. (How will this proposal affect our immediate electoral prospects?) He illustrates just how complex was the party lineup on the suffrage issue after the 1910 elections. While some unionist diehards were opposed completely to votes for women, the party as a whole favored a limited measure that would enfranchise only women from the property-owning classes. The Labour and Irish parties, holding the Parliamentary balance of power after 1910, were broadly sympathetic to the women's cause. Labour, however, was unwilling to support any measure: as long as the whole franchise was based upon a property qualification, while the Irish were wary of any measure that would use up the Parliamentary time and good-will that was required for the passage of a Home Rule Bill. For their part, the Liberals, though sympathetic toward electoral reform, were afraid of jeopardizing their plans for the abolition of plural voting by linking it with a votes for women proposal. Eventually, the influence of Asquith was decisive in causing the Cabinet to place the suffrage issue fairly low on its list of legislative priorities in the crucial sessions of 1911 and 1912.

All in all, Morgan has produced an impressive case-study of British Parliamentary party relationships, and of the British Cabinet at work. The history he recounts clearly has its lessons for contemporary British politics. Political scientists and practicing politicians, as well as historians, can learn much from this account of the pressures to which a British Government is subject when (be it 1977 or 1912) it lacks a working majority in Parliament, faces competing demands for legislation on the major constitutional issues of home rule and electoral reform, and is divided within its own ranks on these issues.

R. M. PUNNETT

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Revolution at Querétaro: The Mexican Constitutional Convention of 1916-1917. By E. V. Niemeyer, Jr. (Austin: University of Texas Press, 1974. Pp. 297. \$10.00.)

It is good for students of comparative constitution-making to have a book like Niemeyer's, to remind us of the different paths by which modern nationhood may be approached. The title is well chosen, stressing as it does that the distinctive feature of the Mexican revolution was not the armed conflict of 1910–1915.

After all, that phase merely displayed the same characteristics of violence, disorganization, and power struggles as other revolutions and rebellions of history. The real revolution and the real victory lay in the adoption of the first truly modern constitution, responsive to the needs of the twentieth century, incorporating not only the old eighteenth century notions of political rights but also the completely new concepts of social and economic guarantees.

For comparison it is worth noting that in 1952 when the Commonwealth of Puerto Rico included some mild clauses in its constitution referring to the right to work and to obtain an education, the U.S. Congress refused to ratify the document until those clauses were deleted. They were held to be "inappropriate" and "unrealistic."

Niemeyer's chronicle shows us how the three unique articles of the constitution - No. 27, reaffirming national 'ownership of subsoil resources and outlining an agrarian reform program; No. 123, specifying the rights of labor; and No. 130 (the so-called "anticlerical" article), regulating religious activities - took shape during the convention itself, with no prior examples to guide the delegates. The chronicle further reveals that another revolution took place at the convention, one in which the forces of the Left, captained by Francisco Múgica and Heriberto Jara, outmaneuvered the timid liberals, achieving adoption of a document that went far beyond the aims of Venustiano Carranza, who had convoked the gathering.

Describing the vote on Article 3 (providing for free secular education) as the most significant of the convention, Niemeyer states that the outcome "showed ... that the delegates would decide the great issues according to their own convictions and not according to loyalty to any particular revolutionary chieftain or interest groups" (p. 78). This is one of the few occasions in which the author permits himself to interpret the record. For the most part, he seems to stand diffidently on the sidelines, presenting unadorned to the reader the chronological account of the convention sessions and a few thumbnail biographical sketches of the delegates. His selection of quotations, particularly those used as epigraphs, contributes little to the reader's understanding of the issues involved.

On the other hand, he manages to convey the general atmosphere of provincialism, roughness, and crudity from which the remarkable document emerged, like an ugly duckling. The student of American history cannot avoid comparing such quotations with the polished pronouncements of delegates to our own constitutional convention, many of whom were graduates of the Inns of the Court. At some points, Niemeyer's almost Victorian delicacy prevents him from giving the full flavor of his subjects' words, as when he mistranslates the remarks of Manuel Amaya, the presiding officer at the preliminary sessions, who had left the hall for a few moments. On returning, Amaya announced from the chair, "No salgo para fumar sino para mear" (p. 53, fn.). The author substitutes the latinized form of the final verb instead of the more accurate Anglo Saxon piss.

Feminists will be grateful to the author for his coverage of the background which led to omission of guarantees of women's rights. In an atmosphere of barroom bonhomie, the delegates on one occasion discussed the matter of rape in connection with their own libidinous proclivities, and on another occasion rejected out of hand Felix Palavicini's lone plea for woman suffrage (pp. 203–210).

Although there is no document comparable to Madison's journal, Niemeyer's painstaking reading of available materials helps fill the gaps in our understanding of the processes through which the revolutionary articles evolved. We are told, for example, that two days before adjournment, after almost frantic activity of the drafting committee, Article 27 was presented, discussed, and voted on in one evening, "with only three or four copies of the draft available" to the 220 delegates (p. 143).

A Mexican lawyer once told me, "we have the best constitution in the world, and some day it will be a reality." Niemeyer's book renders a service to the English-reading public in showing how that constitution was adopted. Reality may have to wait a while yet.

EVELYN P. STEVENS

Cleveland, Ohio

Saints and Politicians: Essays in the Organization of a Senegalese Peasant Society. By Donal B. Cruise O'Brien. (New York: Cambridge University Press, 1975. Pp. 200. \$13.95.)

One of the most vexing questions about developing states concerns the nature of relations between the urban, modernized national center and the rural, traditional structures. This can be examined in many ways, such as the role of national political structures, the tutelary notions and kleptocratic penchant of leaders, the effect of ethnic fragmentation, or the growth of class struggle. O'Brien focuses on two aspects of social structure which tend toward a measure of stability in Senegal: one is a given,

the dominance of the Wolof; another, more crucial, is the role played by Wolof leaders. Most Wolof are organized in Muslim brother-hoods and monopolize the production of the country's only cash crop; therefore Muslim saints — particularly among the Mourides — have become indispensable intermediaries as they enjoy peasant legitimacy, control and dispense optimal material rewards, and use their political base to extract or deflect demands from the national center, thus helping to provide an "underlying coherence of Senegalese politics" (p. 13).

The present cohesion of the Wolof is the result of historical accident and of leadership by three kinds of actors: warlord, saint, and knight. Before the French conquest, they were divided into two factions struggling for ideological and political supremacy: military Muslims, and "backsliding semi-pagans" controlled by chiefs and warriors. When the remnants of Wolof armies fell to the French, new leaders emerged and reorganized the disoriented Wolof into brotherhoods combining religious puritanism with a new economic base. Warlords faded from the scene because of their conservatism. Saints, seen as ascetic scholars endowed with Weberian charisma, provided the religious and ideological base for change. And knights, marching in on behalf of the saints, placed their organizational skills and ambitions at the disposal of the new order. The role played by such leaders is well researched and presented with drama and sympathy, but also emerges as a function of chance as much as planning and initiative.

O'Brien suggests that the current stereotype of the Mouride peasant as coldly exploited by religious confidence men is accurate only in part. French land grants, distributed without social discrimination, reinforced the saints' religious legitimacy and material power to draw converts from disenchanted lower castes. Brotherhoods like the Mourides thus serve three main functions: spiritual rewards on the way to paradise after death, material rewards providing a living standard better than that of most rural Senegalese, and political rewards through pooled wealth used to purchase favors from the national government. Brotherhoods operate in a system of "creative indigenous adaptation" which serves as an important channel for "patronage redistribution from the centre to the localities" (p. 177), an informal context in which the saints remain the major actors. Peasants thus behave rationally since they have some access to political influence. Although they are exploited, they have resisted other types of particularism because of overarching

religious allegiance combined with minimally optimal materials rewards.

The author points out that disregard for formal rules was institutionalized during the colonial period, and distinguishes four styles of elite predation: bureaucrat, party politician, private trader, and saint (President Senghor's fulminations against corruption, incidentally, are directed at the first three and seldom the latter). Since only the saint has roots in peasant society, and since he combines moral sanctions with extractive skills which benefit followers, only he is a predator with an effective sense of noblesse oblige.

This volume largely achieves its stated objectives: a historical review of colonially-induced changes, and "an interpretation of the present situation which does some justice to its real complexities." The book is also a bit décousu. the common property of most collections of previously disparate essays. The treatment of the Wolof, especially of the Mourides, is obviously sympathetic, and the materials used provide a useful corrective to conclusions reached by other observers. The "meek did really inherit the earth," at least for a while. But the author also sees that the saints' control is slipping, if only for lack of additional land they prefer short-term speculative gain to longrange productive investment in the national economy. In the end, earlier adaptation and innovation seem to have lost steam, and the question of the saints' future viability remains open.

W. A. E. SKURNIK

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Political Development and Political Parties in Turkey. By Ersin Onulduran. (Ankara, Turkey: Ankara University Press, 1974. Pp. 116. TL 12.50.)

State and Society in the Politics of Turkey's Development. By Ilkay Sunar. (Ankara, Turkey: Ankara University Press, 1974. Pp. ix, 196. TL 20.)

The literature on modernization in Turkey continues to grow, and fortunately much of it is being produced by Turkish social scientists. Few "developing" countries have as good an indigenous body of scholars as Turkey, scholars who can study themselves with considerable objectivity and add an invaluable dimension to both theoretical and practical guidelines for appreciating the contributions which can be made by study of this extremely interesting country. Its quarter century of uninterrupted multiparty politics is a rarity among "develop-

ing" nations. How this has happened, and what are its implications, are the subjects of these two books, both originally written as doctoral dissertations at American universities.

Onulduran traces the role of political parties in Turkish "modernization." While he breaks little new ground that has not been covered in parts of other studies, his small book is useful in enabling those who are not experts on Turkey to get an overview unavailable elsewhere. A discussion of modernization theory is followed by some pre-Republic history, leading up to an analysis of the roles of contemporary major parties. The three most important ones (Republican, Democratic, Justice) are discussed in terms of their platforms, tactics, and social bases. Onulduran also supplies a very interesting analysis of their voting strength among different educational, occupational, and regional groupings.

Onulduran concludes that several important conditions of "political development" have been met, including firm anchoring of party politics; he also avers, perhaps most significantly, that the two major parties are essentially moderate and thus appeal to the electorate increasingly in terms of issues. He holds that since, in addition, mass participation is fully accepted, new groups are constantly emerging and making politics ever more pluralist and ever more flexibly attuned to the needs of the nonelites.

Sunar's book is much more ambitious, but it is striking that he basically comes to the same kinds of conclusions. Sunar traces the development of state and society from Ottoman times to the present. He sees the integration - or lack of it - of state and society as the major variable which led to the failure of most reform attempts in the Empire, and he sees shortcomings in such integration as accounting for many contemporary Turkish problems. These deficiencies include the rapidity but unevenness of social and economic change, and the conservatism of those very leaders who might have helped Turkey toward more rapid and basic change. Sunar criticizes a number of important theories, such as Halpern's ideas about the modernizing role of the New Middle Class, and some of Kemal Karpat's hypotheses about Ottoman development. He correctly concludes that working-class, collectivist movements which flourished in the 1960s failed basically because despite the rapid change which Turkey has undergone, socially conservative attitudes and structures have persisted (e.g., "urbanization rather than industrialization has been a decisive factor in the development of class relations in Turkey" [p. 151]).

I share the concluson stated or implied in both books that the populists rather than the Marxists will have the best chance of continuing Turkish modernization and that "if there is any chance of further progress in Turkey, it rests largely with those who can appeal to broad strata of society and incorporate them effectively into the polity" (Sunar, p. 185). Possibly the most intriguing fact about Turkey, however, is that it has developed two large parties, the moderate left Republican People's Party which has revitalized itself after bearing the image of twenty-five years of an earlier autocratic period, and the moderate right Justice Party under whose auspices much of Turkey's recent development has taken place. Each of these parties has based its strength on a combination of many strata, and this fact has enabled Turkey thus far to continue both development and multiparty politics. What will bear close watching is whether one of them, and if so which one, will be able to handle the ever more difficult problems of a now fully mobilized population. For example, can multiparty politics in a "developing" nation produce leaders who are courageous and strong enough to effect both mass and elite changes which will improve economic performance by mobilizing the latent talents of the lower strata? Such actions are likely not only to diminish the power of some currently dominant groups and to demand greater production discipline from all, but must at the same time respect the conservative social values of most of the Turkish population so as to minimize the social and personal dislocation of many Turks whose retreat might well be to the radical right rather than to the left.

On specific points in both books experts on Turkey will dissent. In addition, Onulduran presents some problems in terminology (is "the nonelite professions" a useful term, for example?); and one wishes (among other things) that Sunar had done as thorough an analysis of international aspects for the current period of Turkish development as he did for the Ottoman period. But both authors clearly indicate (and the 1973 and 1977 elections corroborated the point) that a relatively modern, issue-oriented, and discerning electorate has emerged in Turkey as a result of a half-century of continued political development. For all students of political development who want to see whether Turkey's extremely interesting future challenges can be met, both Sunar and Onulduran provide a solid background.

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Revolution Administered: Agrarianism and Communism in Bulgaria. By Nissan Oren. (Baltimore, Md.: The Johns Hopkins University Press, 1973. Pp. 204. \$8.50, cloth; \$4.00, paper.)

Of all the Communist countries of Eastern Europe Bulgaria has been the least affected by the winds of change that have swept across the continent in the last decade. Whereas first Yugoslavia, then Albania, and finally Rumania have strongly resisted Soviet hegemony, Bulgaria has sought to strengthen its ties with Moscow, especially since 1968 - which has made it an anomaly in an area that has become the stronghold of "national communism." One of the consequences of Bulgaria's docility in foreign affairs and of its internal orthodoxy has been a general lack of concern with - and informed opinion about - Bulgarian developments, not only among the general public but within the scholarly community in the West as a whole. While studies of Poland, Hungary, the GDR, Czechoslovakia - not to mention Bulgaria's Balkan neighbors Yugoslavia, Rumania, and Albania – continue to proliferate, Western scholars have generally adopted an attitude of "benign neglect" toward Bulgaria.

In light of the scant scholarly attention given Bulgaria in the West — especially since the Communist take-over — Nissan Oren's Revolution Administered: Agrarianism and Communism in Bulgaria is a welcome addition to a rather bare cupboard. His book is the eighth and last in the series of monographs by Jan Triska on "community building" and "integration" among Communist states in Eastern Europe. The emphasis in the series, as Triska notes in the foreword, is on viewing these states as a "system" and on trying to find "operational indicators" that facilitate systematic comparisons.

Despite the attempt to pay lip service to the conceptual framework of the Triska series, however, Oren's book is more a political history than a systematic analysis of Bulgarian politics. The basic problem is that the book falls between two stools. It does not really add much conceptual insight in terms of "operational indicators." At the same time the need to conform to the general framework of the series prevented Oren from doing full justice to the complexity of Bulgarian developments, as he did so admirably in his earlier work, Bulgarian Communism: The Road to Power, 1934-44 (New York: Columbia University Press, 1971).

As it stands, the book presents a rather uneven treatment of Bulgarian politics. The first four chapters (about half the book) deal with the period prior to the takeover by the Bulgarian Communist Party on September 9, 1944. Here Oren is at his best, tracing the ins and outs of party strife and discussing the factors that facilitated the takeover. In relation to the rest of the book, however, this section is too long - especially for a book that purports to treat Bulgaria in the postwar period and facilitate comparisons with other Communist states in Eastern Europe. By comparison the treatment of the years after 1949, particularly the period after 1966, is rather sketchy and in places superficial. For instance, with the exception of the treatment of the Macedonian question, there is little discussion of Bulgaria's interaction with its Balkan neighbors in recent years or of the interesting period in 1965–1966 when Bulgaria displayed an unusual degree of diplomatic boldness and came close to establishing diplomatic relations with Bonn. Similarly, little attention is given to agricultural developments, especially the establishment of the agro-industrial complexes (AICs), an area in which Bulgaria has been uncharacteristically innovative in comparison to other East European countries.

Finally, although intraparty politics are discussed in some detail, little attempt is made to assess the role and leadership of Todor Zhivkov, who after all has now been in power longer than any other head of a Warsaw Pact country (23 years). Admittedly Zhivkov is a dull and lackluster figure in comparison to other Bulgarian leaders like Stamboliski, Dimitrov, and King Boris, whose personalities and politics are extensively and commendably discussed by Oren. Nevertheless, Zhivkov's impact on Bulgarian politics has been just as great as, if not greater than, that of the others, and his role deserves more attention than Oren has allotted to it.

The rather sketchy and uneven treatment of developments since 1966 is all the more regrettable because from the comparative point of view this period is perhaps the most interesting. As noted earlier, there has been an unfortunate tendency among Western scholars to dismiss Bulgaria out of hand as an irredeemable bastion of orthodoxy hardly worthy of attention, and to concentrate instead on other East European countries such as Yugoslavia, Hungary, or pre-1968 Czechoslovakia, whose pattern of reform has tended toward greater economic and political decentralization. This tendency has reflected an inherent, if unconscious, bias on the part of Western students of Communist societies, who have tended to associate modernization with greater economic and social pluralism. When little evidence of such pluralism can be found - as in the case of Bulgaria — many Western scholars have seen this as evidence of "stagnation" or "degeneration."

This unconscious bias is present in Oren's book as well. It is hard to question his concluding judgment that as a result of the takeover of the BCP "[domestic] politics was converted into mere public administration and international politics all but lost its meaning." From an analytical point of view, however, Bulgaria's "deviation" from the pattern of political developments elsewhere in Eastern Europe is precisely what makes it interesting. The fact is all too often overlooked that behind its rather placid facade Bulgaria has developed a distinct pattern of modernization that combines high economic growth with tightly controlled economic and political centralization. This pattern contrasts markedly with the pattern of modernization elsewhere in Eastern Europe, particularly Hungary. Yet the Bulgarian "model" is one the Soviet Union would like to see developed in other East European countries (one of the main reasons Moscow has been willing to underwrite Bulgaria's modernization with extensive credits), and, given present trends in Eastern Europe, it may become even more relevant to developments in Eastern Europe in the years to come. It therefore deserves more attention from Western scholars than it has received to date.

In sum, Oren's book is a highly readable, if uneven, essay on Bulgarian politics, more useful to the undergraduate desiring a general overview than to the specialist concerned with acquiring a detailed understanding of postwar Bulgarian politics or to the methodologically oriented scholar interested in comparative analysis. To a large extent owing to the limitations imposed by the framework of the series of which it is a part, The Revolution Administered is not really a sequel to Oren's earlier, admirable study, Bulgarian Communism: The Road to Power, 1933-44, referred to above, which will long remain the definitive work on this period. The specialist can only hope, therefore, that Oren will one day be able to turn his attention to writing a real sequel, because he is uniquely qualified to do so.

F. STEPHEN LARRABEE

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Contrasting Approaches to Strategic Arms Control. Edited by Robert L. Pfaltzgraff, Jr. (Lexington, Mass.: D.C. Heath and Co., 1974. Pp. 350. Price unknown.)

The book before us is a compilation of papers prepared in connection with the Sixth International Arms Control Symposium held in Philadelphia late in 1973. Robert L. Pfaltzgraff, Jr., Director of the Foreign Policy Research Institute, has served as editor; he and some fourteen other authorities on arms control have contributed essays. The focus of the book is on recent developments in strategic arms limitation, with specific reference to the implications of SALT I and the forthcoming SALT II discussions. Relevant documentary materials on the subject are included in three appendices.

As is not uncommon in symposia, the individual chapters vary considerably in interest, cogency, and the originality of their ideas. Not infrequently, the contributors advance contradictory conclusions about important trends and their implications.

In company with several other authorities, William R. Van Cleave is dubious about the benefits accruing to the United States from the SALT I agreement — and he is convinced that the SALT II stage of "qualitative" arms reduction will prove equally disadvantageous for America. His contention is that very few Americans have adequately understood the political and strategic motivations behind Soviet behavior on the disarmament question.

In Graham T. Allison's view, the Soviet-American "interaction" with regard to the steady escalation in strategic weapons is much more complex and ambiguous than is often supposed. Allison doubts, for example, that competition for more destructive weapons is sparked by some new development in which one side suddenly finds itself threatened (e.g., America's decision to develop MIRV missiles was not essentially a reaction to Russia's acquisition of the ABM). Conversely, a decision by Washington to limit its strategic and conventional forces would not necessarily lead to a Soviet decision to do likewise.

Morton A. Kaplan analyzes the extent to which SALT I, and potentially SALT II, affect the Soviet-American strategic balance, construing this broadly to include psychological and political aspects of national power. Various Soviet spokesmen are cited to the effect that the global balance now and in the future favors the Communist side. Kaplan's verdict is that the outcome of SALT I "in many respects... provides both for symbolic Russian superiority and potential actual superiority" (p. 78); and that SALT I and SALT II pose "deadly risks to American interests..." (p. 85).

Robert L. Pfaltzgraff believes that the value of SALT I for the United States will be heavily conditioned by the results of SALT II (and he

is fundamentally skeptical about American gains from the latter). Uri Ra'anan is even more doubtful about the benefits for the United States of recent arms-control measures. Groups favoring disarmament negotiations, he believes, routinely overlook (or discount) the extent to which arms limitation is favored by the Kremlin, as a tactic to promote its long-run political and strategic objectives, which have not really changed despite détente.

Several contributors discuss the regional implications of SALT I and SALT II. Joseph I. Coffey, for example, is apprehensive about the impact of SALT I upon the cohesiveness and effectiveness of NATO and the cause of Western unity generally. Gaston J. Sigur and Makoto Momoi appraise the consequences of recent arms-control agreements upon Japan's internal and external policies.

The role of Communist China in attempts to limit strategic armaments is evaluated in detail by Harry G. Gelber. Characterizing Peking as a "silent partner" in the SALT talks, he calls attention to the dilemma posed for policy makers in Washington and Moscow by China's growing military might. On the one hand, the inclusion of China in the SALT negotiations would almost certainly guarantee their failure. On the other hand, China's exclusion from these discussions of course vitiates many of the beneficial results of a Soviet-American understanding on arms-control. Gelber believes there is considerable validity in Chou En-lai's observation that SALT I was a sham. For its part, Peking shows little or no evidence that it desires to slow down the steady buildup of its own strategic arsenal or that it has abandoned a desire to achieve nuclear parity with Russia and America.

In one of the more original essays in the collection, George H. Quester advances a novel (if not altogether persuasive) thesis on the subject of nuclear proliferation. From the evidence thus far, attempts to prevent such proliferation have obviously failed. Yet Quester is not excessively pessimistic about the future. The American public — along with other "publics that matter" — has evidently lost interest in, and become bored with, the whole problem of nuclear deterrence. Rather sanguinely, Quester interprets this phenomenon as dampening the desire of "n-th countries" to join the nuclear club.

In an incisive essay, James E. Dougherty analyzes developments on the strategic weapons from "From SALT I to SALT II.' On balance, with several other contributors, Dougherty's verdict is more negative than positive about the consequences of SALT I for American, and for

overall global security; nor does he believe that the results of SALT I improved the negotiating stance of the United States in the forthcoming attempt to impose "qualitative" restrictions upon strategic weapons. Appropriately, he cautions American students of deterrence and disarmament particularly against a prevalent tendency to assume too much "rationality" in the behavior of nuclear-armed states. On the basis of recent experience in arms-control conferences, he finds no evidence that Soviet officials actually desire, or are willing to settle for, mere military "parity" with the United States. His own estimate is that Russia's military position vis-à-vis the United States is likely to improve during the remainder of the decade. After a detailed analysis of the results of SALT I, Jacquelyn K. Davis concludes that any significant reduction in America's defense effort is unjustified. She adduces several reasons why new defense projects - along with accelerated progress in research and development ought to be completed by the United States.

As a group, these essays on an important public issue are interestingly and ably written, objective, scholarly, and provocative. They highlight several aspects of the problem of arms-control which are often neglected by other commentators. Their collective impact is to raise substantial questions — and to express major doubts — about the gains accruing to the United States from the SALT I accord and about the advantages which may be anticipated from SALT II.

The overall verdict of the contributors is less than optimistic concerning the value of recent arms-limitation agreements. Yet in most of the essays, the authors successfully steer a middle course between the kind of mindless euphoria which events like SALT I and SALT II sometimes generate in both the official and the public mind, and the kind of cynicism which devotees of Realpolitik often display toward any new attempt by the superpowers to reduce global tensions. Implicitly, this volume warns the reader that neither attitude is warranted; and it calls attention to a host of new, important, and difficult problems which inevitably grow out of limited arms-control agreements in the contemporary world.

CECIL V. CRABB, JR.

Louisiana State University

Willy Brandt: Portrait of a Statesman. By Terence Prittie. (New York: Schocken Books, 1974. Pp. 356. \$10.50.)

Although Prittie's is not a good book, it

merits brief discussion. First, the author, a long-time German correspondent for the Manchester Guardian, has, for prolonged periods, provided us with our main sources on both Adenauer and Brandt. Adenauer has never received an overabundance of biographical attention, and Brandt is just now beginning his career as biographical subject. Second, and much more important, this biography provides an all-too-perfect example of work which makes those of us in the discipline who are interested in developing a "scientific" approach to the study of political leadership, despair. In particular, Prittie's book serves as an unwelcome reminder that academicians eager to investigate the contemporary political leader are almost inevitably stuck with impressionistic materials submitted by random observers, or with a book such as this, which pretends to be a life history but which is, in fact, little more than a life chronology and a flawed one at that.

Far be it from me to chastise Prittie for all the sins of his kind. He never claims social scientists (or historians) as his intended audience. Is it possible then, that were we to shed our guise as members of the academy, we would find a new virtue here? Not likely. Indeed, by posing the question, we pinpoint the methodological and substantive flaw: a biography such as Prittie's is written without any clear audience in mind and is, as such, a bastardized form. This book, for example, is quite carefully footnoted and presumably aspires, therefore, to a certain respectability among scholars. Regrettably though, it lacks a complete historical accuracy as well as a social scientific exploration of causality. The lay person is offered no compensation; the literary style is so pedestrian that the reader is denied even a rudimentary aesthetic and/or vicarious pleasure in perusing the story of one of the most strikingly exciting lives in modern political history.

Prittie has taken his cues from Brandt's own autobiography (My Road to Berlin [London: Peter Davies, 1960]). That is, the tone of his book and the materials included pick up from Brandt — not from where Brandt left off. (In the case of personal and professional developments after 1959, Prittie continues the pattern of elaborating to the same limited degree and in the same cautionary mode.) The effect of this submission to subject is that the biography suffers from the same distortions, inclusions, and omissions as the autobiography. But, of course, what is tolerated as an inevitable subjectivity in the autobiographer is not excused in the supposedly more objective life historian.

For instance, Prittie allots precisely sixteen

pages to the first nineteen years of Brandt's life. (Brandt had spent thirty on approximately the same span.) The twenty-eight years between 1945 and 1973 are given 249 pages. (About 110 of these cover his life as a "statesman.") This absurd imbalance occurs not only because access to information about Brandt's life after 1945 is relatively easy to obtain or because this more recent history is virtually harmless in terms of contemporary German politics. It also indicates the author's failure to shed light on those aspects of Brandt's history which are more controversial with regard to his personal life. Nor does he even begin to explore the private motivations underlying Brandt's public behavior. The effect of this abdication goes beyond stretching our patience with omission; we are forced to cast doubt on that information with which we are provided.

Cases in point from the early years: (1) We are told that Brandt "became an active participant in discussions and plans" (p. 23) for the International Bureau of Revolutionary Youth (1934). We are not told about the role Brandt continued to play in this organization or about the effect that the ensuing debacle had on his subsequent political development. (2) We are told that Brandt went to Spain in 1937 "primarily as a correspondent of Norwegian and Swedish newspapers" (p. 32). We are not told that the primary reason for his Spanish stay was political (he was to promote coordination of the parties of the Left), and the important pamphlet Brandt wrote on this subject does not receive a single reference. (3) Prittie mentions, in passing, the name of Jacob Walcher as SAP colleague. But, almost certainly because Walcher settled in East Germany after the war, he does not tell us of the major impact Walcher had on Brandt circa 1933-39.

Prittie does offer a quite accurate account of Brandt's professional life and progress after 1948, and he devotes some care to placing him within the broader political environment — i.e., within the German, European, and East-West settings. But beyond that, his book does not illuminate nor fully inform. In spite of the author's affective bias and because of his refusal to probe beneath the event, the account remains bloodless. If the book has any importance at all, it is by default. Where do researchers, teachers and students turn for single case analyses of subjects with manifest political impact — subjects who are not already six feet under?

BARBARA KELLERMAN

Tufts University

Transition in Africa: From Direct Rule to Independence. A Memoir. By Sir James Robertson. (New York: Barnes & Noble Books, 1974. Pp. 272. \$15.00.)

Sir James Robertson was one of that elite corps of colonial administrators - an Oxford graduate with a second in "Greats" and a Blue in rugby - who ruled the British Empire with a minimum of fuss and expense and a great deal of integrity and skill. He entered the Sudan Political Service in 1922 and stayed there until 1953, just two years before the Sudan became independent. He served for almost twenty years in the provincial service and then rose to be Civil Secretary, the senior officer in the S.P.S. In 1955 he was appointed Governor General of Nigeria and saw that country through to independence in 1960. Thus his career spanned the period of colonial trusteeship to independence when power was transferred to Africans.

The District Officer — that jack-of-all-trades — was the key to good government. Sir James vividly portrays all aspects of the D.O.'s work — anthropologist, builder, policeman, judge, councillor, tax collector, animal and agricultural expert. The interwar period was probably the golden era of colonial governance, falling as it did between the harshness of the pioneer days and the clashes with African nationalists after World War II.

The political process whereby Africans in the Sudan and Nigeria got Great Britain to waive its rule is clearly charted by Robertson. The key was to maintain the cooperation of Africans while avoiding precipitous withdrawal or using undue force to remain. How new states were formed out of colonies by colonial officials and indigenous leaders is essentially the subject matter of this book. Robertson provides much information on his difficulties over policies in the southern Sudan from 1945 to 1953 and in forming Nigeria's three regions into a federation.

This is a good study and a personal document by one who ruled in Africa and helped transfer sovereignty to Africans.

PETER DUIGNAN

Hoover Institute

Authority, Participation and Cultural Change in China. Edited by Stuart R. Schram. (New York and London: Cambridge University Press, 1973. Pp. viii, 350. \$17.00, cloth; \$4.95, paper.)

What was the Cultural Revolution of 1966-69? What were the causes of its emer-

gence and development? Was it a power struggle among the top leaders, or a massive rebellion of the youth against the authority of Party and government? Was it a violent clash between the opposing ideologies, a struggle for domination among key institutional forces, or an inflamed confrontation between policy alternatives and development strategies?

Today, even as the turmoil and violence of the movement have long subsided, the answer to these basic questions, and their practical significance and theoretical implications for China's political process and social transformation are still being vehemently debated among students of contemporary China. The field has benefited in the last several years from a number of solid works on the Cultural Revolution by competent scholars like Richard D. Baum, Robert J. Lifton, Lowell Dittmer, Edward E. Rice, and Thomas Robinson. These works have indeed contributed greatly to our detailed knowledge of the dynamics of power politics and ideological disputes. Yet few studies have focused specifically and systematically on the question of concrete policies and their impact on the course of development after the Cultural Revolution.

Hence, we are indebted to the publication of the volume under review. Representing a significant departure from the over-stressed perspective of power politics, the volume addresses itself primarily to the question of policy innovations and organizational changes during and after the Cultural Revolution and relates these to the problem of leadership and participation in China's social change and economic development. A major objective of the book, as the editor puts it, is to answer the question, "what has happened to China as a result of the Cultural Revolution?" (p. 1).

The volume is a product of the efforts of a group of European scholars concerned with contemporary China, through a series of research conferences held in 1971 and 1972, organized under the energetic leadership of Stuart R. Schram, then director of the Contemporary China Institute at the School of Oriental and African Studies in London. The eight contributors are all scholars of impeccable qualifications, competent both in the subject matters and in the Chinese language. Being Europeans, some have even enjoyed the special advantages of being able to work for an extended period of time in the People's Republic of China.

This volume consists of six topical essays by different authors with an editor's introduction. The 108-page long introductory essay by Stuart R. Schram illuminates the book from the very

beginning with a penetrating analysis of the key variables of contemporary China's political process and revolutionary movements, such as cultural traditions, competing ideologies, policy choices, development strategies and dominant political personalities. The Introduction also presents a masterful synthesis of the interplay of these variables from a historical perspective, which places the Cultural Revolution in the broad context of modern China's turbulent search for national survival and development. With rare eloquence and skill, Schram demonstrates that Mao as a political genius has not only had a sophisticated grasp of the forces at work in Chinese society but has successfully manipulated such forces with imagination and courage for the attainment of his revolutionary goals. Moreover, with new rigor and fresh insight Schram raises the fundamental question of the extent to which China under Mao's leadership can continue to borrow Marxist-Leninist and other Western "techniques" (yung) of organization and modernization and adapt them to serve China's needs for development without eroding what Mao perceives to be the very "essence" (t'i) of the Chinese revolution. This was, of course, the central question that the political reformers of modern China asked themselves a century ago. Interestingly enough, the very same question is again asked today by political leaders and students of politics alike, who are concerned with the processes of modernization and political development in the Third World.

The six topical essays collected in this volume, with the exception of Jack Gray's, focus on the introduction and development of policy innovations and organizational reforms during and after the Cultural Revolution. Unfortunately, the subject matters treated lean disporportionately toward the economic sector, probably because of an uneven distribution of research interests among the participants of the project. Four out of the total of six articles fall into the realm of economic planning and management. Aside from Jack Gray's historical survey of the development of the struggle between the "two lines" with special attention to agricultural mechanization, Marianne Bastid emphasizes the trend toward what she characterizes as "decentralization" of economic initiatives and decision making at various levels and units; while Jon Sigurdson treats the increasing transfer of technology to the rural sector for a more balanced pattern of modernization; and Christopher Howe deals with changes in labor organization and the incentive system in the urban sector. In the last two essays, the far-reaching reforms instituted in the educational system are taken up by John Gardner and Wilt Idema, and the impact of the cultural revolution on social relations in selected sectors of population is examined by Andrew J. Watson with emphasis on attitudes toward authority and social participation.

While analyzing problems from different perspectives, conceptual frameworks and data bases, the authors seem to have reached a consensus that the basic policy and organizational lines introduced through the Cultural Revolution are by no means new and unfamiliar. All of them can easily be traced back to Mao's previous political experiences and theoretical writings. But the magnitude of the efforts to implement and sustain the innovations clearly exceed those witnessed in the past. The writers also agree that the impact of the Cultural Revolution varies from sector to sector. Bastid, Sigurdson, and Gardner conclude that the innovations in the areas of agricultural development, rural industrialization, and educational revolutionization are bound to have a far-reaching and positive effects on their future development, while Howe and Watson believe that the effects of radical changes pushed during the Cultural Revolution in the urban industrial sector and in social relations are at most mixed and may be of only short-range significance.

As the dust of the Cultural Revolution continues to settle and the new pattern of organizational behavior and policy orientations takes on a more identifiable trend, a definitive assessment of the impact of the Cultural Revolution will undoubtedly become easier. But, for now, this volume has clearly distinguished itself as a pioneering work in the analysis of the strategy and trend of development of the post-Cultural Revolution period. No student concerned with contemporary China can afford to overlook the stimulating analyses and insights offered in this volume.

MICHAEL Y. M. KAU

Brown University

The Wartime Alliance and the Zonal Division of Germany. By Tony Sharp. (New York: Oxford University Press, 1975. Pp. 220. \$19.25.)

Drawing upon Foreign Office and Cabinet papers that have recently become available to scholars, unpublished or unused American documents, and the recollections of participants in the events under examination, Tony Sharp has presented a meticulously detailed account of "how between 1943 and 1945 Hitler's Reich and Berlin its capital were di-

vided respectively into four zones and sectors" (p. 1).

In his book Sharp focuses on two themes: the planning for and negotiations that led to Germany's zonal division, and the military context within which this took place. Although much of the information on these events has long been common knowledge, "the interaction between negotiations and military strategy has not been presented before" (p. 1). The author's basic thesis is that these negotiations can only be understood if viewed in terms of their military context. Thus it is these interrelationships between military and diplomatic developments that provide the ordering framework for the book.

The book begins with a summary overview of the general relationship between the course of negotiations on the zonal protocol and the military situation before and at the time of Yalta. Sharp then examines in detail interallied planning for the postwar order of Germany beginning in late 1942, on through the Yalta Conference, to the withdrawal of British and American forces back to the agreed upon zones of occupation, the setting up of the Allied Control Council and the entry of Western troops into Berlin in July of 1945. Except for the treatment of the tripartite negotiations in the European Advisory Commission, the study focuses on the development of the British and American (and later the French) positions. Here interesting and new material is offered on the disputes between Washington and London over occupation of the north-west zone (with its North Sea ports), control of the Bremen-Bremerhaven area, and other policy disagreements. In a concluding chapter, a comprehensive discussion of the extensive negotiations over the French zone of occupation and the several proposals for the allocation of the sectors in Berlin is presented.

Sharp argues that the planning and negotiations on the different proposals for the zonal division were colored by an "awareness that the nature of the postwar order would be largely determined by the respective position of the Anglo-American and Russian armies at the end of the war" (p. 3). He examines in detail how the momentary military situation and the projected course of future operations, as well as the perceptions of what the Russians were likely to do formed the background for the Western allies' planning and negotiations. He shows how these factors influenced the positions taken as the allies sought to consolidate their hold on what they felt they were likely to get, as well as to guarantee an appropriate share of what the other side might capture. In this

sense, the protocol on the division of Germany into zones of occupation was seen as "legal insurance" against a major conquest of Germany by one side or the other.

In the chapter on the allied advance into Germany Sharp points out how the protocol itself was an important political factor in the debates on military strategy between the American and British, e.g., the advance to the north, the desirability of retaining the capture of Berlin as a primary military objective, and the link-up with advancing Russian forces. The presence of Western forces in parts of the Russian zone of occupation at war's end seemed, particularly to the British, to offer an opportunity to renegotiate parts of the agreement and to resolve other outstanding issues between the West and the Soviet Union. Here Sharp discusses the significant disagreements between the British and the Americans as to whether and how the timing of withdrawal should be linked to attempts to bargain with the Soviets.

Although many of these events have been treated separately before, Sharp points out that until now "all the aspects of zonal division have never been dealt with together" (p. 2). In addition, much that has been written about in general outline before has been "supplemented and clarified by a wealth of detail" (p. 2). And this "wealth of detail" is both a strength and weakness of the book. Such an abundance of material is often hard to digest, even though the interweaving of the themes of negotiations and military context provides the reader with some point of orientation for moving through the information presented. Still the macro frame of reference is not enough to order the myriad relationships and interactions among the different planning bodies and individuals that continuously enter and leave the study at the micro level. It is when trying to keep the multifarious actors apart and to thread through the proposals and counter proposals that a reader may wish Sharp were less the compleat historian and more an analyst of the decision processes themselves through which the protocol was shaped.

With this book Sharp has given us a comprehensive and exhaustive account of an important set of policy decisions and of some of the factors that influenced them. As such this study is more suited for reference than for general reading or classroom use. Its greatest utility would seem to be as a source of information for more theoretical or general analysis of these events and the complex interactions among institutions and personalities out of which the plans for military operations and the diplomatic

moves emerged.

KENNETH HANF

International Institute of Management, West Berlin

The Lions of Dagbon: Political Change in Northern Ghana. By Martin Staniland. (New York: Cambridge University Press, 1975. Pp. 241. \$22.50.)

Martin Staniland's book is a political history of the Dagomba people, a group of almost 300,000 people spread over 8,000 square miles of savannah in northern Ghana. The study focuses upon the position of the Ya-Na, the paramount chief of the Dagomba. Although not stated explicitly in the book, one might conclude that three factors have led Staniland to study the paramount chief of the Dagomba, as opposed to the chiefs of other Ghanaian societies. First, Dagomba society has preserved much of its traditional character, and the position of the Ya-Na, while very different from what it was in precolonial times, demonstrates much more historical continuity than paramount chieftaincy in other parts of Ghana, where the forces of modernization have exerted themselves more powerfully. Second, the rules governing the selection of the new Ya-Na are complex and intriguing, and consequently the process of succession constitutes the core of the study. Third, violent disputes costing many lives have occurred in recent years over who should hold the position.

Professor Staniland points out that the process of succession, like other facets of chieftaincy, has been buffeted by a series of disruptive and threatening forces over the past seventy years. First British colonial administrators strove to adapt the Ya-Na's position to varied and shifting models of colonial rule. The theme of indirect rule prevailed, but controversies raged about what indirect rule meant for Dagomba. Colonial administrators could be classified into two groups: "For one school, efficiency was the final test; for the other authenticity. The paternalism of one could not, in the last resort, be reconciled with the traditionalism of the other" (p. 82).

Additionally, according to Staniland, new demands placed upon the Ya-Na and his subordinate chiefs to promote modern education and economic development and fit their kingdom into the frame of the emerging nation-state of Ghana, cast doubt on the appropriateness of the rules of succession. These rules accorded prime importance to royal blood and longevity, with the result that many Ya-Nas were senile by the time they attained the office.

The traditional patterns were also challenged by the inevitable involvement of Dagomba in national politics. Forces in conflict at the center supported the various disputing lineages in Dagomba. The winners at the center rewarded their supporters at the Dagomba periphery by altering the rules of succession so that their man became Ya-Na. This history of central government intervention has meant that "the proliferation of government measures defining, regulating, and protecting chieftaincy signified a further degradation of the political status of the institution. Its formal status was preserved as its major functions were removed" (p. 132). The central authorities (successively colonial, parliamentary, one-party, and military) have demonstrated their ineptness in comprehending and settling questions of political succession in this traditional polity.

The study is full of historical detail, and if the reader is able to compare the Dagomba experience with other traditional polities in Ghana and elsewhere, the book provides the basis for many insights. One wishes, however, that the author had provided more reflection and comparison. Moreover, when he does offer generalizations, he sometimes treats Dagomba as if it were all of Ghana. For instance, in a concluding remark, Staniland asserts that the book has demonstrated the "resilience and vitality of chieftaincy and politics in Ghana" (p. 175). Rarely, however, does he make reference to other parts of Ghana. The utilization of comparative material would probably have led Staniland to note the unusual resilience of chieftaincy in Dagomba, rather than its typicality. Nevertheless, Staniland's understanding of Dagomba is very considerable, and the intelligence with which he handles his data makes this book significant for those interested in the political history of traditional polities in Africa, and the transformation they have undergone as they interact with various types of national governments.

DAVID R. SMOCK

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Death of a Utopia: The Development and Decline of Student Movements in Europe. By Gianni Statera. (New York: Oxford University Press, 1975. Pp. 294. \$10.95.)

The main thesis of this book is that the student protest movement of the 1960s was "an original phenomenon of collective behavior involving young people," shaped largely by a set of utopian ideas. Through a careful and detailed analysis of the student movements in

Germany, Italy, and France and informative illustrations from other European student movements the author shows that this set of chiliastic and utopian ideas (à la Karl Mannheim) greatly appealed to vast numbers of students living and studying in old, hierarchic, autocratic, overcrowded, and understaffed institutions. A situation of mass action was consequently created and was greatly responsible for the initial great success of the protest. However, as soon as utopia was replaced by ideology, that is to say by dogmatic Marxist-Leninist ideology and centralized and disciplined action, the "Betrayed" by their movement declined. leaders, the students refused to avail themselves for further action and brought the movement to its abrupt end.

The use of the concepts of utopia and ideology to denote the two historical phases of the European student movement is based of course on Karl Mannheim's sociology of knowledge, and one of the virtues of Statera's study is that this is done very convincingly. These concepts help to focus our attention on the unreducible political character of the groups concerned and enable us to understand not only why a large number of young people were available for mass action, but how did it happen that specific chiliastic ideas, developed by Herbert Marcuse, Rudi Dutschke, Daniel Cohen-Bendit and others have come to play such an important role in the protest as motivating symbols for action. Most of these ideas have thus far been considered by students of traditional political philosophy and by social critics as unsystematic ergo unimportant. Statera's analysis makes a significant contribution to the correction of this approach and his theoretical arguments are satisfactorily backed by rich documentation and detailed historical accounts. His emphasis on the political and ideological dimensions of the students protest does another important service for the students of the protest phenomenon. It helps them to understand the intense fragmentation of many of the "groupuscouls" (small groups) and the splits among student groupuscouls, leftist parties, and adult organizations. The result is that the reader of the book is provided with a study which is both very informative in its content and properly theoretical in its form - not a small achievement in comparison with other existing studies.

To be impressed by many of Statera's accomplishments in this book is not, however, to agree with all his arguments. I would like here to take an issue with the main thesis of the book, regarding the decline of the student movement. The decline is seen by Statera to be

a result of the replacement of the creative utopian thinking by the dogmatic ideological one. What we have here, in my opinion, is a classical case of the post hoc ergo propter hoc fallacy. What Statera skillfully demonstrates is that toward 1969 there was (1) a marked decline in the intensity of protest, (2) a significant decline of the utopian elements within the protest movement, and (3) a significant ideological transformation of many of the groupuscouls in a more dogmatic and ideological (traditional Marxist-Leninist) direction. Nowhere does he show however that the decline of the intensity of protest was a result of the other two developments. For such a proposition to hold. Statera should have provided us either with one empirical case where continuing utopian zeal had retained a high level of intense protest for a longer period of time or with an empirical example in which an early "ideological" takeover prevented an intense protest from occurring. He does not perform these necessary operations for a very simple reason: such examples do not exist.

A close examination of the students protest movements in Europe would show that among many of the groupuscoules, "ideologization' followed the failure of the spontaneous protests to accomplish major revolutionary goals and was a direct result of this failure and of the consequent decline of the protests. What actually happened was that after the decline of the protest activities, many of the radical activists came to the conclusion that a serious revolution could not be carried out through the spontaneous and nondisciplined action of students and would be conceivable, if at all, only through the well-planned activity of disciplined revolutionary organizations appealing to the workers. This, of course, led them to the rediscovery of Lenin's lessons. The fact that this rediscovery has brought about the unfortunate rejection of some of the creative elements of utopia cannot be denied, but its depiction as a "betrayal" of the movement and as the cause for its decline is unjustifiable and untenable.

In spite of its strength and appeal, Statera's analysis lacks a whole structural dimension essential to the understanding of the protest movement of the 1960s, i.e., the process of its delegitimization. The really important transformation that the movement has undergone has not been from utopia to ideology but from a moderate opposition to the powers that be, to an intense rejection of their legitimacy, expressed by new antiregimist symbolic language and by sheer violence. Only the very active elements within the movement were ready to bear the full consequences of the crisis of

legitimacy that ensued, the repressive means applied by the governments concerned, the growing resentment of the population, and the consequent need to go underground with no hopes for immediate success. Most of the protesting students were never revolutionary. were exhausted by the experience of protest, and became satisfied with the academic reforms achieved as a result of their earlier activity. They refused to respond further to groups such as La Gauche Proletarienne in France or Baader-Meinhoff in Germany. But had these groups remained utopian, had they retained all the creative elements that Statera identified within the earlier stages of the protest it would have made no difference.

EHUD SPRINZAK

Hebrew University of Jerusalem

Protest and Response in Mexico. By Evelyn P. Stevens. (Cambridge, Mass.: The Massachusetts Institute of Technology Press, 1974. Pp. vi, 372. \$17.95.)

With this work, Professor Stevens not only joins the growing ranks of scholars critical of the ruling elite in Mexico, but offers one of the most devastating commentaries about the contemporary Mexican political scene that has appeared in recent years. The author's principal contention is that Mexico is ruled by an authoritarian regime which, in contrast to democratic or totalitarian arrangements, is characterized by lack of a well-defined ideology, an absence of political mobilization, and limited pluralism.

Dr. Stevens proceeds to show that there is a remarkably predictable pattern in the Mexican government's response to protest movements that do not bear the official seal of approval. After a brief period of confusion and indecisiòn, which she attributes to a temporary failure in communication channels and to the excessive dependence of Mexican bureaucrats on the ultimate problem-solving initiative of the president, the government characteristically evades real negotiation, although it might at first voice promises to "study" the protestors' demands. These are only "half a carrot" promises, however, which the government never really intends to keep. Instead, the authorities invariably embark upon a massive campaign aimed at discrediting and dividing the protest groups, and at penalizing the leaders of such

While the author acknowledges that the intensity of the government's actions varies from situation to situation, depending on the

nature of the demonstrations, she nevertheless maintains that variation in response is relatively minor, and then mainly in determining what "justice" shall be meted out to key offenders. Measures could range in severity anywhere from blacklisting, exile, and imprisonment, to kidnapping or even more violent types. In every situation, however, the authorities will enlist the support of a servile legislature, a largely unprofessional press, and government-sponsored unions in their confrontations with protest groups, whose bona fide spokesmen are depicted by the government as Communist agitators, foreign agents, or even Mexican traitors.

To substantiate her findings, Dr. Stevens avails herself of three well-publicized cases of protest against the Mexican regime: the railroad workers' strike in 1958-59; the doctors' strike in 1964-65; and the student revolt prior to the 1968 Olympics. She concludes that in each of these situations the regime's response was repressive, and that "in trying to deal with the new kinds of protest which arose in the 1960s, the regime did not become either more democratic or more totalitarian; it became more authoritarian" (p. 13).

On the whole, Professor Stevens's analysis is insightful, as she is able to get to the underlying truth of much of what goes on in Mexican official circles. While she does come to the right conclusions, she does so not because of methodological sophistication, but because she is both intuitive and clearly familiar with the Mexican character. In fact, the principal flaws in this volume are methodological, and they regrettably detract from some of the author's main arguments.

Most notable is her failure to exploit fully her field and survey research, which she asserts was conducted in Mexico in 1965-66 and later in 1968, and included more than two hundred interviews of persons who participated in the strikes. There is very little evidence, either in the text or in the footnotes, that she actually utilized these resources in any meaningful or systematic manner. For example, the reader learns that Professor Stevens is a frequent visitor to Mexico only by means of occasional references, such as: "a series of frustrating experiences in 1973 involving out-of-order telephone lines, convinced the author that telephone communication is still a somewhat hazardous venture" (p. 59). She was apparently not an on-the-scene observer of the strike demonstrations; most of her references are drawn from press releases (whose reliability she had already questioned), previously published accounts, and the testimony of respondents

(which she admits obtaining in some instances many years after the subjects had been involved in the protest movements). Can Dr. Stevens attain her avowed goal of describing the behavior of strikers and government officials, rather than their attitudes, without having been there in the midst of things?

Similarly, it seems hardly worth the effort to conduct so many interviews when the author is unwilling to produce any systematic results of her findings. Instead, she is merely inclined to generalize about her respondents, with typical asides such as "two of the physicians interviewed for this study assured the author repeatedly that their phone wires had been tapped during the strike period" (p. 61). Did she not submit a standard questionnaire to the physicians she interviewed? If she did, shouldn't she tell the reader what percentage shared the view expressed by the two stated in the author's own words: physicians? Also, why does she consistently refer to her respondents as "informants" when she has explicitly expressed reservations about previous studies on Mexico in which "too much effort has been expended in trying to force the square peg of political reality into the round hole of previously formulated theory?" (pp. 10-11). Granted that Mexican's regard revelations to foreigners of the activities of their government an unpardonable indiscretion; yet the choice of label for her respondents does seem to betray biases which she herself holds.

Despite these serious problems, this is, as noted earlier, a very provocative study. The author's examination of many aspects of social custom that have a direct bearing on the informal character of government activities in Mexico is, to say the least, most revealing, as is her account of the subtleties of what she terms "communication, Mexican style" - a process understood by Mexicans, but seldom by perplexed foreign observers. From all indications, Professor Stevens is an admirer of the work of Mexican poet Octavio Paz. While her own contribution is no Labyrinth of Solitude, hers comes as close to it in some respects as any other work by a non-Mexican. This, in itself, should be enough to forgive her for any "sins" of omission or commission.

FRANZ A. VON SAUER

Oklahoma State University

Land Reform and Politics: A Comparative Analysis. By Hung-chao Tai. (Berkeley: University of California Press, 1974. Pp. 586. \$25.00.)

This comparative study of the land reform

process in eight nations (Taiwan, the Philippines, India, Pakistan, Iran, Egypt, Colombia, and Mexico) provides an excellent summary and empirical assessment of the major political issues involved in the redistribution of land. Tai's work is theoretically and conceptually unpretentious. The questions he asks are those with which policy makers are most concerned - e.g., what are the political and economic consequences of agrarian inequality; which land reforms are most successful in terms of equity and productivity, does land reform produce political stability, what kinds of political coalitions are most effective at carrying out land reform? In the course of answering these and other questions, Tai presents the reader with a host of statistical and qualitative comparisons that are very illuminating.

The major hypotheses can be succinctly

1) The perception of the need for legitimacy prompts the political elite to initiate land reform; 2) the relationship between the initiating elite and the landed class determines the manner of program formulation and content of the adopted program; and 3) the political commitment decisively affects the extent of program implementation (p. 468).

Some of these hypotheses may seem self-evident or even tautological, but they are not. For example, in discussing the second proposition, Tai convincingly discounts the role of peasant mobilization in most land reform programs and shows how the autonomy of the elite from landed interests is often decisive. There was little popular clamor for land reform in either Taiwan or Iran. Instead it was the Kuomintang's need to build a peasant political base and the Shah of Iran's need to break the hold of aristocratic landowners which made extensive land distribution a desirable political goal. Tai concludes that autocratic governments which can move quickly are, other things being equal, more capable of successfully executing a land reform program than are parliamentary systems such as Colombia and, until recently, India and the Philippines, where the electoral power of landowners can generally block any but the most token reform programs.

After dealing with the origin and implementation of land reform, Tai moves, in the second portion of the book, to consider the impact of land reform on rural political participation, national integration, institutionalization of rural organizations, rural stability, and the appeal of communism. As one might expect, he concludes that land reform "has a definite and positive contribution to make to these aspects of political development" (p. 468). It is, to be

sure, a rather gross intrusion of ideology to assume that whatever diminishes the appeal of communism is a contribution to development. The reader, however, may dismiss the author's prejudices and still profit from Tai's discussion and argument.

Tai's analysis of the perennial question of the relationship between distribution and productivity will be of interest to many readers. It is a commonplace that productivity may be expected to fall in the immediate aftermath of a land reform. The data, however, fail to support this conclusion and suggest, in fact, that productivity more often remains constant or even rises. Although the revolutionary turmoil that may accompany the struggle for reform may diminish productivity, these effects are not attributable to the reform itself. Tai does document the impressive and nearly universal tendency for productivity (both in terms of output per unit of land and output per unit of labor) to increase greatly in the years following a thorough reform. His case would have been somewhat weakened by the inclusion of such deviant cases as Burma, but I believe his general analysis is convincing. Finally, Tai somewhat more speculatively suggests that, in the long run, the proliferation of small family farming units may eventually work against productivity gains. Evidence along these lines comes largely from the comparison between the ejido and non-ejido sectors of Mexican agriculture. In any event it is clear that new institutional arrangements are often necessary to allow small household farms to share the cost of lumpy inputs and to market cooperatively if they are to remain viable.

It is regrettable that Tai should have borrowed the most simplistic form of relative deprivation theory as the basis from which to argue the explosive consequences of agrarian inequities. There are by now convincing logical and empirical reasons to doubt the connection between some "want-get gap" deduced from economic statistics and stratification, on the one hand, and the radical mobilization of the peasantry on the other. Without examining the structure of the peasantry, its social organization, and its political history, as well as the role of the landed class and the state, it is clearly impossible to claim that rural inequalities lead in any simple way to revolutionary radicalism. If rural exploitation were alone sufficient to generate rebellion, most of the Third World would be in flames. One senses that Tai wishes to convince conservative elites that they must reform or be overthrown. The evidence, alas, should suggest to Tai as well as to those who took the Alliance for Progress ideology seriously, that reactionary regimes have generally contrived to reject reform and to employ other means of remaining firmly in power.

One major conclusion which emerges from Tai's analysis - a conclusion which I find as chilling as he finds encouraging - is that a reformed peasant sector can offer a conservative regime the kind of social base which can then be deployed successfully against urban and middle-class dissidents. One major effect of an extensive land reform is to remove the landed class which hitherto mediated the relations between the peasantry and the state and thereby to allow the state directly to penetrate the countryside. Typically, the state or ruling party then establishes a network of peasant organizations through which the beneficiaries of reform become dependent on, and identified with, the official bureaucracy. Once the peasant receives his land it is likely that he and his class will willingly, even enthusiastically, accede to the corporatist plans of the new elite. Tai goes so far as to suggest that it is cheaper to buy the compliance of the peasant through land reform than to deal with the insatiable demands of the urban working class (p. 437). Once assured of peasant support or quiescence, the elite can then beat back the more radical threats of students and workers in the cities. This conservative postreform coalition is not just an abstract possibility, for one need only look at Mexico, Egypt, Iran, and Taiwan to appreciate how land reform can demobilize the peasantry and work against further structural change. Looked at from another perspective, therefore, land reform may constitute the keystone of a counterrevolutionary strategy.

JAMES C. SCOTT

Yale University

The Future of Democracy in Latin America. By Frank Tannenbaum. Edited by Joseph Maier and Richard W. Weatherhead. (New York: Alfred A. Knopf, 1974. Pp. 246. \$10.00.)

This book commemorates the late Frank Tannenbaum's thirty years of scholarly contributions as an outstanding Latin Americanist and professor at Columbia University. The two editors have accomplished this goal by writing a forty-five page biographical essay, followed by their selection of thirteen essays, both published and unpublished, to illustrate his approach to the understanding of Latin American problems.

The biographical essay on Frank Tannenbaum is exceptionally good since it consistently evaluates how his life experiences affected both his philosophy of knowledge and his belief about how a scholar should operate. The editors, both former students of Frank Tannenbaum, have looked at his life closely and empathetically present his scholarly outlook. They show in detail how his experience as a labor organizer in New York City and his consequent sentencing to a year in jail kept him from becoming a radical ideologist and helped him recognize the limitations of institutions and human beings. His later writings on labor unions, penal reform, and Latin America reflected these insights.

The next part of the biographical essay describes his especially close relationship with Columbia University, including the intellectual stimulation he derived as a student, his teaching approaches, his love for the University, and the special weekly seminars he started on Latin American problems that became a permanent fixture. The editors evaluate these seminars to illustrate how he tried to break down the traditional barriers among the academic disciplines by inviting persons from labor, business, government, other universities, and Latin America to thoroughly dissect a problem for the purposes of gaining understanding, the use of different approaches, and perspective.

Professors Majer and Weatherhead's description of Frank Tannenbaum's interest and research on the Mexican revolution fills out the remainder of the biographical essay. The editors describe his travels in Mexico, his personal friendship with Lazaro Cardenas and other leaders, and the value he placed on such personal knowledge for research. They also discuss his general attitude toward the accomplishments and obstacles of the revolution. The central themes of his three books on Mexico are then briefly mentioned. Greater elaboration of these books would have been valuable, especially in evaluating the originality of their research findings and in analyzing how well they have stood the test of time.

The editors describe Frank Tannenbaum as a social philosopher. This is an apt characterization and would seem to mesh with what he himself recommended in one of the papers as proper training for social scientists in the Latin American field. He recommended that students have a good grounding in literature, cultural anthropology, and social psychology in order to understand the culture, people, and the inner basis of political and institutional life. Of equal importance was a minimum of two years of travel in several countries, developing personal contacts, and observing especially life outside the cities.

The thirteen papers in the book are a motley

group, consisting of four unpublished essays, and articles that have appeared in Foreign Affairs, The Year Book of World Affairs, Foro Internacional, The Hispanic American Historical Review, Political Science Quarterly, Estudios Antropológicos, and the Journal of World History. No excerpts from Tannenbaum's books on Mexico have been included, on the ground that the editors felt that the lesser-known essays deserved a larger readership. Most of the essays illustrate the special scholarly approach Frank Tannenbaum used to dissect problems. Unfortunately, the editors have provided no introductions to the essays and have not grouped the essays in any coherent way. Thus one has constantly to refer to points made in the biographical essay to provide a linkage. The Latin American specialist can do this on his own, but it may be much more difficult for the general reader.

The articles fall into different types. Three are on the obstacles to and the conditions which facilitate political stability in Latin America. In these the problems of personalism, caudillos, transfer of power, political parties, local government, political culture, and the difficulty in finding a source and symbol of legitimacy are brought out. These articles will be of value for the general reader for a first dip into the area. The specialist will find some insights, and can react one way or another to the importance given some ideas, such as that strong local rural communities must constitute the base of democracy. Those who insist on rigorous conceptual research designs will not be satisfied with these articles, and the analysis is more appropriate for the dictatorial politics of the 1950s than for recent happenings.

I found three articles outstanding in opening up new ways of approaching a problem. The best is an extended logical dissection of Arnold Toynbee's claim that Latin America must choose between economic efficiency and social justice. Tannenbaum argues that the dichotomy is false and meaningless, and he adduces evidence from Latin American politics. In an article on adaptation in the Mexican revolution, the author draws on his knowledge of Mexico to stress the pragmatic and nonideological approach that was used by the many leaders to tackle problems as they came up. These two articles belong together since Professor Tannenbaum insists that the turbulence and unpredictability of Latin American politics leave no time to discuss rational ideological alternatives. The third article on the incorporation of the sizable Indian communities in many Latin American countries is provocative since the author argues that real governmental progress in

this area could very well lead to the surfacing of long-standing Indian hatreds and to the emergence of separatist dissident nationalistic movements.

ROBERT D. TOMASEK

University of Kansas

China, the Struggle for Power, 1917—1972. By Richard C. Thornton. (Bloomington: Indiana University Press, 1974. Pp. 403. \$15.00, cloth; \$3.95, paper.)

This book on Chinese politics is divided into three parts in chronological order. Part I deals with the origins and development of the Chinese Communist movement and covers the years 1917-41. Detailed are Soviet Russian support and interference in the Chinese Communist movement, power struggles among the Chinese Communist leaders, and the Kuomintang-Communist rivalry. A part of this section is based on the author's earlier book The Comintern and the Chinese Communists. 1928-31 (Seattle: University of Washington Press, 1969). Part II covers the period 1941-49 and is entitled "The American Experience in China." The struggle between the Communists and the Nationalists in China in this period is seen as a part of the global power struggle. As this struggle gathered momentum towards the end of World War II, it upset the Roosevelt-Stalin design to fashion a postwar balance of power in the Pacific, a design requiring a unified China under the KMT leadership, strong enough to keep domestic peace, but not so much as to challenge the authority of the two big powers.

Part III covers the period 1949-72, from the founding of the Chinese People's Republic, through the turmoil of the Great Proletarian Cultural Revolution, to the demise of Lin Piao. The genesis of the Cultural Revolution is traced to the CPR's program for China's industrialization and to the realization that such a program must be independent of Russian control and manipulation. In this light, the Great Leap Forward and the Sino-Soviet split led directly to the Cultural Revolution; the Sino-Soviet split had caused polarization of Chinese leaders to such an extent that Mao had to launch the Cultural Revolution to fortify his positions. Finally, considerations of China's defense needs, particularly along her northern border, brought the Cultural Revolution to an abrupt halt in late 1968. The Chinese now regard the presence of the USA in the western Pacific as a deterrent to a precipitate Russian move into Peking; after the Russian intervention in Prague in 1968, this spectre has been haunting the Chinese.

Professor Thornton has interpreted a number of historical events from new points of view. Emphasizing the early Chinese Communist movement as a part of the worldwide revolutionary conspiracy, he reconstructs Mao's difficult and protracted rise to power inside the CCP. According to him, Mao was opposed to the United Front as late as August 1937. The United Front, a Russian-designed strategy, was then being implemented in China by the Russian-returned group, the very group Mao had to discredit. Mao embraced the strategy as his own only after he achieved top leadership. Thus, Thornton puts Mao's ascendancy to top leadership at sometime after August 1937.

Also, Thornton sees the New Fourth Army incident as an instance of intraparty struggle between Wang Ming and Mao, while other scholars generally treat the incident as a case of KMT-CCP interparty struggle.

Concerning U.S. involvement in China, Thornton questions the efficacy of the activities of some junior officers of the U.S. Foreign Service in wartime China, particularly in their undermining of Patrick Hurley's credibility and representativeness of U.S. intentions. The author pinpoints the initial success of the Marshall Mission as the "seeds" of the American failure in China. With the then superior Nationalist troops restrained from attacking the inferior Communist forces, the Communists became "no longer interested in reaching a negotiated settlement in which their position would be weak" (p. 204).

Thornton singles out the battle of Szepingchieh (1946) as the turning point of the civil war in China. The battle marked the zenith of Nationalist power, soon after which, the Nationalists assumed a defensive strategy. Thornton attributes this change to an "unwise" U.S. embargo on ammunitions shipped to Chiang Kai-shek's government, which caused Chiang to adopt holding operations to conserve ammunitions. The embargo was unwise, Thornton stresses, because at the same time Soviet Russia was helping Lin Piao rebuild his Fourth Field Army. He argues that if the U.S. had not withheld ammunitions from Chiang in the crucial months of 1946 the outcome of the Chinese civil war might have been very different. Thornton does not point out that the Nationalists' inefficient use of the military stemmed more crucially from many previous and endemic problems other than supplies of ammunitions. The Nationalists' defeats were never directly due to lack of ammunitions: witness the stockpiles abandoned by the retreating and surrendering Nationalist troops in 1948-49. The Chinese Communists' ability to organize men shows forth even as Thornton meticulously compares the weapons and ammunition of the two sides. He also questions the soundness of the Truman-Acheson hands-off policy in the last stage of the Chinese civil war. "Who Lost China?" thus still is an issue for the author.

Thornton defines history as the record of man's achievement and aspiration; "the task of the historian is to measure the one in terms of the other" (p. vi). Throughout the book, he adheres closely to his task. He advances hypotheses to interpret events, and cites sources for support. His reconstruction of events such as the Cultural Revolution and search for the motives of the major actors are on the whole plausible and sometimes ingenious. In emphasizing the international context of the Chinese power struggle, however, he does not cover adequately the socioeconomic basis of Chinese politics and ideological commitment of the opposing groups. On the other hand, the strength of this book lies precisely in its "international" focus. As such it is a valuable addition to works on Chinese politics.

I must add, though, that the author's rendition into English of Chinese place names should have been more consistent. On p. 110, it was not the Kwantung Army that captured Nanking in December 1937. On p. 346, the sequence of footnotes 21 and 24 should be rearranged. Most regrettable is the omission of a comprehensive bibliography at the end of the volume.

SUSAN H. MARSH

Brown University

Vietnamese Communism: Its Origins and Development. By Robert F. Turner. (Stanford, Calif.: Hoover Institution Press, 1975. Pp. xxix, 517. \$14.95.)

The Indochinese Communist Party — the genesis of communism in Vietnam — was founded more than forty years ago, in 1930. This voluminous study of the history of the Communist movement in Vietnam graphically relates its changes and development through the years: including a mass of information about the Indochinese Communist Party, the Vietnamese Worker's Party, and the People's Revolutionary Party — all of which, according to this authority, formed one Communist Party in Vietnam. In tracing this development of Vietnamese communism, focus is directed on these three important questions: (1) How did the Vietnamese Communists gain control of the

anti-French resistance movement and use it to their advantage? (2) How did the Vietnam Communists manage to maintain control in North Vietnam after the Dien Bien Phu victory over the French in 1954? (3) What has been the relationship of the Vietnamese Communists to international communism?

In various places within the work the author discusses other intricate questions which academic teachers and other thinking Americans have pondered with reference to Vietnam and communism. To cite a few: What role did North Vietnam and the Communist Party play in the recent Vietnam War? Why did the 1973 Paris Agreement fail to bring the desired peace in South Vietnam? A question provoking real debate centers on Ho Chi Minh as a Vietnamese nationalist and international Communist - as a nationalist, did he use Communist support to liberate Vietnam from Western imperialism; or, was he at heart an international Communist who used slogans of nationalism to his advantage?

The author, a Public Affairs Fellow at the Hoover Institution on War, Revolution and Peace, has made an intensive study of Vietnamese communism for more than a decade. This probing has not been performed in an isolated academic setting but is rooted in hard field research during four valuable trips to Vietnam. While in Vietnam, the author gathered a large amount of raw material from which this definitive account of Vietnamese communism has been written. His knowledge of the Vietnamese language has enabled the author to make a wide use of vernacular documents. Among the most valuable research sources he used in this study are many Vietnamese Communist publications - books, pamphlets, newspapers, and periodicals (e.g., Hoc Tap, Nhan Dan, Lao Dong), as well as Communist radio broadcasts. Beyond that, he used numerous accounts of personal interviews with defectors from the North Vietnamese Army and refugees who witnessed the various stages of development in North Vietnam.

The writer appears to have made an earnest effort to document all factual statements or assertions in the body of the text which might be questioned by the reader; every page abounds with detailed footnotes — thus displaying the prodigious work expended to insure accuracy. For example, to clarify and expand the fact that Ho Chi Minh — given an entirely different name at his birth in 1890 — was known by numerous pseudonyms, Mr. Turner in a most lengthy footnote mentions the twenty-eight actual names by which Ho Chi Minh was known during his lifetime. One

well-known name this Vietnamese leader used from 1918 for a quarter of a century, was Nguyen Ai Quoc, which translated means Nguyen who loves his country, or Nguyen the Patriot. In 1939 when this Communist leader was in China he first used the name Ho Chi Minh, meaning "he who aspires to enlighten." It was by this name that he became well known from 1942 to his death in 1969.

Appropriately, this work opens with a most informative biographical sketch of Ho Chi Minh, the man who established the Communist movement in Vietnam. Born into the home of a "patriotic scholar," or nationalist, he naturally emerged as a young Vietnamese leader - who later became converted to Lenin's doctrines, received training in Moscow, was a Communist agent in China, organized Vietnamese youth into a Communist group, neutralized nationalist competition to the wider Communist goal, and worked hand in hand with Moscow for the establishment in Vietnam of a Communist Party in 1930. Near the end of World War II Ho Chi Minh was recruited in Kunming, China, in March 1945 to serve as an intelligence agent in Vietnam for the U.S. Office of Strategic Services, using the code name of Lucius. Some years later he was misunderstood and rebuffed by the Washington administrations, a costly error which eventually caused the U.S. to become hopelessly entangled in the recent Vietnamese conflict.

In closing, it is significant to note that two-fifths of the volume comprises salient documents relating to the Vietnamese Communist Party and the international Communist movement. It is a veritable source book for the diligent researcher desiring to see the actual text of such documents - many now translated into English for the first time. For example, these appendices include: Lenin's theses on the national and colonial questions, the treatise which converted Ho Chi Minh in 1920 from a nationalistic patriot to an international Communist and placed on him the responsibility for the application of Leninism to Vietnam. Among the other documents are: Action Program of the Indochinese Communist Party; Declaration of Independence of the Democratic Republic of Vietnam; Manifesto and Program of the National Liberation Front of South Vietnam; and Documents of the 1954 Geneva Conference, and the Paris Agreement on Vietnam in 1973.

CECIL HOBBS

Library of Congress

Communism in Scandinavia and Finland: Politics of Opportunity. By A. F. Upton. (Garden City, N.Y.: Anchor Press, 1973. Pp. 442. \$2.95, paper.)

This volume is for the most part (nearly 300 pages) a history of the Finnish Communist Party by A. F. Upton, a professor of history at St. Andrews University, with short essays on the Danish and Norwegian parties by Peter P. Rohde, a free-lance writer who was expelled from the Danish party in 1953, and a more extensive essay on the Swedish party by Åke Sparring, Director of the Swedish Institute of International Affairs.

Sparring interprets the history of Swedish communism as a constant struggle between "reformers" and "dogmatists" (p. 62), and his discussion of the party's attitudes toward a parliamentary transition to socialism is of particular interest to students of international communism. The history of the Danish party is seen by Rohde as the withering away of a party that never was very impressive (pp. 7, 33), and his history of the Norwegian party is a brief description of the rise and fall (1923-69) of a once powerful movement (pp. 47, 59). Both Rohde and Sparring provide interesting glimpses of Communist leaders (e.g., Axel Larsen in Denmark, Peter Furubotn in Norway, and C. H. Hermansson in Sweden), although it is the Sparring contribution that stands out as the most significant of the essays.

Upton's treatment of the Finnish Communist Party, which he rightly sees as having much greater historical significance than its counterparts in the three Scandinavian countries, constitutes the core of the book. Some fifty years of party history are covered, with emphasis on the years 1918-66, and the nonspecialist may find something useful in Upton's description of the Finnish movement. It should be noted at the outset, however, that the reader will be hard-pressed to discern a theme. It is perhaps to be found in the author's reference to the "persistence and consistency" of the Communist vote over the years and to the ability of the party to hold the loyalty of its rank-and-file in spite of "all its absurdities and contradictions" (p. 341). Upton refers to "proof" (p. 350) that the Communists are able to maintain their hold on between 21 and 24 per cent of the Finnish voters, although the results of the 1970 and 1972 elections, in which the Communists got 16.6 and 17.0 per cent respectively, seem to have necessitated an insertion by Upton of two final paragraphs that contradict his theme and that provide little explanation for the reversal in the electoral fortunes of the Finnish Communist Party.

A more apparent weakness in Upton's treatment of the Finnish party is his heavy reliance on secondary sources. Upton refers repeatedly and uncritically to the memoirs of Arvo Tuominen, who was a prominent Communist in the interwar years, and to the memoirs of Yrjö Leino, a Communist who was Minister of the Interior in the early postwar period. Upton considers the fourth congress of the party a milestone in the development of Finnish communism (p. 142), but there is no indication that he has read the stenographic report for that congress. He refers to the Left Group of Finnish Workers (p. 186), but he did not consider it necessary to look at the unpublished minutes for meetings of the Group, even though the Group included leading Communist figures who broke with the party in the late 1920s. Upton refers to testimony contained in "police records" (p. 233), but one must conclude from his footnotes that he has not worked in the police archives. Primary sources are not even used for many direct quotations (e.g., Manner on p. 139, Sirola on p. 140, Kulmala on p. 174, Lenin on p. 210, Gylling on pp. 211-212, Paasikivi on p. 247, and Leino on p. 260).

The value of Upton's work is further undermined, at least in my eyes, by numerous errors (some important, some not so important) and by misleading statements. It is curious to read (p. 108) that leaders of the Finnish Social Democratic Party were being urged by "members from below" in the summer of 1917 to take power and institute a socialist revolution. It is not "beyond dispute" (p. 119) that between seventeen and twenty thousand Finnish Reds lost their lives in prison camps during the course of 1918. Edvard Gylling and Lenin were not "close personal friends" before the Finnish Civil War (p. 210), and President Urho Kekkonen would be surprised to learn that he "always favoured conciliation with the Soviet Union" (p. 313). Kustaa Rovio did not "control" (p. 213) the Autonomous Karelian Soviet Socialist Republic in 1923 (he became first secretary in 1929), and the hint that Gylling may have been released from a Soviet labor camp in the 1950s (p. 214) is at variance with the fact that he died in the 1940s. Last but not least, it is misleading to state that the Communist vote in Finland is "essentially an urban vote" (p. 344) when statistics show that the Finnish party has on occasion (e.g., 1966) won more than half its national vote from rural communes (as opposed to cities and towns).

In sum, the above weaknesses are of sufficient magnitude collectively to discourage the

specialist for sure, and perhaps also the generalist, from turning to the Upton volume for knowledge about communism in the Nordic countries.

JOHN H. HODGSON

Syracuse University

North Vietnam's Strategy for Survival. By Jon M. Van Dyke. With a Foreword by Edwin O. Reischauer. (Palo Alto, Calif.: Pacific Books, 1972. Pp. 336. \$10.95.)

Among the many irrational acts committed by the United States in the Second Indochina War, perhaps the most irrational was the bombing of the Democratic Republic of Vietnam. The decisions that led to the bombing and to specific targeting policies already have become a primary data source for the intellectual descendants of Professors Snyder, Bruck, Sapin, Paige, Hilsman, and Allison. The bombing is destined to rival the Korean War and the Cuban Missile Crisis as a case study in American foreign policy decision making. Curiously, scholars have made almost no effort to examine systematically the impact of the bombing on the country it was intended to defeat - the country which, in the end, defeated the United States. The persistence of parochialism about the Vietnam War among American social scientists - matched only by Pentagon planners and American presidents - is striking. One exception is Jon M. Van Dyke, whose North Vietnam's Strategy for Survival remains, five years after its publication, the most thorough account of how the DRV responded to the American "air war of destruction."

First drafted for the National Security Policy Seminar at Harvard University in 1966-67 under professors Leach, Quester, and Kissinger, and subsequently expanded to cover the period of the most intensive bombing through late 1969, the book describes the specific measures which the DRV employed to survive the bombing and at the same time match the American escalation of the war in the South. Defensive weapons systems, shelters and alert procedures, transport and communications maintenance techniques, mass mobilization, propaganda and labor redistribution measures, administrative decentralization, militia training, evacuation of population and dispersal of schools and industry, revision of agricultural and marketing systems, aid from abroad, and many other subjects are described in exhaustive - and mind-numbing - detail. The principal sources are DRV publications translated by the Joint Publications Research Service, supplemented by

accounts of foreign observers and discussions with persons in the "Washington intelligence community." The author's hope is to "help explain the Vietnamese system of values and indicate why the strategy of bombing North Vietnam was not a successful strategy" (p. 24).

No matter how much damage the bombing caused, the DRV found compensatory means to maintain a tolerable livelihood for its people and was never deterred from continuing the war. The DRV achieved this mainly by virtue of its institutional effectiveness and popular support, and without the slightest cession of sovereignty to the Great Powers aiding it. Van Dyke presents an incontrovertible rebuttal to American strategists who hoped the Vietnamese would be less resilient than all the other peoples whom air power had failed to bomb into submission.

In spite of this admirable quality, the book has some weaknesses that arise because Van Dyke, professor at Hastings College of Law, is not a Vietnamologist and because he places a higher value on policy relevance than on social science. The subject which needs most to be discussed in a theoretical and comparative context, namely, the political and administrative response, is treated in the purely descriptive manner that characterizes the rest of the book, perhaps because the DRV political process is not well understood by nonspecialists. The social and cultural contexts are all but ignored. These shortcomings may be overlooked by someone interested only in the mechanics of how people and cadres coped, but the book makes no contribution to the sociological study of war.

Explanation of why the DRV chose certain policies and the promised illumination of Vietnamese values are left to the reader to draw for himself from the enormous pile of facts. For example, Van Dyke accurately reports such things as the ambivalence of urban school teachers and the government toward each other (pp. 139-140) but makes no reference to the traditional values and social composition of the teaching profession in the DRV which account for this ambivalence. He also can be led by nonexpert sources into such absurdities as saying, "the Muong . . . are ethnically related to the Chinese and ... sometimes show more allegiance to Mao Tse-tung than to Ho Chi Minh" (p. 219). These shortcomings, however, do not undermine the book's central purpose of showing how the DRV survived "the most sophisticated and sustained bombing campaign history has yet known" (dust cover). While the effect of the war on the DRV still deserves further study, Van Dyke's book stands as evidence that Henry Kissinger either did not read seminar papers submitted to him at Harvard, chose to ignore them, or was driven by other factors than those he stated publicly when he supported Richard Nixon's decision to bomb Hanoi in December, 1972.

WILLIAM S. TURLEY

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Populism and Political Development in Latin America. By A. E. Van Niekerk. (Rotterdam: Rotterdam University Press, 1974. Pp. 230. \$20.00.)

This is a brief though ambitious book, which is the published dissertation of the first Dutch Ph.D. specializing in Latin American politics at Rotterdam University. The focus is upon the characteristics and political impact of populistic movements in Latin America. The author reviews "theoretical" material in populism and political development, in attempting to define populism in Latin America largely in regard to its ideology, its relationship to political and economic development, and its implications for governance and coalition politics. Theoretical material is drawn partly from general works on political development by Apter and Nettl and partly from major Latin American scholars such as Furtado, Jaguaribe, Weffort, and Cardoso (e.g., some of the Brazilian social scientists who have studied populism). Moreover, six cases are reviewed: Brazil, Peru, Bolivia, Venezuela, Argentina, and Colombia.

Among his major arguments the author claims that populism functioned mainly to "integrate" varied political interests into national politics through coalition-building that served to co-opt newly organized groups arising from industrialization and urbanization in Latin America. Populistic movements, once in national power, utilized charismatic leadership, extensive patronage, and a division of the spoils of bureaucratization as sources of governance. Clearly, these sources were not unique to populism but were nonetheless used generally to control or channel political mobilization accompanying capitalist economic development.

Though the linkage between theoretical and case materials is not always clear, and though the author reviews mainly orthodox material without innovative or novel interpretation, the book is useful as a general overview of basic political development literature of the 1960s and of some major cases of populism. As I indicate below, the book would appeal more to

beginning students of Latin American politics than to advanced professionals.

Unfortunately, the book does not provide sufficient nor explicit linkage between populism and current topics in Latin American political study. Recent research focuses upon the bases, organization, exercise, and policy consequences of national power in Latin America. While electoral politics are basic to populism since the 1930s, such politics have been removed recently in a number of nations (including many of those reviewed by the author) in favor of military-technocratic regimes. The author does not discuss populistic movements in relation to the recent rise and persistence of military regimes, to the reality of dependency of Latin American nations, to the political mechanisms of corporatism (which are fostered partly by populism itself), and to the question of political control over technology and economic development as central issues of power throughout Latin America. Nor does the author deal adequately with the question of political recruitment and bureaucratization in relation to populism and the structure and exercise of power in Latin America.

In sum, the book may give rise to nostalgia for those longing for populism and a degree of popular participation in Latin American politics. There is, however, little novel material in the study, though, as an overview of general and orthodox material for introductory students, it is a useful reference volume.

CLIFFORD KAUFMAN

Wayne State University

Uruguay: The Politics of Failure. By Martin Weinstein. (Westport, Conn.: Greenwood Press, 1975. Pp. 190. \$13.50.)

Professor Weinstein's slim volume on Uruguay attempts to explain the prolonged political decay of one of Latin America's more modernized nations. The author argues that the traditional view of Uruguayan politics - that it was different from and superior to what existed elsewhere in the region - was a misconception, based on the assumption that Uruguay was a pluralistic, participant society that had through government initiative diminished class identities and inequalities. Weinstein sees Uruguay as a nation historically divided by class and ideology, unable to generate a cohesive national identity, and victimized politically by the same Ibero-American tradition of "corporatism" as its neighbors. He argues that Uruguay's outwardly democratic institutions were emasculated by its leaders' political bias toward "coparticipation," a system for sharing power which Weinstein illustrates in considerable detail. Coparticipation, he believes, rests on corporatist assumptions that there is a natural and legitimate order to societies and that once that order is established, elements in the society must not seek through political means to alter it radically or expand their power at the expense of others. Weinstein finds a basic contradiction in Uruguay between representative institutions and coparticipation, a contradiction that could be ignored so long as economic development and prosperity were sufficiently high to minimize conflict and competition in the political system. But during the extended period of economic decline that followed the Korean War, the contradiction created increasingly serious confrontations, and was responsible for the growing instability and eventual collapse of the political system.

Like most observers of Uruguyan politics. Weinstein traces the present dilemmas to José Batlle, who succeeded during the first three decades of the twentieth century in stabilizing the nation after seventy-five years of turbulence and civil war. Batlle tried to impose on the nation a visionary ideology of nationalism (Batllismo) which advocated the values of economic development, nationalization, social welfare and equality, and political stability, and which relied for its political support upon the rapidly expanding urban population of Montevideo. Weinstein points out that from the very outset Batlle was unable to crush the rural counterculture and its powerful elites, whose traditional exports were paying the cost of national transformation. His compromise solution was coparticipation, through which economic and political power was divided so as to guarantee a minimum share for minority interests. The author cites how the opposition party (Blancos) was given hegemony over those departments in the nation where its interests were strongest, and how minority opposition to Batlle even within his own party (Colorado) was recognized and protected through an unusual and carefully devised electoral system. There were experiments with dividing formal power between the parties in the legislature, and from 1952 to 1967, a collegial executive extended coparticipation to that branch of government.

Weinstein raises important and difficult themes, and his exploration of the corporatist tradition in Uruguay is often provocative. Unfortunately the book suffers from extraneous material and digressions, problems aggravated by its brevity. Some of the discussion of the

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revolutionary Tupamaro movement and the discussion of social classes is inadequately related to the general theme, and the diffuse theoretical framework provided at the outset is labored and unconvincing. However, the analysis does contain an alternative interpretation of Uruguayan politics which is both persuasive and timely.

RONALD H. McDonald

Syracuse University

The Chinese Calculus of Deterrence: India and Indochina. By Allen S. Whiting. (Ann Arbor: University of Michigan Press, 1975. Pp. 299. \$15.00.)

This is clearly a major book, the most important one on Chinese foreign policy since Jay Taylor's China and Southeast Asia. The author, who is well known in China-watching circles, has had a distinguished official and academic career and has published a number of significant works, notably a standard book on China's entry into the Korean War. The study of Chinese crisis behavior and of the ways in which foreign powers, especially the United States, should behave in order to avoid war with China has been probably his main professional concern. Although one can of course legitimately differ with Professor Whiting's methodology (which stresses content analysis), his interpretations, and his conclusions, one can only respect the high level of factual knowledge and analytical insight that is displayed in his work.

The book under review, the author tells us, was written in lieu of a memoir of his years in governmental service. He is to be congratulated on avoiding reminiscence in favor of analysis. The book deals mainly with China's border war with India in late 1962 and Peking's more indirect role in the heyday of the Vietnam War (1965–68). For comparative purposes, some brief reflections on China's role in other crisis situations, including the Korean War and the Taiwan Strait crisis of 1958, are also included.

The first major section, which deals with the Sino-Indian border war, is generally excellent. To be sure, it makes a bit too much use of content analysis, tends to take Peking's propaganda statements at face value, attaches too much causative importance to Peking's concern over American subversive activities in Tibet, and relies unduly on the excessively anti-Indian account of Neville Maxwell, but these are minor flaws compared with the high overall level of the analysis. Peking's attack on India is skillfully related to the Cuban missile crisis and to

Chinese concern over other developments of that period, including tension in the Taiwan Strait, in Southeast Asia, and along the Sino-Soviet border.

The account of China's role in Vietnam is briefer and less satisfying, but it is undoubtedly right in concluding that Peking's actual role, which was designed with great care and skill, and still more the fear of what Peking might do if further provoked, exerted a significant restraining influence on the United States, for example by deterring an invasion of North Vietnam.

From a purely substantive point of view, it is unfortunate that Professor Whiting does not deal at length with Peking's role in the Sino-Soviet border crisis of 1969. Presumably he considered that his resignation from the government shortly before would deprive his analysis of the authority that he believes, with considerable justification, is conferred on his treatment of the Sino-Indian border and Vietnamese crises by his official position at those times.

The author's conclusion that Peking's external behavior is essentially rational and nonaggressive (at the overt level, at any rate), if sometimes activist, is correct, as is his warning that when Peking acquires a reliable secondstrike nuclear capability its caution may not be so dependable. He perhaps does not see clearly enough that Peking has tended to exaggerate (i.e., both misperceive and misstate) external threats to itself, and that this tendency ought to decrease with the passage of time. In any event, the Chinese response generally begins with counterthreats ("... will not stand idly by," etc.), which may of course be bluff. On the other hand, they may not be, since they have sometimes led to what Peking considered a pre-emptive or forestalling attack, notably in Korea.

Professor Whiting's study is fully documented and is in every way a valuable guide to some important aspects of Peking's external behavior. Its main defect is its style, which is rather cumbersome and hard to read.

HAROLD C. HINTON

The George Washington University

Captured German and Related Records: A National Archives Conference. Edited by Robert Wolfe. (Athens: Ohio University Press, 1975. Pp. 299. \$10.00.)

Death and fire are proverbially reputed to be the best forces working on behalf of social scientists threatened with ever-accumulating records of past political events. Mr. Wolfe and his colleagues have ably presented their case of a contrary phenomenon: their efforts made from 1943 to 1968 to preserve and systematize access to the documentation retrieved by the Western allies from the disintegrating Third Reich. It records the losses by war and postwar stupidies or wanton destruction. But the contributions made at this National Archives Conference should elicit professional wonder and appreciation that so much was done under often very difficult military, physical, and political circumstances. Beyond the fascinating human-interest accounts of discovery, restoration, and preservation of records is a basic fact: this book is indispensable for any social scientist working with source materials on the Weimar Republic, Nazi Germany at home, and Nazi-occupied Europe. It contains the most complete English-language guide to the present location and dimensions of all documentary collections on twentieth-century German history to 1945 that are readily accessible to Western researchers. Considerable collections, of course, remain in East Germany and other Russian satellite nations. No comparable compilation of resources there exists, and access for Western scholars can be affected by changes in political wind. For the immediate future, however, this excellent combination of archival history, records of documentation, and professional discussion can keep most of us fully occupied.

HENRY CORD MEYER

University of California, Irvine

Essays on the Study of Urban Politics. Edited by Ken Young. Foreword by Edward C. Banfield. (Hamden, Conn.: Archon Books, 1975. Pp. 208. \$15.00.)

The adjective "urban" suffers from overuse. As an illustration, I spend most of my professional life working in an office on the fourth floor of a ten-story building in a gigantic multiversity. Starting from the bottom and proceeding heavenward, there are offered courses — depending on the floor — in the following: "urban anthropology," "urban geography," "urban politics," "urban studies" (a program, not a department), "urban sociology," and "urban history." For all I know, there may be courses labeled "urban psychology," but psychology has its building half a mile away and I am not privy to their undertakings. But I do know that the College of Education has a whole department called "Urban Education."

This massive fragmentation of efforts to study urban phenomena has resulted in duplica-

tion, sloppy thinking, and enormous confusion. It is an open secret that this confusion has spread to the ranks of political scientists. In his Preface to this collection of essays by a group of British and American scholars, Edward C. Banfield blames our relative ineffectualness on four general factors. "Firstly, the urban political scientist cannot satisfactorily specify the object he is investigating" (p. viii). "Secondly, the urban political scientist, because he studies a rag-bag of matters, is obliged to employ a rag-bag of concepts and theories" (p. ix). "Thirdly, the urban political scientist cannot settle upon a 'one best way' of doing empirical research" (p. ix). And, finally, "... our studies do not build upon another: they do not add up to a body of knowledge" (p. x).

Essays on the Study of Urban Politics was originally conceived of as a special edition of the British journal, Policy and Politics. All of the seven essayists are editors or contributors to that important journal, and each is distinguished in his or her own specialty. Since they all agree with Banfield's general verdict, they are united in their endeavor to raise the level of urban political studies, mostly by making use of comparative British and American materials, both old and new. As the editor, Ken Young, puts it, the book has two principal objectives: first, contributors were expected to delineate their separate areas within the field; and, second, "the overall collection was intended to achieve a clearer prospect of the field of urban political inquiry, to demonstrate the relationship between its several aspects, and to point up alternative avenues of exploration" (p. xiv).

Kenneth Newton begins with a review of issues in the American community power studies and their lessons for British research. His finding is that the American studies have little relevance to Britain. Jeffrey Stanyer, noted for his studies of English local authorities, discusses the state of urban electoral analysis, and offers a complex model for the examination of urban elections. Janet Lewis argues for the recognition of a wide range of causally significant and often elusive variables in her chapter, "Variations in Service Provision: Politics at the Lay-Professional Interface."

This is followed, quite logically, by an essay by Bleddyn Davies, senior editor of *Policy and Politics*, who outlines an approach to the variables identified by Lewis through multivariate analysis. Oliver Williams, whose writings on the subject are well known on both sides of the Atlantic, argues for the centrality of the spatial dimension in urban political analysis. Editor Ken Young, drawing heavily upon Amer-

ican studies, explores metropolitan reorganization on a comparative basis. Finally, in a most interesting essay Stephen L. Elkin discusses comparative urban politics and interorganizational behavior. The analysis is imaginative and suggestive of further important research.

How does the editor himself view the final product of this collaboration of experts? He considers the essays to represent a potential step toward the common objective: the development of an urban political theory. In one way or another, he says in his concluding chapter, all of the contributors are working toward this objective by touching on the question of values. Drawing on the research of Clyde and Florence Kluckhohn, Young asserts that the three key concepts within the field of urban political analysis that have a significant value content are urban imagery, localism, and professionalism. Each of these requires systematic exploration by political scientists, and Young suggests how this may be done.

Considered as a whole, how well does the book measure up to the goals delineated by the editor? My own judgment is that the work is a significant contribution to the growing field of urban politics. It takes a long and heady stride forward in achieving a "clearer prospect of the field of urban political inquiry," to use Young's own words. Urbanists are not only collecting their own materials; they are also building systematically on the findings of their colleagues. This book deserves a serious and widespread readership, and it should serve to stimulate and to encourage urbanists wherever they happen to reside.

Whatever directions future research in urban politics may take, the Young collection of essays makes it clear that urban politics has become firmly established as a legitimate subfield of comparative politics. Widespread recognition of this relationship may in itself help urban political scientists to develop hypotheses and theories that urban politics so assuredly needs.

MURRAY S. STEDMAN, JR.

Temple University

Liberalism i Kris. Folkpartiet 1939-1945 (Liberalism in Crisis: The Liberal Party 1939-1945). By Kent Zetterberg. (Stockholm: Liber Förlag, 1975. Pp. 428. Sw. Kr. 46:50.) In Swedish with summary in English.

Since 1966, a team of Swedish scholars has been engaged in a large-scale research project on the subject of Sweden during World War II. The present volume, a doctoral dissertation at Stockholm University, is part of a subproject designed to examine the behavior of the major Swedish political parties during the war years.

On December 13, 1939, in response to a conflict that was steadily drawing nearer to neutral Sweden, four major parties (excluding the Communists) announced the formation of a wartime coalition government. In the newly formed cabinet, the Social Democrats assumed five seats, and the Opposition parties (Conservatives, Farmers, and Liberals) were allotted two seats each. The coalition survived three wartime elections — in 1940, 1942, and 1944 — and was not dissolved until July, 1945, when the circumstances behind its creation no longer obtained.

During the life of the coalition, the fundamental problem for each of the participating parties was how to maximize its contribution to national unity without sacrificing its own distinctive political profile in the process.

This study of the Liberals is divided into two parts. The first deals with the behavior of the party's parliamentary delegation with respect to three wartime issue-areas: national defense, economic policy, and civil rights. The second part proceeds from a more comprehensive perspective, and is devoted to an analysis of how the party's actions related to party strategy.

Although the Liberals agreed on the immediate need for a greatly strengthened defense effort, they were not converted to the traditional conservative policy of a permanently strong defense. They remained instead faithful to their prewar policy of "elasticity" — a cyclical policy of adjusting defense preparedness to the fluctuations of the international environment.

The major economic issues of the war turned on how the burdens of retrenchment, defense expenditures, inflation, and taxes were to be allocated among the different income groups. It is not surprising that the Liberals, with their diverse class constituency, adopted a stance that was situational, pragmatic, and "social liberal" (an acceptance of some government economic intervention, but not too much).

The section on civil liberties is one of the most interesting in the book. All the government parties accepted the general principle that if one were forced to choose between the survival of the nation and the maintenance of democratic values, the former would have priority. The real problem was where to draw the line in imposing restrictions on speech, press, or party organizations. The reaction of the Liberals here was both complex and subtle.

In essence the party implicitly agreed to pursue different policies at different levels of its own organization. Those levels farthest from power (extraparliamentary women's and youth groups) were the most persistent in their pressures against censorship. Although the Liberal members of the government dutifully supported the government restrictions, a majority of Liberals in the Riksdag refused to follow suit.

While the first three parts of this book may be illuminating chiefly for students of Scandinavian affairs, it is the final part that contains the kind of systematic analysis most likely to interest political scientists. The author applies to his study of party activity a general framework of political party norms developed by Günnar Sjöblom in his Party Strategies in a Multiparty System, one of the better theoretical works to come out of Scandinavia in recent years. Thus, in the early years of the war (1939-1942), when external pressures were greatest, parties pursued largely what could be characterized as "unity norms." As the danger receded in 1943, more and more party activity was motivated by "party norms" (maximization of parliamentary influence, votes, and party cohesion).

The study demonstrates amply that the war, with its concomitant quest for national unity, did not entirely eliminate partisan political activity, but instead caused a redefinition of the boundaries of acceptable controversy. To the world outside Sweden, the appearance of an almost monolithic unity prevailed, but to a sharp eye, the differences between parties were still there, although expressed in subtle and indirect forms which became sharper and more explicit as peace approached. For anyone interested in how democratic coalitions behave under conditions of considerable but temporary stress, and in the problem of reconciling democratic values with wartime exigencies, there is much to be learned from the Swedish example.

JOSEPH B. BOARD

Union College

Afghanistan's Foreign Affairs to the Mid-Twentieth Century: Relations with the U.S.S.R., Germany, and Britain. By Ludwig W. Adamec. (Tucson: University of Arizona Press, 1974. Pp. 324. \$13.00, cloth; \$7.95, paper.)

Political scientists tend to look askance at diplomatic histories because they feel that they often record events in chronological order without any coherent or relevant theory to explain the political processes involved. To a certain extent Professor Adamec's account of the foreign relations of Afghanistan could be subjected to that criticism. But in fact a careful reader will perceive that Adamec does have a hypothesis which he develops and documents even though it remains largely implicit throughout the book.

He elaborates and demonstrates two important principles of international politics: (1) that a small power, geographically located in a strategically sensitive area of rivalry between major powers, is in an unfortunate position and constantly under great pressures. Twist and turn as it may, it cannot altogether escape them and must find ways to preserve its freedom of action and its very independence; (2) that apparently trapped in such a situation, the Afghans developed to a very high degree the techniques of the artful dodger and eventually turned the pressures to their own advantage by developing their country at the expense of the great-power antagonists. All this before the onset of the cold war. By implication, other small countries in the same fix should take note.

Of course, as Adamec explains, Afghan rulers have had much longer experience at playing major powers off against each other than most Third World nations. Nonalignment was almost invented in Afghanistan, at least in its modern form. Afghans even have their own term for it — bi-tarafi, a balancing of two sides. As Adamec points out, this was not a cold war invention but a practice Afghan rulers have followed since the eighteenth century in their attempt to preserve Afghan independence from the encroachments of Russian and British imperialistic expansion in Asia.

One of the subtle techniques of bi-tarafi pioneered by the Afghans is that of using a third power, preferably one geographically remote and more interested in commercial advantage than political penetration, to soften the asperities of bipolar rivalry and to cushion the neutrality and independence of the small power. In this book, Adamec shows how and why the Afghans chose Germany to play this third-power role. As World War II approached, the Afghan choice of Germany to play this neutralizing role seemed unfortunate because Germany, the presumably "benevolent neutral," attempted to convert its position of commercial partner and source of technical assistance into political leverage to persuade Afghanistan to enter the war on the Axis side. Germany's purpose was to tie down British arms on the Indian northwest frontier, which in conjunction with the Japanese threat to India's

northeast border was designed to inhibit effective Indian-British participation in the European war effort.

For a time early in the war it seemed that Germany might succeed in persuading Afghanistan to abandon its traditional neutrality. Lavish promises of territorial gains giving Afghanistan access to the sea were made and, more importantly, the opportunity to settle old scores with Britain over the treatment of the Pushtun tribesmen of the Indian frontier was offered by Germany to the Pushtun ruling elite of Afghanistan. In the end, Adamec tells us, Afghanistan maintained its balance especially after Germany invaded the USSR and was stopped at Stalingrad. Later under pressure from Britain and the USSR Afghanistan reluctantly expelled all nonofficial Axis nationals, fearing that a denial of this demand would precipitate an allied takeover as had happened slightly earlier in Persia.

Adamec's account stops shortly after World War II so that he does not cover the success of Afghan diplomacy in enlisting the United States, first as a successor to Germany as the distant third power and later, using cold war leverage, as a successor to Britain to counterbalance the USSR. But Adamec has laid a well-documented foundation for the fascinating story of how Afghanistan turned its geopolitical disadvantage into an asset by stimulating economic competition within Afghanistan between the cold war rivals.

Viewed correctly then, as a case history of the role and problems of a strategically placed neutral small nation threading its way through a bipolar environment, this book is worth the full attention of political scientists interested in international politics.

LEON B. POULLADA

Northern Arizona University

Oil, Politics and Seapower: The Indian Ocean Vortex. By Ian W. A. C. Adie. (New York: Crane, Russak & Co., 1975. Pp. 98. \$5.45, cloth; \$2.95, paper.)

Adie's title is confusing and misleading; the monograph should have been called "Indian Ocean Megapolitics." Too ambitious for its size, the study nonetheless informs and prescribes in a fashion familiar to strategists. Adie's point of view is lucid and will seem rational to neo-Hobbesians: sovereignty requires arms, for the implacable forces of other sovereigns invariably devour the weak. So, although no clear and present threat exists for Australia (his host during this research), Mr. Adie urges an arms

buildup by Australia in alliance with the United States because long range threats are certain — from the Soviet Union, from China, from wherever!

Why misleading? While oil is the catalyst for discussion, most of the book deals with the politics of economic change among the powers involved in the Indian Ocean theater. Although seapower is a topic of comment, the author properly views naval capability as a tool, not an intent. Indeed, China has no naval presence in the area, yet its policies receive a dozen pages of description throughout the volume.

This raises the question of the book's organization. I found the material confusing because the seven chapters each include comment on Soviet, U.S., and Chinese policies: whether the focus be "Arabia, the Persian Gulf, and the Subcontinent," "Franconesia and the African States," or "Australia and Japan." The reason is understandable, for Great Power policies, by definition, are not regionally confined; yet the book purports to be organized along regional lines.

The author is knowledgeable about details of the Indian Ocean area. From the French base at Crozet, to the current condition (1974) of Diego Garcia, and on to Pine Gap in central Australia, the reader is escorted through the fantasies of strategists in planning rooms of Canberra, Delhi, Moscow, Peking, Tehran, Tokyo, and Washington. The mystery of it all deepens as one reflects that while merely two decades ago the area was a British domain, now the Union Jack's presence receives a scant paragraph from one of her ex-civil servants. Not since the Napoleonic Wars has the Indian Ocean scene changed so rapidly. That change is judiciously comprehended by Adie, who describes the varied momentum of the several powers as they extend their naval capabilities and rhetoric.

Accurate foresight usually comes at the cost of scientific evidence in the "futures game." In this study one finds the argument persuasive, if uneven, and it is clear that the author knows more than he has written. Is he lazy, was he constrained by the editorial policy of the National Strategy Information Center (funding agent for the publisher), or is he simply so accustomed to the ideological mind-set of the Foreign Office that he ignores the need for deeper political analysis required by a critical scholarly audience? While Adie has insight into the strategic issues that drive small, medium, and great powers into an Indian Ocean imbroglio, he establishes little linkage between foreign policy and domestic politics. A weakness in the monograph is this lack of connective tissue. Example: Japan has a vital interest in open sea lines, especially through the Malacca Straits, but Adie never tells us just what domestic forces keep the Conservative government from following the U.S.'s, China's, or the U.S.S.R.'s route to alliances or deep political ties. Likewise, the ungainly steps by the Soviet Union are unfavorably compared to the astute "two-track" policy of Maoists, but no effort is spent analyzing why the Soviets grope so.

This is No. 24 in a series on strategic problems funded by the National Strategy Information Center, a rather conservative group. Change in general is viewed skeptically by the authors they sponsor, and planned change by Communist or Socialist powers is particularly suspect. Conversely, U.S. defense postures are usually found wanting. With Vietnam behind them it is understandable why the Indian Ocean should loom so large in their vision of threat to Free World security. Adie assists in portraying that vision, the fourth such study on the Indian ocean sponsored by the Center. I believe that Adie comes nearer the mark than his three predecessors in helping the uninformed grasp the essentials of megapolitics in this enormous area, the Indian Ocean.

JOHN BADGLEY

Institute of the Rockies

West German Reparations to Israel. By Nicholas Balabkins. (New Brunswick, N.J.: Rutgers University Press, 1971. Pp. 384. \$12.50.)

It is an old lament in academia that a good many intellectual efforts could have done better as tightly condensed articles than as fullblown tomes. Professor Nicholas Balabkins's West German Reparations to Israel suffers almost from the opposite problem. Most of its chapters could easily stand on their own as article-length monographs, yet as result, they may not quite add up to a book. Lucidly written and exhaustively researched, the book represents a veritable grab-bag of information and analysis on such wide-ranging topics as the economics of the transfer problem, economic history of Israel and West Germany, international law and ethics, and the politics of diplomatic bargaining.

The heterogeneity of the narrative stems from its ambitious scope. Its centerpiece is the Luxemburg Agreement, concluded between Israel and the Federal Republic in 1952. Under its terms, the Federal Republic bound itself to pay close to 3.5 billion deutschmarks (then \$840 million) in money and kind as collective reparation to the Jewish people as represented

by the fledgling state of Israel. The author set out to address two basic questions: How did this agreement come about, and what effects, both political and economic, did it have on either side and their evolving relationship? In his attempt to answer these questions, the author has spared neither effort nor disciplinary boundaries, traversing the realms of economics, history, and international law with scholarly thoroughness and analytical skill.

Thus the Reparations Agreement itself turns almost into a secondary concern, serving as take-off point for excursions into various related fields of inquiry. In legal terms, for instance, the Luxemburg Treaty was strictly res nova: one would be hard-pressed to find a precedent in international law for reparations paid voluntarily by one political community to another. Moreover, since they were both born in the aftermath of the Holocaust, Israel and the FRG were not even, strictly speaking, ex-belligerents. There was no basis in international law for a claim by one state (Israel) on behalf of individuals (the murdered and dispossessed Jews of Europe) who had not been members of its own citizenry. Accordingly, these legal conundrums prompted the author to look backward in history for antecedents and analogies. These are surveyed in "Germany's Road into Industrial Genocide" (chap. 1) and "Reparations in Perspective: The Victor-Vanquished Relationship in Interstate Wars" (chap.

Ultimately, West Germany's debt and Israel's claim could only be based on some idea of international morality, historically the most unreliable guide to political intercourse among nations. It was one of Adenauer's (West Germany's Chancellor from 1949 to 1963) greatest political achievements that he not only recognized West Germany's moral debt (as East Germany did not) but also goaded his country into concrete financial amends. Given the dismal state of the West German economy after 1945, restitution to Israel initially looked like a serious drain on the FRG's meager resources and hence not exactly like a profitable domestic strategy. Yet Adenauer's task was soon lightened by the fortunate confluence between moral imperatives and economic plenty. By 1952, the FRG was well on the way toward the "economic miracle" which quickly dwarfed the magnitude of the "transfer problem." (Both are discussed at length in chapters 2 and 3.) Moreover, considerations of morality and Realpolitik nicely reinforced each other at this point. Clearly, as Adenauer understood better than his domestic critics, financial amends to Israel and World Jewry would not only ease

Germany's excruciating moral burden but also contribute to its rehabilitation in the community of nations and expecially by the three Western victor powers which were about to grant sovereignty to the FRG in 1952.

For Israel, on the other hand, morality was at odds with economic necessity. How could Israel possibly accept money from a successor to the Third Reich which had systematically murdered six million Jews and by this acceptance appear to forgive something which could not be forgiven? Balabkins juxtaposes an account of the painful domestic debate (chap. 4) with a skillful rendering of Israel's miserable economic condition in the early years of its existence (chap. 5). In the end, Israel agreed to face-to-face negotiations with Germany at the Wassenaar Conference (chap. 6). Its outcome was the Luxemburg Treaty whose financial and technological benefits were instrumental in launching Israel's economy onto a path of sustained economic growth (as analyzed in chapters 8, 10 and 11).

As an economist, Balabkins has devoted particular attention to the economics of the West German-Israeli relationship. His chapters on the Israeli and West German economies offer instructive case studies in economic history and development which might make profitable reading even for those who are primarily interested in the politics of the relationship. Indeed, concentrating as heavily as he does on economics, Balabkins has gone a long way in heeding the often-heard exhortation to integrate economic and domestic factors in the study of foreign policy. Balabkin's book is an analytical narrative but not a theoretical study. Given its diverse foci, it may strike many as a very heterogeneous contribution, and as such, it may not satisfy the "specialists" of either economics, political science or history. It is a veritable one-man interdisciplinary show and, depending on the biases of the reader, it might demonstrate either the perils or the promises of interdisciplinary approaches to the problems of the social sciences.

JOSEF JOFFE

Harvard University

Agreement on Berlin: A Study of the 1970-72 Quadripartite Negotiations. By Dennis L. Bark. (Washington, D.C.: American Enterprise Institute for Public Policy Research. background, the preliminary jousting, the issues at stake in the negotiations, and the final outcome of the bargaining over the status and access rights to West Berlin. It is a useful overview for the educated layman and undergraduate student, but contains little grist for the specialist.

It is necessarily cast within the framework of the other negotiations associated with the broad context of the Ostpolitik, or the mutual efforts to reach a modus vivendi between the Federal Republic of Germany and its East European neighbors. One of the central themes presented is that both the Soviet and East German positions on the status of access rights to West Berlin have varied significantly. The Western Allies abrogated Article 23 and 144(2) of the West German Basic Law (constitution) at the time of the creation of the FRG, which denied West Berlin equal status with the other Laender; it was to retain a special status unique under German law and allied authority. Bark, Research Fellow, Hoover Institution, recalls that in his 14 January 1970 State of the Nation Speech, Chancellor Brandt restated the FRG position: Zugang, unrestricted access to and from West Berlin; Zuordnung, preservation and strengthening of political and economic and cultural ties between West Berlin and the FRG; and Zutritt, entry by West Berliners to East Berlin and East Germany.

Five days later Walter Ulbricht held an unusual press conference to reinforce the DDR's stance against both the West and the Soviets. He stated that West Berlin was an independent political entity within East Germany and that "the capital city of the German Democratic Republic, Berlin . . . is not subject to four-power control." The author rightly points out that the final Berlin Protocol, which traded Soviet presence in the Western sectors and assured ties with Bonn for formal Soviet acceptance of the ultimate responsibility for Western access, severely undermined the DDR claims.

The monograph ends in 1972 and was published two years later, a serious gap when events moved so fast. One wishes the author had addressed a number of substantive questions anticipated during the negotiations or that have emerged since. For example, what are the legal and political implications of the Soviets' legal acceptance of access responsibilities and their delegation of the right to the DDR to

overall product. The monograph is wordy and redundant and could have been cut by one-third. Roughly 75 per cent of the references are West German newspapers, half of which are citations from the right-wing *Die Welt*. This led to several instances of recitation of press speculation. The virtual omission of the vast primary and secondary literature in German, part of which is available in English, weakens the depth of research and analysis.

LAWRENCE L. WHETTEN

University of Southern California

The Handling of International Disputes by Means of Inquiry. By Nissim Bar-Yaacov. (New York: Oxford University Press, 1974. Pp. 370. \$24.00.)

The study of conflict has recently emerged as a major area in the field of international politics. Although contemporary research has focused primarily on the nature, incidence, and causes of international conflict - rather than on conflict management and resolution - the ways in which conflicts are resolved have also attracted the attention of scholars in the behavioral sciences, including political scientists. In his book, published under the auspices of the Royal Institute of International Affairs. Dr. Bar-Yaacov, Associate Professor of International Relations at the Hebrew University of Jerusalem, brings his analytical skills to bear, rigorously and comprehensively, upon inquiry as a means of settling international disputes. The institution of international commissions of inquiry, established by the Convention for the Pacific Settlement of International Disputes of 1899 and reaffirmed by the Convention of 1907, was designed to "facilitate a solution to these disputes by elucidating the facts by means of an impartial and conscientious investigation" (Article IX of the Convention, 1907). In subsequent evolution, however, the term "inquiry" was often used in various other meanings and - as shown by Bar-Yaacov - the method itself assumed many different forms, thus losing its original identity as envisaged by the Hague Convention. The purpose of Bar-Yaacov's study is to clarify the notion of inquiry and to find out whether and to what extent inquiry remains an independent method of settling international disputes.

In the Introduction the author analyzes the different categories of inquiry employed in international practice and various theoretical conceptions of inquiry which culminated in the two Reports of the Secretary General of the United Nations. Bar-Yaacov perceives two basic

dichotomies in the area of inquiry: first, inquiry confined exclusively to the ascertainment of facts versus inquiry intended to clarify all aspects of the dispute, whether factual, legal, or political; and second, inquiry under bilateral arrangement as opposed to inquiry under the auspices of international organization. These major themes are explored thoroughly in all their variations in the historical exposition of inquiry included in Chapters 2 through 8.

Chapter 2 covers the political and legal issues as they emerged from various texts and statements at the Hague Convention of 1899. The famous Dogger Bank incident, which was the first test of the Hague institution, is examined in detail in Chapter 3. The author proves that despite certain departures from the Hague model, the commission acted as a genuine inquiry body and by preventing escalation contributed to international peace. After reviewing the institution of inquiry under the Hague Convention of 1907, the author follows up the main theme of how inquiry was used in international practice after 1907. The discussion is divided into four parts: inquiry under treaties, disputes handled by commissions of inquiry, inquiry in the work of commissions of conciliation, and inquiry in the framework of the UN. Chapter 5 deals with various treaty arrangements such as, for example, the treaty between the United States and Canada, 1909, the Bryan treaties, 1913-1940; and the UN Charter. The functions of the commissions set up by treaties are analyzed not on the basis of the formal name or title of the commissions. but according to their actual terms of reference. The examination revealed "a definite tendency to submerge inquiry into the facts in favor of the enlarged inquiry, and subsequently to submerge the enlarged inquiry in the process of conciliation" (p. 138). The disputes covered in Chapter 6 include the cases of the Tavignano, the Tiger, the Tubantia, and the Red Crusader, all dealing with incidents at sea. The author argues that despite limited resort to genuine inquiry, the system proved its value in disputes of moderate character, involving claims over what actually happened, especially incidents at sea. The author analyzes the present conception of inquiry in the process of conciliation, based on the work of one "commission of inquiry and conciliation" (concerning the Chaco dispute of 1929) and three "commissions of conciliation." These case studies, together with his examination of the doctrine, lead the author to conclude that conciliation is a process that has two stages (1) the examination of facts and (2) conciliation proper. Bar-Yaacov emphasizes, however, that a conciliation commission may

decide not to elucidate facts likely to be prejudicial to the conciliatory effort and, in general, is guided by the principle that its inquiry must serve the purpose of conciliation.

The longest chapter is devoted to the study of inquiry in the framework of the United Nations. In general, Bar-Yaacov concludes that the concept of "inquiry" in the UN practice has been politicized and emptied of its original meaning, including, as it does, any routine preventive reporting system. Nevertheless, he is cautiously optimistic, believing that inquiry can still perform a useful function "in appropriate disputes and in favourable conditions of international cooperation and trust" (p. 329).

Bar-Yaacov's book is certainly not an interdisciplinary behavioral study in the area of conflict resolution. Quantitative precision, possibly in the form of tables summarizing various dimensions of inquiry, would add to the value of an otherwise rigorously conducted, solid piece of traditional research. Ample documentation, a number of maps and ocean charts as well as a general index and a treaty index make the book useful as a reference work. Several key Hague and UN documents are included in the Appendices. Substantively, the book is the best existing study of the Hague system of international inquiry, and as such it must be recognized as a major contribution to the growing literature on conflict resolution, of equal interest to students of international law and political scientists.

BOLESLAW A. BOCZEK

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The Horn of Africa: Strategic Magnet in the Seventies. By J. Bowyer Bell, Jr. (New York: Crane, Russak and Co., 1974. Pp. vi, 55. \$4.95, cloth; \$2.45, paper.)

The recent appearance of Mordechai Abir's study of Oil, Power and Politics: Conflict in Arabia, the Red Sea and the Gulf (London: Frank Cass, 1974) has served to emphasize the need for a full-length study of the strategic significance of the Horn of Africa. For Dr. Bell, "The basic strategic importance of the Horn . . . is simple geography. The Horn commands the Red Sea and the northwestern Indian Ocean littoral" (pp. 8-9). He defines the Horn as comprising Ethiopia, the Somali Republic and the French Territory of the Afars and Issas (Djibouti), adding thereto somewhat arbitrarily the Sudan and, as arbitrarily, excluding Kenya. Events following the Six-Day War in 1967 have progressively enhanced the importance of this region. The closure of the Suez Canal and rupture of diplomatic relations between the United States and Egypt, Libya, and the Sudan together with the withdrawal of the British from Aden opened wide the door to expanded Soviet adventures in Somalia, Aden and the Indian Ocean. With Soviet support Syria stepped up her interventions in Eritrea on the western shores of the Red Sea at a time when the United States had decided upon a policy of withdrawal from that same area. At this writing, present wide-scale military operations in Eritrea and Ethiopian-Somali rivalry over the Djibouti territory soon to become independent have made the Horn a focus of international concern.

Much of the analysis in the book is concerned with internal economic, religious, and political instabilities and conflicts within the Horn rather than strategic considerations. The author nevertheless concludes that the area will remain essentially in equilibrium (p. 48); nor does he foresee a clash of rival protege regimes (p. 8). He takes the view that "the great ideas, particularly of revolution, out of the Western world have ... often been fashioned to adorn old causes or warped to buttress ancient ambitions" (p. 34). While these assessments may be questioned in the light of recent events, there is the prediction that "In the fullness of time, a pro-Chinese Ethiopia might well face a pro-Russian Somalia, but the same old local political issues would be the reason for the alignment" (p. 8). The single greatest source of regional instability and confrontation, Somalia's designs for a Greater Somaliland, is viewed solely as a struggle for self-determination. Ignored is the far more critical and insoluble problem of the dependency of the nomadic Somalis on the grazing lands of Ethiopia.

More rewarding are the two chapters devoted to the roles of the United States, the Soviet Union, China, Israel, and France. The inveterate and bitter hostility of the Arab world, however, toward Ethiopia and in particular of Libya, Syria, and Somalia (now a member of the Arab League) is not brought into focus. Libya's campaign to remove the OAU Headquarters from Addis Ababa, Syria's leadership of the Eritrean revolt and power moves in the Red Sea itself have been ignored.

Inaccuracies flaw the work such as the assertion that the Somalis are partly of Mediterranean origin and their language unrelated to their neighbors (pp. 3-4), that the Gallas moved into the Horn 400 years ago (p. 4) and that Nimeiri and Qaddaffi came to power in 1967 (p. 45).

JOHN H. SPENCER

Tufts University

The Last Secret. By Nicholas Bethell. (New York: Basic Books, 1974. Pp. xiv, 224. \$8.95.)

In this dramatic expose of what has hitherto been one of the best-kept secrets of World War II, Lord Nicholas Bethell turns a spotlight on the forcible repatriation of some two million Russians and Ukrainians who found themselves in English and American hands in Europe between 1944 and 1947. The operations involved not only former Soviet citizens but also large numbers of men, women, and children who had never been citizens of the U.S.S.R.

Lord Bethell, a translator of Solzhenitzyn and the author of *The War Hitler Won*, examines the considerations that led the Western Allies to display such unwarranted zeal to please the Kremlin. His treatment is calm, dispassionate. His material is based on official archives and on the testimony of many eyewitnesses. Hugh Trevor-Roper, in his introduction, characterizes the work as one that calls not for judgment but rather for reflection.

The author tells us that, as a result of a secret protocol signed by the three powers at Yalta and another agreement reached in Leipzig in June 1945, the number of persons turned over to the Russians exceeded two million by the middle of 1945. The Americans specialized in tracking down Russians who had fought for Hitler and remained in Germany, and the British in locating and delivering those who had fought against the Red Army and surrendered in Austria. Combined with the Cossacks and Russians in Austria were some 200,000 Croats, Ustashas and Domobrans and their families, whom the British returned to Tito at the end of May 1945.

The rounding-up process, according to Lord Bethell, was invariably accompanied by a wave of mass hysteria and many suicides. The records show heads of families killing their wives and children and then themselves rather than return. In the end, however, except for a few who had either fled or had been allowed by their British and American guards to slip away, all who surrendered or captured were deported.

Through May 1947 the deliveries continued, the last of them labeled Operations Dachau, Plattling, Keelhaul, and Eastwind. In these instances, again, the deportations were accompanied by the same pattern of desperation, suicides, separation of families, and the frenzy of prisoners reacting to the use of force by U.S. or British military police. By late spring 1947, Stalin had violated every clause in the Yalta Agreement, and the cold war had set in. Very few victims remained to be delivered, the

author insists, and the Allies felt under no further obligations to Stalin. The forcible repatriation ground to a halt in June 1947.

One may well ask why this tremendous activity escaped attention in the Western press. Why there was no public outcry? What were the overriding considerations that underlay the Western powers' zeal to carry out a policy that was "abhorrent to their peoples, traditions, and moral values"; They felt, says Lord Bethell by way of summary, bound by the solemn commitment undertaken at Yalta and at Leipzig. Bethell finds some validity in the practical reasons for the deliveries. There was first a need to protect Western prisoners formerly held in German prisoner-of-war camps and now in Soviet hands. Then the Allies could not afford to arouse the suspicion of Stalin for fear of impeding the war efforts. Finally, there was great concern over the logistical difficulty of absorbing and resettling the large number of anti-Communists in the West.

Bethell feels that the steady barrage of pro-Stalin propaganda to which the Western public had been exposed year after year by the British and American press had led people to believe that to oppose Stalin was tantamount to opposing the Allied cause. Who at the end of the war could have believed that a person might be violently anti-Stalin and yet neither pro-Hitler nor opposed to the Western democracies? The public "knew nothing of the internal affairs of the Soviet Union. Why should they?" (p. 208).

In assessing the responsibility of the British and American military men in the whole process, each of the officers in command, according to the author, places the blame on his superiors, and each defends his action on the grounds that he was carrying out orders. Bethell finds an analogy between the ready explanations of all those involved that they were only carrying out orders and the same plea entered by the German generals on trial at Nuremburg. Though obedience is one of the cornerstones of the soldier's craft, the judges at Nuremburg refused outright to acquit those German generals whose defense for committing cruel acts was that they were only obeying their superiors. The accused were convicted and many were sentenced to death.

Bethell argues that though the prisoners had no claim on Allied loyalty, they were nevertheless human beings. He raises the question of whether we may have arrived at the point of accepting a new standard: that a prisoner of war — whether civilian or military, innocent or guilty — once having been captured or having

surrendered, is at the mercy of his captor.

MILOS MARTIC

University of California, Berkeley

Cyprus: The Vulnerable Republic. By Dimitri S. Bitsios. (Thessaloniki, Greece: Institute for Balkan Studies, 1975. Pp. 223. No price indicated.)

Published originally in Greek in 1973 under the title Crucial Hours, this translation is the latest addition to the growing literature on the Cyprus question. I read this book with great anticipation because the author, currently the Greek Foreign Minister, has been a career diplomat actively involved in the problem of Cyprus since the early 1950s. Mr. Bitsios's book concentrates on the period 1951 to 1965, with the addition of a brief postscript consisting of his October 31, 1974, address to the U.N. General Assembly on the Turkish invasion of Cyprus.

Despite the author's qualifications and knowledge of the problem, this book unfortunately fails to live up to the claims of its publisher and the expectations of the reader. In contrast, other current works on the subject, such as those by C. Christides, (Kypriako Kai Hellinotourkika [Athens: n.p., 1967]), P. Terlexis, (Diplomatia Kai Politike Kypriakou[Athens: Rappas, 1971]), and S. G. Xy dis, (Cyprus-Reluctant Republic Hague: Mouton, 1973]), and (Cyprus: Conflict and Conciliation, 1954-1958 [Columbus: The Ohio State University Press, 1967]) to name a few, provide far better insights into the problem of Cyprus even though these authors never held posts comparable to those of Mr. Bitsios. Certainly, the author brings out in passing some little-known aspects of the diplomatic background of the Cyprus question. For example, the British High Commissioner in Cyprus seems to have encouraged Makarios to formally present his 1963 constitutional revision proposals, which became the catalyst of the postindependence phase of the dispute. Similarly, at the London Conference (January 15, 1964), Duncan Sandys presented a British scheme for the geographical separation of the two Cypriot communities. Finally Mr. Bitsios indirectly mentions what is already known, i.e., that the Greek-Greek Cypriot relationship was frequently tense even in the early stages of the anticolonial struggle. Unfortunately, the reader is left with the task of evaluating the significance of these facts, especially in the context of the developments in and around Cyprus of the last few years.

What disturbs and puzzles me the most, though, is Mr. Bitsios's superficial examination of the Cyprus question. Is the Foreign Minister concerned about embarrassing his associates, who today, as well as during the 1950s, handle the Cyprus problem? Are his ideological convictions such that he finds it necessary to downplay the role of the United States and NATO in this dispute? Note, for example, his implied criticism of the Greek diplomatic tactics during the colonial phase of Cyprus. Yet, instead of presenting alternatives, or showing where and why Greek diplomacy went wrong, he gives the reader sympathetic accounts of Prime Minister Karamanlis and Foreign Minister Averoff. In discussing the crucial 1955 London Tripartite Conference the author tells the reader that his purpose is not to enter in the details of the conference. Why not? Wasn't, after all, this conference one of the most important turning points in the Cyprus question?

The reader is also told that John Foster Dulles sent Greece a message of sympathy in the aftermath of the 1955 riots against the Greeks living in Turkey. Somehow the author has forgotten the content of the identical note sent to both Greece and Turkey by Dulles in the aftermath of this tragic event. Moreover, having defined the concern of the United States in the Cyprus question on strategic grounds, Mr. Bitsios provides no discussion of the aims and methods of American overt and covert diplomacy during the critical 1964-1974 period. It was during this period that the United States actively pursued solutions, through direct mediation or secret Greco-Turkish talks, which proposed to end Cypriot independence through the partition of the island between Greece and Turkey.

When the Greek edition of this book first appeared, Greece was under military rule and censorship. This may explain the author's decision to end his discussion of the Cyprus problem with the events of 1965. There was no reason, however, for the failure of the English edition of this book, published well after the restoration of democratic rule in Greece, to deal with the 1967-1974 period unless Mr. Bitsios wanted to avoid embarrassing Greece and the United States because of their role in the dispute. With the expertise born of his active service in the Foreign Office (through 1972), Mr. Bitsios could have shed some light on this dark and crucial phase of the dispute. He chose not to do so, and therefore this book - most noteworthy in what it does not say

about the diplomacy of the Cyprus question — is a disappointment.

VAN COUFOUDAKIS

Indiana University at Fort Wayne

In Search of a Responsible World Society: The Social Teachings of the World Council of Churches. By Paul Bock. (Philadelphia: The Westminster Press, 1974. Pp. 251. \$10.00.)

The study of international organization often appears to be little more than that of the United Nations, its specialized agencies, and a few European organizations. It is as if the scholarly, functionalist dictum of an initial, low-key involvement has succeeded so well that the very existence of the hundreds of other international organizations (governmental and nongovernmental) is virtually an arcanum to all but a few devotees. The costs for the development of general theory are obvious. Paul Bock's analysis of the social teachings of the World Council of Churches (WCC) offers a small palliative for this scholarly ailment, and, incidentally, an updating of an earlier study by Edward Duff, The Social Thought of the World Council of Churches (New York: Association Press, 1956).

The general object of Professor Bock's attention, the WCC, is a "supranational Christian body" (p. 151) consisting of some 263 Protestant, Anglican, Old Catholic, and Orthodox churches in ninety different countries and territories. The Organization includes, therefore, representatives from the Communist and Third World states, as well as the West. Founded in 1948 (although its immediate antecedents hark back to the Stockholm Conference of 1925 and earlier) and headquartered in Geneva, Switzerland, the WCC has proved to be an important NGO actor in international and transnational politics. The present study offers some idea of why this is so.

Although Bock's numerous and lengthy citations from various ecumenical pronouncements become at times burdensome to his reader, they do illustrate well the WCC's concept of a "responsible" international society: The synthesis of Western values emphasizing freedom with those of much of the remainder of mankind stressing economic and social justice and security. Individual chapters are devoted first to a survey of ecumenical social thought in general and then to specific analyses of the Council's statements concerning different political and economic orders, war and peace, communism, racism, and economic and social development. One theme stressed persistently is that Christianity should neither avoid social involvement nor become identified exclusively with any particular system or bloc.

The analysis is descriptive and chronological. Perhaps because of the overlap in the subjects covered, there are numerous tedious repetitions throughout the text. The reader is told several times, for example, that SODEPAX is an agency through which the WCC and the Vatican leaders cooperate on international social, development, and political matters, and that Tambaram, the site of an ecumenical meeting in 1938, is near Madras, India. One might also note here that the United States exploded its first hydrogen bomb in 1952, not 1950 (p. 102).

Most of the chapters end with a succinct summary followed by a comparison between Vatican and WCC thought on the issues in question. The study contains a short bibliography and is indexed.

If Kant's definition of justice as that behavior which treats human beings as ends in themselves be accepted, one should come away from a reading of Bock with an admiration for the social teachings of the WCC. This is a work that can be useful for students of religion and political theory, as well as international organization.

MICHAEL M. GUNTER

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Decisions in Israel's Foreign Policy. By Michael Brecher. (New Haven, Conn.: Yale University Press, 1975. Pp. xiv, 639. \$25.00.)

This sequel to *The Foreign Policy of Israel* is more ambitious and less successful than the earlier volume. While the first was an exhaustive though flawed clarification of a significant actor in world politics, here the same diligence is applied to several of its major decisions for much less gain to the study of foreign policy than is claimed.

General readers will this time find the going much harder, although there are rewards in the form of thoroughly documented accounts of Israeli decisions on Jerusalem, relations with Germany and China, Jordan waters, and three instances of war in the Sinai. Those who have kept up with the literature will find these accounts familiar, though more meticulously and massively presented than in other places. Its deliberately esoteric side apart, the book's encyclopedic quality is often vitiated by excess. When well-known propositions are established anew by reference to still more evidence, the line between scholarship and pedantry fades.

Political scientists without serious background in Middle East study are likely to be impressed with this volume, for Brecher mirrors particularly well a trend that both excites and troubles the present state of the discipline. Another was more explicit in the precursor.

One is a fascination with tools of inquiry productive mainly of jargon, besetting much contemporary social science, against which Lewis Coser recently warned sociologists meeting in San Francisco. "He spoke of 'embarrassing triviality' and 'enormous ballyhoo' for results 'mostly dross or interminable methodological disquisitions and polemics,'" (NYT, 8/30/75, p. 17). All of these are evident in this volume, much of which is given to announcements of the grand designs accomplished therein, the building of a novel general theory of foreign policy behavior.

Claiming that intellectual inquiry has heretofore been either untheoretical or unrelated to empirical data, the author proposes a synthesis in a "structured empiricism." What follows looks rather like an unrestrained hoarding of data presuming to be scientific. While no datum, however trivial, seems omitted, the "structuring" of the mass is mainly by codification and repeated reprocessing according to an elaborate research design. The codification accomplishes the opposite of simplification and economy. Thus: "The universal actor in the global system (G) was omnipresent. UN Secretary-General Hammarskjöld [repeatedly] visited the area of conflict..." (p. 226). When not just piling the esoteric atop the commonsensical, the author directly translates ordinary, if not trivial, propositions into pretentious language. Thus the notion that Israel had to face relations with China once it had to face relations with China: "The initial stimulus to Israel's first major decision on China was the proclamation of the PRC on 1 October 1949 and its request for recognition from all states" (p. 127). Much of the narrative given initially in each case is further reprocessed under different headings, with many tables still further repeating already articulated propositions in graphic or statistical form.

What the author really produces is a rich but not well-written encyclopedic treatise that is quite useful as such. Anyone interested in, say, who of the top Israeli leaders was most concerned with the Soviet factor in a particular decision, or a similar matter, will find satisfaction in this volume. But I doubt that anyone at all familiar with the literature would really appreciate much material, such as content analyses and tables trying to establish the not very startling proposition that Egypt was upper-

most in the minds of Israelis when that most weighty of Arab nations was most directly involved in a crisis with Israel, along with other equally unsurprising discoveries.

Students of comparative foreign policy will, however, appreciate Brecher's attempt at testing a number of hypotheses current in their literature in the light of his seven cases. This more genuine contribution to theory building elicits two observations. One concerns the nature of the hypotheses, for several seem banal or simply tautological. The other concerns the several strategic and tactical decisions used for testing, which are not necessarily produced by the author's elaborate scheme, but rather are commonsensical derivations from thoroughly familiar events.

Brecher's work also mirrors the tendency in the discipline to presume global validity for parochial, Western conceptualization. Students of non-Western civilizations are invariably bemused by the inclination of political scientists trained in international relations to explicate phenomena in societies about which they know next to nothing in terms that would make sense only in their own. They thus identify "moderates" or "rightists" in wholly inappropriate circumstances or take for granted rationality as they understand it, where neither motivation, values, or basic mentality is essentially the same, or even remotely similar. Attitudes of leaders in violent, pre-industrial, rigidlysectarian societies are almost invariably interpreted as if they were consensus politicians in Western countries. Just as Alexander Woodside has recently marveled at the massive treatment of Vietnamese affairs by analysts who disdained a knowledge of Vietnamese history, so one discovers also that a lack of serious study of the Middle East characterizes the authors of what prestigious foreign policy journals usually publish on that area. Being informed, in this genre, means accepting the current journalistic and diplomatic lore, often just cliche conceptions of reality.

If Israeli foreign relations could be understood by studying only Israelis, Brecher's work could be faulted mainly for misplaced methodological zeal, for he has been exceedingly diligent in researching that side. But one cannot really study the Arab omnipresence in such a problem by researching Israelis. Accepting the guidance of Israeli diplomats (as Brecher indicated in an article in The New Middle East) is not an adequate substitute for serious study of the Arab factor. That he and other scholars should follow diplomats rather than appropriate area scholars, is in the case of the Arab-Israeli conflict further motivated by the pre-

ference they share with diplomats for rationality, bargaining, and compromise. International affairs generalists and diplomats persist in viewing that conflict as indeed amenable to such means, if only the elusive formula were discovered, while the consensus of Orientalists is hardly favorable. Their inclination to exaggerate greatly the variability in attitudes toward the Jewish state in the Arab world is mirrored in Brecher's mocking reference to "the Arabs" throughout. This is curious behavior, especially where particular insistence is voiced on the importance of sober perceptions in international relations.

GIL CARL ALROY

Hunter College, City University of New York

La Coexistence Pacifique. By Philippe Bretton and Jean-Pierre Chaudet. (Paris: Armand Colin, 1971. Pp. 328. No price indicated.)

The authors, both French jurists, have described the history of diplomatic relations between the Soviet Union and the United States with exactitude, constantly striving to give, in a small volume, the greatest possible amount of information (even, for example, references to internal jurisprudences).

They do not employ any of the fashionable concepts adopted today by the science of international relations, notably American, but abide by a very traditional, very "French," idea of the study of international phenomena which they describe in the most unbiased manner possible. Moreover, the book's bibliography is almost exclusively French; English literature is completely ignored, with, for example, only one book in English cited on the conflict in Vietnam

This very factual presentation has, however, a positive aspect: the events described in plain terms are neither falsified by theoretical discussions, nor obscured by a complicated and tedious vocabulary. This attitude also has the advantage of not tempting Professor Bretton and Professor Chaudet to speculate on the future. By purely and simply describing the events (often recent), they act as historians and history goes out of fashion less quickly than international political science.

Nevertheless, their book does not avoid the inconveniences inherent in all studies concerning constantly developing news: it is no sooner written than it is out of date. Think what has transpired since the publication of this book in 1971. The signature of the S.A.L.T. agree-

ments, the C.S.C.E. and the M.B.F.R., the Sino-American reconciliation, the Yom Kippur War, and, of course, the petroleum crisis have greatly contributed to the increase in the ambiguity of the meaning of coexistence, to which little consideration have been given recently.

In general, the work lacks internal cohesion since the events described are juxtaposed without any overall interpretation. This important weakness stems from the limited attempt to explore in depth the theory of the notion of peaceful coexistence (pp. 25-44) and, therefore, that of the cold war. Although no precise definition is given, the authors appear to consider that peaceful coexistence refers to the evident or latent desire of the Soviet Union and of the United States not to come to armed conflict.

Such a view appears to be very limited. On the one hand, it implies a lack of knowledge of the field of application of this notion. Adopted by popular consensus by the nonaligned countries, coexistence not only refers to the two superpowers but it actually represents the present goal of international relations in general. On the other hand, at no point do the authors attempt to trace the roots of the doctrine. It is extremely arbitrary to fix its origin as they do, at the beginning of the 1950s; the decree on peace made by Lenin in 1917 could no doubt be considered its first manifestation, and the division of the world into two main sociopolitical systems may be considered concomitant with peaceful coexistence, notably its economic implications (p. 20).

The book is divided into three parts. The first is devoted to the aftermath of the Second World War and, curiously enough, of the cold war. The second deals with the limitation imposed by the major powers on nuclear armament by international society. The third treats the "unarmed" conflict between the United States and the Soviet Union.

The comparison of the first and the third parts leaves the reader perplexed. Any classification of this nature is extremely arbitrary. However, taking into account the pragmatic policy adopted by the authors, a purely chronological presentation and a classification by type of problem or crisis would, no doubt, have been more appropriate, and their goal would have been more easily attained.

In conclusion, the specialist will no doubt find certain topics for reflection but will regret the weakness of certain analyses, notably those concerning Berlin and China. On this latter subject, the authors state that the People's Republic of China "rejects" the idea of peaceful coexistence (p. 34). This virtually unsupported statement is rather surprising: China, at Panch Sila in 1954 and Bandung in 1955, was one of the most ardent defenders of this doctrine and has on many occasions since confirmed its nominal position.

In spite of its weakness and some debatable aspects, the book presents, for the neophyte, a clear and serious introduction to a study of Soviet-American relations and is a quite useful reference book for a specialist. One should not expect anything more than this, and readers who are looking for theoretical explanations, original ideas, or intellectual stimulation will be disappointed.

ALAIN PELLET

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Diplomats in Crisis: United States — Chinese — Japanese Relations, 1919—1941. Edited by Richard Dean Burns and Edward M. Bennett. (Santa Barbara, Calif.: Clio Press, 1974. Pp. 346. \$15.00, cloth; \$4.95, paper.)

This is the story of thirteen men - five Americans (Cameron Forbes, Joseph Grew, Stanley Hornbeck, Nelson Johnson, John Mac-Murray), five Japanese (Hirota Köki, Matsuoka Yosuke, Nomura Kichisaboro, Shidehara Kijūro. Shigemitsu Mamoru), and three Chinese (Chou Fo-hai, Hu Shih, Wellington Koo) - in search of solutions to the mounting crisis in East Asia in the period between the wars. They were generally second-echelon diplomats, not top-level decision makers, although Shidehara and Matsuoka might be viewed as exceptions. "Well-meaning and competent," as Professor Norman Graebner describes them in his useful introduction, dedicated to the national interest as they saw it, they sought peaceful solutions, not war. Since the Sino-Japanese war and Pearl Habor were the end product of the period, their efforts ended in failure. Why the failure and to what extent were they implicated in it? These are two of the major questions raised in the Introduction. Were these men too imperceptive of the forces operating in the world about them? Did they fail to communicate effectively? Were they largely messengers, or did they seek to make an imprint on the policies their governments pursued? Were they, finally, overwhelmed by forces which they were powerless to control?

While the essays do not address themselves directly to all of these questions, some generalizations can be gleaned from the text. A failure

to transcend parochial viewpoints and to comprehend the motivation of others is certainly one. Hornbeck's insistence, to the very end, that sanctions against Japan would not produce war because all rational calculation showed she must lose, is a case in point. His misconception, however, was no greater than that of, say, Matsuoka, the self-proclaimed expert on American psychology, who insisted that a tough policy, which included adherence to the Tripartite Pact, was the key to reaching accommodation with the United States.

To what extent should the diplomat be more than a message conveyer? From Tokyo, Grew (like Forbes before him) was increasingly apprehensive about the collision course on which U.S.-Japanese relations appeared to be headed. His warnings and suggested alternatives not only fell on unreceptive ears in Washington, they evoked criticism from Grew's superiors which led him to pursue a more cautious approach. Could — or should he have tried to speak out more forcefully? One raises the question in the age of the Pentagon Papers; Grew's course was to look to the historical record for vindication.

On the other hand, the example of Admiral Nomura could be taken as an illustration of an overly personal role. Convinced that neither government truly wanted war, and dedicated to preventing the final rupture, he occasionally allowed his own objectives to temper what he communicated between Washington and Tokyo. While the essay devoted to him does not stress the point, his actions produced a rebuke from his own government and, at times, appear to have furthered misunderstanding rather than to have reduced it.

Compared to the foregoing, Ambassador Hu Shih's experience reads like a success story. Here is the diplomat as "communicator" in another sense. Leaving more traditional embassy matters to others, he spent his time cultivating support for the Chinese cause in American public opinion, which he saw as an important ingredient in American policy. Not all diplomats, however, can enjoy the favorable juxtaposition of circumstances which existed for him—his own personality and background, the relative accessibility of the American public, the pro-Chinese predisposition of American opinion.

Professor Hilary Conroy, in his essay on Nomura, raises a fundamental question. Did these men, regardless of their views, make any difference? Did they in any perceptible way influence the course of events or were they helplessly swept along by them? His answer, rather tentatively put, is yes (as he rightly points out, a contrary answer would make this book a futile undertaking). Had there been a more favorable response at the right time, the Nomura mission might have succeeded. One can disagree with the example and still reject a deterministic approach. But just how did these men interact with circumstances? Herein lies a problem with these essays: the approach is not truly "transnational" (p. viii) but remains largely one-dimensional, and, if as the Introduction would have it, domestic factors were the major determinant of policy, the individuals too frequently appear in a vacuum, remote from the context of events in which they were involved.

Interpretative and critical, the essays are well written and maintain a uniformly high quality unusual in collections of this kind. Brief sketches of the social background and attitudes of the subjects are often highly perceptive. A political scientist interested in studies of elites, in bureaucratic behavior, or even in psychological approaches to politics should find suggestive material here.

WILLARD H. ELSBREE

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Kto i kak delaet politiku SShA (Who Makes Policy in the USA and How it is Made). By Sergei B. Chetverikov. (Moscow: Izdatelstvo Mezhdunarodnye Otnosheniia, 1974. Pp. 329. Rubles .98.)

This is a landmark volume in the Soviet study of American politics because it utilizes Watergate to introduce some long overdue corrections into the conventional Soviet interpretation of the American political system.

For many decades Soviet analysts have maintained that as economic power becomes more concentrated in fewer hands in the United States, so does political power. Institutionally, that is reflected in the decline of Congress and the hypertrophy of the presidency. The fatal inevitability with which these processes transpire has been the subject of numerous Soviet studies.

Chetverikov, a specialist on executive branch organization and politics who earlier wrote under the pseudonym Sergei B. Marinin, undertakes to explain how the inevitable was temporarily reversed when an increasingly assertive Congress and a series of negative court decisions effected the resignation of a president attempting to expand his powers.

The author's often implied but never explicitly stated conclusion is that power is more diffused in the American political system than the conventional Soviet wisdom allows. Certain legal or formal aspects of American government which Soviet analysts had considered to be unimportant or subterfuges concealing the real concentration of power now must be taken more seriously. The principle of the separation of powers, for example, was used with telling effect against President Nixon by the Congress, and the system of checks and balances really worked. Whereas Soviet writers had largely ignored the role of the independent judiciary. Watergate demonstrated its importance. Even the Soviet tendency to underrate the options made possible by the two party system has to be reconsidered since that system created obstacles to the growth of presidential power.

Chetverikov says, in effect, that the Soviets must treat as really significant the very structures and processes of American government that most American analysts would consider to be its strengths. On the other hand, he identifies what he thinks will be the basis for an eventually much greater concentration of power in the presidency.

Before the increased power can be attained, however, the president must overcome one great obstacle, and the analysis of this process is Chetverikov's unique contribution to the study of the presidency. The president must contend with the "triple alliance," a gradual coalescing or fusing of the congressional staffs, the middle-level executive branch bureaucracy, and the bureaucracies of the large corporations. He responds by trying to impose his control over the federal bureaucracy through increasing the coordinating powers of his executive office, a phenomenon Chetverikov dubs "bureaucratic centralism." Simultaneously, the large corporations attempt to control the executive branch through increasing their representation on the numerous consultative and coordinating committees. Whether this brings big business into conflict with the president, as seems apparent, the author does not say.

Even though Chetverikov does not trace the concrete steps in resolving the conflicts, he does project the president's prevailing in them, and so comes to the conclusion characteristic of Soviet Marxist futurology: power will be concentrated in the hands of the executive.

Chetverikov has given an only very partial answer to the questions posed in his title. But he has also raised some intriguing, challenging, or fanciful (depending upon one's tastes in these matters) possibilities in the study of American politics — or, more particularly,

politics of the bureaucracies, by the bureaucracies and for the bureaucracies.

RICHARD M. MILLS

Fordham University

East Asia and U.S. Security. By Ralph N. Clough. (Washington, D.C.: The Brookings Institution, 1975. Pp. 248. \$8.95, cloth; \$3.50, paper.)

This is an extremely well-written, thoughtful, and informative study that fills a large gap in the literature on the post-Indochina War context for U.S. policy in Asia. The book benefits from Clough's many years in the Foreign Service, his meticulous mind, his careful pen, and his provocative probing into the assumptions that have underlaid U.S. policy for the past two decades. Its contribution at various levels of understanding makes the study suitable for the undergraduate and the senior specialist as well.

The major portion of the book deals with U.S. relations among the Big Three of East Asia: China, the Soviet Union, and Japan. These relations are re-examined in each of their bilateral sets and in specific problem contexts such as Korea and Southeast Asia. The historical background of each interaction is concise, and the future perspective is dispassionate. Clough sees Japan as central to U.S. security interests in East Asia but does not expand this to advocate Japanese rearmanent, much less advise Tokyo to acquire nuclear weapons. On the contrary, he believes that the joint security arrangements should continue with whatever modifications are necessary to sustain Japanese political participation but argues against a qualititative or quantitative change in Japan's military participation because of the effect on Chinese and Southeast Asian perceptions.

During 1975 the Ford administration worked persistently to repair the damage of "Nixon shocks" to Japanese-American relations, with some success. Clough thus overstates the degree to which the alliance is threatened from within. Less visible but no less significant is the more subtle change in Japanese attitudes toward Korea since his writing. My conversations among informed officials and intellectuals in November 1975 contrasted with those of a few years previous in that assurances were now forthcoming of sufficient political support to permit U.S. military use of bases in Japan on behalf of the Republic of Korea should it be attacked from the North. Such opposition as might still occur would probably not be as serious as Clough states.

In view of his extensive China background

perhaps the most disappointing discussion concerns Taiwan. Clough asserts:

Should the United States break relations with Taipei, abrogate the defense commitment, and set Taiwan adrift, the people of Taiwan could be compelled to accept control by Peking.... As the Chinese thrust military forces into the Pacific close to territory disputed between Japan and China, and the United States in the face of military threat abandoned fifteen million people whom it had pledged for over twenty years to help defend, the shift in the power balance could not fail to make a deep impression in Japan ... [and] immediately raise grave questions about the reliability of the U.S. defense commitment to South Korea and increase the risk of conflict there (p. 140).

This "worst case" possibility lacks a detailed, credible scenario for Peking's attainment of Taiwan, collapses the time frame so as to juxtapose a U.S. disengagement with a "military threat," and adds Korea, where — unlike Taiwan — the U.S. maintains a military presence and has fought one war for its interests there. Moreover, while Clough suggests there is little likelihood that the People's Republic will resort to force, he fails to consider how this might be rendered even less likely through bilateral understandings or unilateral moves by the United States.

Despite the book's title, Clough includes Southeast Asia in his focus, on the proper premise that the Indochina War expanded to include the U.S., and to a lesser extent China and the Soviet Union, because of the prior interaction of the larger powers in East Asia. Although this portion of the book is minor, it makes a major contribution in systematically exploring the degree of future Chinese threat, which Clough discounts, and the consequent level of American interest and recommended involvement, which he reduces to nil. Except for a few outdated portions written before the total collapse of the U.S. position in South Vietnam and the subsequent emergence of Hanoi's disputes with Peking over influence in Cambodia and islands in the South China Sea, these two chapters make salutary and trenchant reading.

Any effort to summarize the political situation in Asia during so dynamic a period as 1973-74 runs the risk of rapid obsolescence. The only serious instance of this phenomenon concerns China's sudden emergence as a producer and exporter of oil, particularly to Japan. This development weakens Clough's negative assessment of Peking's prospects for earning foreign exchange as well as for strengthening its relations with Tokyo at the expense of Moscow. In all other respects, however, the book is

likely to stand the test of time extremely well, at least until Washington completes the normalization of relations with Peking and thereby terminates its political-military relations with Taipei.

ALLEN S. WHITING

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The United States and Latin America: An Historical Analysis of Inter-American Relations. By Gordon Connell-Smith. (New York: Halsted Press, 1974. Pp. 302. \$16.75.)

Connell-Smith holds the Chair of Contemporary History at the University of Hull in England. His earlier book on The Inter-American System (1966), with Jerome Slater's The OAS and United States Foreign Policy (1967), remains one of the two best studies of the emergence and evolution of inter-American organization before the substantial eroding of the "special relationship" between the United States and Latin America.

In this new volume Connell-Smith has broadened his scope to general inter-American relations from the eighteenth century through 1973. He tells his readers that he has explored further sources of information, including documentary materials that relate primarily to Mexico's participation in the Organization of American States, and that he has made "fresh visits to the United States and countries of Latin America, during which [he] was able to discuss [his] subject anew with leading scholars and men prominent in public life" (p. ix).

The resulting "contemporary history," at least to this reviewer, is somewhat disappointing. Connell-Smith comments: "I have tried to view the events of [the 1960s and early 1970s] in a wider historical perspective, and at the same time — as a complementary part of my historical analysis — to view earlier events in the light of the new perspectives which more recent events have induced. Unremitting comparison between historical phenomena occurring at different points in time is an essential feature of a contemporary approach" (p. xvi). Unfortunately, the product of this approach in this instance is not entirely satisfying either as history or as current analysis.

As history, the study is at best a thoughtful survey of the major events of some two hundred years of inter-American relations. Forty pages are devoted to the thirteen years since the Cuban Revolution, which is some indication of the detail that the author has had to sacrifice simply because of his extended time frame. Had he focused on a single era, as did Bryce Wood in his two classic books on the period of the Good

Neighbor Policy, Connell-Smith might have had a greater opportunity to do pathbreaking historical research.

Nevertheless, Connell-Smith argues, his study does blaze new trails in that his "approach is more critical — and realistic — than that customarily adopted by United States historians towards their country's policies" (p. xii). In his view, his work offers an important corrective to the assumptions of S. F. Bemis and other traditional U.S. historians that "there exists an essential harmony of interests between the two Americas" and that "in spite of many — even serious — mistakes, their country's intentions have been good" (p. xiii). Indeed, he maintains, "the need is apparent for a reappraisal of [United States] Latin American policy" and "to such a reappraisal, [he] would like this study to contribute, in however modest a degree" (p. xvii).

Yet Connell-Smith advances no real prescriptions for "improved" U.S. policies and his "critical" perspective offers few insights that have not been common to most of the recent literature on inter-American relations. The most successful chapter in his book (Chapter I on "The Background"), in fact, neatly summarizes prevailing academic wisdom about the divergent interests of the United States and Latin America and capsules the increasingly familiar debate as to whether Washington has a unified Latin American policy or one fragmented by bureaucratic politics. In a sense, Connell-Smith has sought to characterize his study as revisionist by setting up a straw man - the traditional historian - as his target. He acknowledges that "in recent years, many North American scholars - social scientists ominously more prominent among them than historians - have written very critically on various particular aspects of inter-American relations and on United States policies" and that he has "quoted extensively from their work" (p. xiii). Whatever he intended, he has not added much to what has already been said.

Broad in scope and largely sound in its interpretations, this volume would make a good text. But the hefty price of the sole hardback edition will preclude widespread use in the classroom.

YALE H. FERGUSON

Rutgers University-Newark

Vienna Broadcasts to Slovakia, 1938–1939: A Case Study in Subversion. By Henry Delfiner. (New York: Columbia University Press, 1974. Pp. 142. \$10.00.)

The book contains translations of 51 of the

estimated 170 radio broadcasts in the Slovak language transmitted by Radio Vienna under the Nazi control in the period immediately preceding Hitler's destruction of what was left of Czechoslovakia after Munich. The purpose of these Slovak broadcasts was to lure the Slovaks away from the Czechs and toward the Germans.

The modest merit of this book is to assemble some valuable raw materials on the subject of an episode in the Nazi psychological warfare; the translations of the Vienna broadcasts constitute the "bulk" of the slender book. These materials may be perhaps of some use to some future scholar devoted to content analysis or communication theory neither of which seems to be Professor Henry Delfiner's forte.

The initial assumption of this case study was that the Vienna broadcasts had had a significant impact on their Slovak listeners. The opposite seems to be the case: there was no "wholesale defection by Slovaks of the endangered nation" (p. 6); "the Vienna Slovak broadcasts and other German propaganda ventures did not succeed in setting in motion a genuine Slovak independence movement" (p. 47). Discarding an original assumption under the weight of contrary evidence is certainly not a drawback of any case study; in fact, it happens all the time. In this case, however, the author seems reluctant to admit and explain the very limited role the Vienna broadcasts (so laboriously assembled and translated from Slovak to German to English) had finally played. It seems that their sheer existence (as proof of the Nazi determination) rather than their contents might have helped to convince the Slovak leaders, who were anti-Czech but not pro-German, that their resistance to the Nazi policy of divide and conquer was simply useless.

The methodological weaknesses of the book are obvious. The author has not attempted to assess the quantity and quality of the Slovak responses to the Vienna broadcasts; this might have been done by interviewing the surviving listener-witnesses either in Slovakia or among the Slovak exiles in the United States or the author's native Vienna. Rather than intuitively reading between the lines of the Vienna broadcasts and so only guessing, which is always dangerous, about the psychological warrior's assumptions, the author should have attempted a rigorous content analysis. If, for instance, one makes a very rough count of the three major themes in the broadcasts, as stressed by Henry Delfiner, namely (1) anti-Czech, (2) Slovak pride-and-right, and (3) anti-Semitic themes, there are forty-nine anti-Czech references, forty-three exclusively Slovak concerns, and eighteen anti-Semitic ones. Professor Delfiner claims that the Vienna broadcasts merit our special attention on account of their use of anti-Semitism for the first time to win a non-German people over to the Nazi side; this reviewer wonders whether a comparative analysis might not show that, long before the Vienna broadcasts, anti-Semitic themes had figured prominently both in the Nazi broadcasts beamed to West Europe and in the financial support of the right-wing press in France (Gringoire and its infamous "rather Hitler than Léon Blum"), Belgium (Rex movement), and England (Sir Oswald Mosley's fascists). Without a comparative perspective, or quantitative/qualititative assessment of the Slovak listeners' reaction, the study is of little utility to a specialist and too narrow in its focus to appeal even to those historians of World War II and who still delight in the discovery of the various footnotes to that

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Advancing and Contending Approaches to the Study of Chinese Foreign Policy. Edited by Roger L. Dial. (Halifax, Nova Scotia: Centre for Foreign Policy Studies, Dalhousie University, 1974. Pp. 412. No price indicated.)

A group of scholars of various nationalities has produced here a collection of essays proceeding from the assumption that theory and methodology have made considerable progress in the social sciences in general but not yet in the field of "sinology" (a term used in this book to denote the study of contemporary China, although it is more properly applied to the study of traditional China), and that it would be desirable to eliminate this "China differential" - to employ a term devised in another context. The alternative and plausible assumption - that a great deal of theory and methodology in the social sciences, political science included, may be an inappropriate importation from the physical sciences, and one that physical scientists themselves regard as unscientific neoscholasticism, so that what emerges is neither politics nor science - seems not to have been seriously entertained.

The result is a study replete with references to and quotations from other writings on Chinese foreign policy and with formulas and tables designed to yield models, but with only a modest amount of substantive analysis. Although it is debatable whether this approach "advances" the study of the subject very much, the book deserves to be taken seriously because

the subject is important and interesting and because the approach is fashionable.

Robert Boardman (Chapter 1), after an interesting analysis of the various principal views of the subject adopted by Chinawatchers ("sinologists"), concludes sensibly that 'no unique conceptual framework is needed" (p. 35) and that students of international relations theory and the foreign policies of other states would benefit from paying more attention to Chinese foreign policy.

James Oliver, Robert Boardman, and Roger Dial (Chapter 2) perform content analysis on the interpretations of Chinese foreign policy of a number of American and British Commonwealth writers and conclude that the former are the more prone of the two groups to infer causation of the phenomena described and the less prone to attribute significant causal force to internal developments.

Sang-woo Rhee (Chapter 3) defends the need, in the study of Chinese foreign policy, for theory, the alternative to which in his eyes is "common sense." This is a false dichotomy, since specialized knowledge of the subject, whether accompanied by theoretical generalization or not, cannot be reasonably considered or termed common sense unless the words are twisted to mean something quite different.

Another chapter by Rhee (4) attempts to apply Rummel's field theory to the subject at hand. With the aid of many formulas, it arrives somehow at the shattering error that over the period from 1950 to 1965 there was "no systematic change in any kind of Chinese foreign policy behavior..." (p. 147). If this is science, give me "common sense."

Charles W. Kegley, Jr. (Chapter 5), attacks the view (held by no one so far as I am aware) that the foreign policies of the Asian states are completely different from one another and tends toward the opposite error — i.e., the view that they are all virtually alike.

Jan H. Kalicki (Chapter 6) discusses the behavior of the People's Republic of China and the United States during their crises with one another (none of which happily has occurred for about a decade) and concludes sensibly, after considerable theorizing, that each side has improved the sophistication of its performance over time.

Robert R. Simmons (Chapter 7) applies alliance theory — the term alliance being used in a broad, nontechnical sense — to the interaction among North Korea, the Soviet Union, and China during the Korean War, which he correctly views as a difficult process for those concerned. He also develops the interesting hypothesis that the Soviet Union behaved at the

United Nations in early 1950 in a way calculated to reduce the chances of Peking's admission.

Roger L. Dial (Chapter 8) applies organization theory to the study of the formulation of Chinese foreign policy in opposition to the "totalitarian" or "monolithic" approach, which he rightly says is discredited and which in fact almost no one holds to.

Robert Boardman (Chapter 9) concludes that "studies of perception and cognition at the individual, interpersonal, and organizational levels can throw a good deal of light on Chinese foreign policy" (p. 343). Perhaps, but his essay asserts the proposition rather than demonstrating it.

Roger L. Dial (Chapter 10) discusses the interest group approach to the study of Chinese foreign policy, with a good sense both of its utility and of its limitations. The same comment applies to Douglas Johnston's essay (Chapter 11) on Chinese treaty behavior.

Geoffrey Lambert (Chapter 12) deals inconclusively with the relevance of elite theory and studies to the analysis of Chinese foreign policy.

One of the useful features of this book is that some of the contributions include notes and bibliographies that taken together form a helpful, although by no means complete, guide to the secondary (almost never the primary) sources on Chinese foreign policy.

HAROLD C. HINTON

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A Global Approach to National Policy. By Richard A. Falk. (Cambridge, Mass.: Harvard University Press, 1975. Pp. 320. \$12.50.)

The scholarly work of Richard A. Falk, Milbank Professor of International Law at Princeton University, is distinguished both by originality and by the use of legal theory as an adaptive tool in the solution of contemporary political problems. A Global Approach to International Politics, however, is an uneven collection of essays, only one or two of which represent Falk at his illustrious best.

The book consists of four parts. The first, the framework, treats the state system under siege, lawyers in the conduct of foreign policy, and problems of denuclearizing global politics. Part Two deals with what he calls "wider lessons of Indochina." Part Three, "Analytical Modes," which deals with the United Nations and geopolitics and world order, is by far the best section in the book and should be read by all political science students interested in law and international relations. Part Four, "A

Vision for the Future," apart from being excessively dependent on the mistaken conclusions of the Club of Rome, is an interesting but inconclusive schema for studying the future.

Part Two of the book presents Falk at his worst. Many distinguished scholars share Falk's high valuation of the Nuremberg trials. My own view is closer to that of Justice Stone, who had only contempt for Justice Jackson after his participation in the trials. Rustem Vambery, the distinguished Hungarian jurist, pointed out that the rules at Nuremberg had as much in common with customary international law as the sign of a professor at the University of Texas, who having learned that by custom only dons are permitted to walk on the greens of Oxford, posted a sign in Texas stating that, henceforth, by custom only professors would be permitted to walk on the grass. The reasonable ends of wartime diplomacy could have been achieved by application of the laws of war, which were well recognized in international law, by political acts of the victorious powers as part of their war aims, and by the appropriate use of German law, including, although it was probably not necessary even if constitutional, ex-post-facto laws. Instead, the Russians who had slaughtered 10,000 Poles at Katyn and the Americans who had engaged in the fire bombings of Hamburg and Tokyo found the Germans guilty of violating international law and capped the "legality" of the trials by exculpating the Germans for their violations of the prewar naval protocols because the British had also violated them. The principle really established was that in large wars victors try the vanquished. The danger in this, of course, is that it may make it much more difficult to limit and to terminate wars. In a nuclear age, this could produce catastrophe.

Things get even worse when Professor Falk seeks to apply the Nuremberg precedent to the trial of Karl Armstrong, who bombed the Army Mathematics Research Center at the University of Wisconsin, thus causing the death of Robert Fassnacht. Professor Falk tries to be reasonable. He points out that acts of violence will lead to acts of counterviolence; and, therefore, he believes that Armstrong should not have been entirely exculpated. However, Falk believes that the sentence should have been mitigated because Armstrong believed that he was protesting a violent and illegal war that could not effectively be opposed otherwise (a real howler in terms of democratic theory). Surely Professor Falk knows that many people believe that it is simple nonsense to argue that the war was illegal. Indeed, if any state violated international law, it was North Vietnam for, at the

time of the incident in Wisconsin, the organized armies of North Vietnam were waging the war against a South Vietnam that clearly had legal personality under international law as well as a right to support from its allies. Moreover, even newspaper reporters now understand that the National Liberation Front was ruled from Hanoi by agents who were officials of the northern party.

Should the judge, prosecutor, and jury have taken this into account and perhaps have convicted Mr. Armstrong of first-degree murder under the reasonable man doctrine? Even if Armstrong believed the building to have been empty, he knew that people sometimes did work late at night and intended an explosion that could kill a person in that event.

Juries and judges do properly take into account ordinary character and motivation in arriving at sentences. To extend this principle to crucial political motivation is dangerous. Factually such extensions do occur, as in the case of the juries in Ohio that refused to convict those responsible for the atrocities at Kent State. Think how much it would damage the system of domestic law were we to write into jurisprudential philosophy the legitimacy of political sympathy in the imposition of penalties at law. It would politicize the law and legitimate its repressive use. Law may need to be tempered by moral concern, but Professor Falk's partisanship sometimes leads him to deplorable conclusions.

MORTON A. KAPLAN

University of Chicago

Neither Peace Nor Honor: The Politics of American Military Policy in Viet-Nam. By Robert L. Gallucci. (Baltimore: Johns Hopkins University Press, 1975. Pp. 187. \$10.00, cloth; \$2.95, paper.)

The appearance of Robert Gallucci's book marks a watershed in the American literature on the Vietnam War. With it we pass from accounts by polemicists, participants, and journalists to relatively detached reappraisals by young scholars who never observed it directly. Neither Peace Nor Honor is, I believe, the first full book about the war derived from a doctoral dissertation by such a student. Also, as Gallucci himself indicates (pp. 1-7), we have passed from the conventional wisdom of the "quagmire" (Arthur Schlesinger's Bitter Heritage) to the revisionist conventional wisdom of the "stalemate machine" (Daniel Ellsberg and Leslie Gelb), both of which were forged in and for the heat of home-front battle, and we are now more calmly ready to borrow the new conventional wisdom of "bureaucratic politics" (Graham Allison et al.) to explain America's 1960s Vietnam involvement. No doubt we will soon have a stream of Vietnam books that are as much or more concerned to advance empirical political theory as they are to wave policy flags of any color; such is the nature of academe and the succession of generations. Gallucci's version starts the flow unevenly, but the example he gives is on the whole auspicious, considering the stage in his development and that of the field.

Gallucci's difficulty, I think, is that he, like some others trying to use Allison's Essence of Decision for various purposes, has got himself tangled between "Model II" and "Model III." In effect, Gallucci gives an elegant Alison "Model II" interpretation of American military organizational interests and routines as d_ctating the 1965-68 course of the air war over North Vietnam and of the ground war in the South; but Gallucci is much less successful, it seems to me, in adapting Allison's "Model III" to explain top war-policy making in Washington 1961-68 as it shifted from the Kennedy to the Johnson administrations. Part of the problem is that Gallucci does not even try to distinguish explicitly between Allison's Model II and Model III, lumping them together as "bureaucratic politics," whereas Allison had at least tried to separate "organizational process" (Model II) from "governmental (bureaucratic) politics" (Model III). To be sure, the boundary grew fuzzy in Allison's work as he moved from organizational routine (clearly Model II) toward organizational interest, which could overlap "players" interests (Model III); Gallucci may have felt that avoiding Allison's fuzzy distinction was a desirable simplification. But the effect, in my judgment, is to diminish somewhat the conceptual clarity of Gallucci's excellent account of field operations (mainly Model II) and to mire him in Allison's other chief area of conceptual muddiness: how primus inter pares is the Model III President?

This trap is the most threatening to the "bureaucratic politics" school. Morton Halperin (Bureaucratic Politics and Foreign Policy) struggled to escape it by first writing twelve president-centered chapters to show how decisions get made and then writing three bureaucracy-centered chapters to show how they get evaled; despite the artificiality the impact on balance was probably "presidential" (quite primus). Allison himself wavered from chapter to chapter, perhaps not entirely consciously; but I would say that the overall impact was "bureaucratic" (not very primus). Gallucci, it seems,

would like to rate the importance of the President's personal "intellectual baggage" even lower. Gallucci, however, is dealing with two strong-willed presidents, Kennedy and Johnson; and he is too responsible a scholar to distort the evidence. His solution is to write an account which repeatedly veers toward an overemphasis on the autonomous importance of the president's advisers (the Vietnam war "principals"), yet as often checks itself in a kind of informal dialectic, and finally reaches a loose synthesis in the proposition that Kennedy's relative moderation about Vietnam was both cause and effect of his keeping some doves in his entourage to balance the hawks, and that Johnson's immoderateness was both cause and effect of his not doing so.

The difficulty here is that Gallucci wants to put the emphasis too heavily on the word "effect" in that proposition. Thus he veers: "In fact, a credible argument could be made that by not accommodating views markedly different from those put forth by his military advisers, President Johnson gradually put himself in the position of a leader without significant options — not a position he or any other president is likely to have contrived to attain" (p. 57). But then he checks himself:

When decisions are most difficult because the consequences are so grave, it is human for the man who has the ultimate responsibility to seek as much united support as he can gather, rather than to cultivate views that can sound like heresy before a decision and become a political liability after it. So common sense may make us less optimistic about the possibilities of preventing the isolation of future presidents, but the Vietnam experience should convince us that even partial success would make the effort worth while (pp. 134-135).

Amen to that normative conclusion. But the empirical case remains moot since Gallucci has avoided "the game of trying to figure out when President Lyndon Johnson really decided to bomb North Vietnam. That particular moment of truth will be left to others to ferret out" (p. 41). By another (not well documented) Gallucci argument, the president's decision to bomb made so as to forestall domestic opposition to a prior private decision to send ground troops. So without having even tried to determine just when Johnson resolved upon this general escalation, Gallucci was in no position to judge whether "hunkering down in a bunker," with those who would and would fit there, was or was not a conscious presidential choice, one that might have persisted from very early in Johnson's administration.

Without at least confronting this historical

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puzzle, Gallucci could scarcely have expected that a bureaucratic politics model would lead to crisp conclusions about the differences between the Kennedy and Johnson administrations about Vietnam. And it is to Gallucci's credit that he is not procrustean. He knows he has taken us only part of the way in empirical political theorizing about the Vietnam war. It is, on balance, a good start. The next useful step may be to focus on the presidents themselves, perhaps adapting John Steinbruner's cognitive processing paradigm from *The Cybernetic Theory of Decision*.

H. BRADFORD WESTERFIELD

Yale University

The United States Against the Third World: Antinationalism and Intervention. By Melvin Gurtov. (New York: Praeger Publishers, 1974. Pp. 288. \$9.50, cloth; \$3.95, paper.)

If the 1960s witnessed the traditionalist/behavioralist split, the 1970s are facing the behavioralist/radicalist "great debate." In French social science literature, contributions of Amin, Emmanuel, Palloix...tip the balance in favor of the radicals, but in English the battle is undecided. Hence the importance of Gurtov's book.

Gurtov begins by explicating his objective: to add "to the respectability of the critical and radical literature" (p. ix). How? Through the study of four American administrations' fifteen cases of intervention in the Third World, the author analyzes the conflict between "imperial foreign policy objectives" and "... the official sympathy with nationalism ... and political change" (p. 202).

Chapter I discusses three interrelated axioms of U.S. interventionist credo: 1) America's domestic tranquility depends on security and stability abroad; 2) security and stability abroad for the forces of freedom depend on America's willingness to carry out its mission; and 3) fulfillment of America's mission and security depends on a willingness and ability to intervene in the domestic affairs of other peoples (pp. 4-9). The four subsequent chapters substantiate this by treating the Eisenhower administration's interventionism in the Middle East, Kennedy's in Africa, Johnson's in Latin America and Nixon's in Asia. The last chapter sums up "a number of commonalities in the American perception": e.g., 1) political and social instability in the Third World is a breeding ground for radical leftist forces; 2) history's lessons amply justify an imperial foreign policy. When does U.S. intervention move from a perceptual to a behavioral datum? Two primary and three secondary conditions are explicated: i.e., 1) necessity of a revolutionary milieu; 2) a milieu perceived as destabilizing in its policies or programs to other countries; 3) low risk of direct Soviet or Chinese counterintervention; 4) domestic (U.S.), regional, or international authority sanctions U.S. intervention, and 5) a "representative" of local political authority invites it.

Contrary to Gurtov's statement (p. 1), the study's organization is chronological and regional, but his thematic thesis assures continuity. Recent conceptual/methodological issues are ignored (p. 206), yet a global systematic framework (with Third-World regions as its subsystems) coupled with an explicit comparative perspective would be more reflective of U.S. policymakers' perception and values, help identify similarities/differences among intervention cases, increase the study's conceptual unity and consolidate the persuasiveness of the global interventionism thesis.

For instance, the 1954 Czech arms deal with Guatemala justified U.S. action to topple Arbenz. Nasser's 1955 Czech arms deal was certainly more "destabilizing," and yet he continued in power. Why did not U.S. policymakers do in the Middle East what they did in Latin America? Is it geographical proximity as Gurtov seems to imply (p. 82)? But since Gurtov mentions a combination of military, economic, and political reasons and since "cold war attitudes" condition U.S. global behavior, readers would have been better off with a comparative elaboration. Instead the two subsystem cases are not even cross-referenced.

Similarly, Gurtov's solid part on U.S. intervention in Vietnam discusses the "bureaucratic dimension" (pp. 155-158); but because of the absence of a "bureaucratic politics" approach or a conceptual alternative, the reader faces noncumulative statements, and is unable to see the bureaucracy's precise influence or its specific frontiers. Since Gurtov and – for instance – Allison discussed both the bureaucracy and intervention in Cuba, the reader would be interested in knowing Gurtov's views on Allison's findings. Allison is not mentioned even in a footnote. Equally, a basis of the study is U.S. "national interest," and even though the concept is defined perceptually (p. 206), Gurtov does not discuss, for instance, George's "operational code" approach or Holsti's findings of its application to J. F. Dulles.

This aversion to established (establishment?) conceptualization, nondiscussion of its pertinent findings (throwing away the baby with the bath water?) and the weakness of counterconceptualization lead to predominantly de-

scriptive parts, e.g., two pages on U.S. policy in South Africa and Rhodesia (pp. 79-81).

These limitations, however, should not detract from the study's strong points. 1) The main thematic thesis is pursued systematically. and the supporting evidence varies from tabulated data to telling quotations. The result is that if some area specialists would not find much new about their own area issues, analysis of similar issues in other areas is still rewarding. Precisely, a conscious comparative perspective would have given the book something more than the mere sum of its parts, 2) And because Gurtov writes such good prose, he condenses much material into limited space (e.g., p. 145). 3) The research itself is solid. Computation based on a random sample of the book's footnotes indicates that the ratio of primary to secondary sources is approximately 3:1. Moreover, because Gurtov intends his analysis to be policy relevant, policymakers would find his study enriching and thought-provoking. Students tired sometimes of methodological-conceptual 'hair-splitting' would find Gurtov's book an enjoyable change. Involved political scientists convinced of the soundness of Jurtov's main theses, would have liked this bock to be proof that "radical" political analysis and relevant conceptual-methodological refinements are not mutually exclusive. Hopefully, we are at the threshold of this "radical"/"scientific" symbiosis that should characterize the post-behavioral era.

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The National Archives and Foreign Relations Research. National Archives Conferences, Volume 4. Edited by Milton O. Gustafson. (Athens: Ohio University Press, 1974. Pp. xvii, 292. \$10.00.)

This volume contains papers prepared for and proceedings of the Conference on "The National Archives and Foreign Relations Research," sponsored by the National Archives and Records Service on June 16–17, 1969, and is part of a series designed to "increase contacts and improve relations between the scholarly community and the National Archives." Altogether, twenty-three short papers are presented, along with brief excerpts from conference discussions.

The uninitiated researcher studying U.S. diplomatic history will get from this collection some sense of the organization of relevant archival collections of the government, their availability, the problems associated with mak-

ing the best use of them, and to a degree, indications of which areas and historical periods seem most fruitful for future research. Although it suffers from several limitations and deficiencies which reduce its utility, this volume does belong in the reference collections of serious research institutions.

As is almost always true for any conference or symposium, the papers are of uneven quality, and of varying relevance for the intended topic, which in this instance was "the problem of finding a relevant topic and the resources needed to study that topic." The first seven papers, each presented by a member of the National Archives staff, are descriptive commentaries on the holdings of the Archives which bear on U.S. foreign relations, and may be the most helpful part of the volume for those who have not previously used these materials. Included are discussions of document collections from the State, War, and Navy Departments; of captured foreign records; of materials in presidential libraries; of nontextual records; and of neglected materials from nonforeign-affairs agencies which are nevertheless important for some types of foreign relations research. These presentations are hardly exhaustive, but they do supplement usefully the general and specialized guides prepared by the National Archives (listed in "General Information Leaflet No. 3, Publications of the National Archives and Records Service," which can be obtained from the Publications Sales Branch, National Archives, Washington, D.C. 20408).

A second section deals with publication of records on United States foreign relations, specifically the microfilm program of the National Archives, and the Foreign Relations series prepared by the Historical Office of the Department of State. William M. Franklin's paper on the latter is of particular interest, providing insights into problems which will be encountered as the time period covered by the series reaches the postwar period, and into the relationship between documents published in the series and the much larger number which are not, but which become available through the Archives when the companion Foreign Relations volume is published.

The remainder of the volume is concerned with archival materials bearing on substantive research questions, including domestic influences on foreign policy, the diplomacy of war and peace, United States relations with Europe, with Latin America, with East Asia, and with the Middle East and Africa, and the administrative history of the Department of State. Elmer Plischke, in an extensively documented piece, argues that the latter topic is deserving of more

attention than it typically receives, even when the researcher's primary topic is substantive policy and not institutions and processes per se.

Unfortunately, a number of these papers make only passing reference to how the holdings of the Archives can best be employed for research on the topics or geographic areas covered, or to the types of questions for which these materials are particularly important, choosing instead to present brief surveys of existing research, or samples of research drawing upon archival sources. This is frequently interesting, but it reduces the value of these contributions as general guides for researchers working in the topical areas in question.

Several of these papers, however, deserve specific mention. In a lucid essay entitled "Rewriting the History of American Diplomacy during the Second World War," Gaddis Smith asserts the necessity of using multiple sources in a needed effort to add subtlety to interpretations of the American role before, during, and after World War II. Norman A. Graebner draws heavily upon official records to qualify substantially the frequent conclusion that American foreign policy in the nineteenth century was essentially isolationist. Robert Freeman Smith presents a review of the major themes in recent archival research relations between the United States and Latin America in the twentieth century, accompanied by an extensive discussion of Latin American materials in the archives, and of problems encountered in archival research. These include the locating of documents and the need to correct for the perspective gained if only one nation's records are employed in studies of relations between it and another state. Finally, Morris Rieger's survey of sources in the Archives relating to African-American relations should facilitate better use of materials which are among those most neglected. Because a number of useful general points are made in the course of discussing specific topics, the researcher contemplating a project drawing upon the National Archives should range widely through this volume, rather than confining his attention to those papers which seem most germane on the surface.

A final section of the book contains the conference keynote address of Foy D. Kohler, "Reflections of a Professional Diplomat," on the relationship between history, historians, and diplomats.

This volume leaves the reader with the

helping to improve the usefulness of the Archives' collections for students of foreign relations. Sadly, at least judging from suggestions contained in the rather sketchy summaries of discussions at the various sessions, little concrete help was forthcoming. This undoubtedly attests to the difficulties inherent in finding solutions to problems of indexing, classifications, and access, particularly when resources are limited, but it may also suggest that there was a natural tendency for each of those attending the conference to focus on irritations encountered in his own research rather than on the more general problem of how the Archives can be structured to be of greatest use to the widest range of scholars.

The staff of the National Archives should be commended on its efforts to improve the dialogue with users of its materials, in the series of conferences similar to the one described here. Four other volumes in this series, reflecting four other conferences, have been previously published, and at least four others are to follow. But it is unfortunate that since five years elapsed between the conference and the publication of this record, much of the information about current research and about procedures for access to materials in the Archives is outdated. This difficulty is only partially corrected by the editor's brief updating. And although a number of the papers make reference to guides prepared to aid the scholar in using these collections, a current brief status report on both the general National Archives Guide and available specialized finding aids should have been included, along with information about how they can be obtained.

This volume represents a good idea flawed in execution. It will heighten the interest of many scholars in using archival records, but it will provide them with only a limited amount of assistance in doing so.

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Cultures in Conflict: The Four Faces of Indian Bureaucracy. By Stanley J. Heginbotham. (New York: Columbia University Press, 1975. Pp. xii, 236. \$12.50.)

Explanations of bureaucratic behavior primarily in terms of cultural variables are rare in the literature of political science. Fred Riggs

of cultural characteristics. But the book was not easy to digest, and very few followed up his suggestions. At the same time, although unrelated to Riggs's effort, Michel Crozier (The Bureaucratic Phenomenon [Chicago: University of Chicago Press], 1964) attempted to explain aspects of French bureaucratic behavior by reference to cultural patterns in France. James Abegglen (The Japanese Factory [New York: Free Press, 1958]) did the same sort of thing for Japan. There hasn't been much else. Now, Professor Heginbotham has done it for India. His book is important because it is one of the rare attempts in the literature to treat aspects of culture as independent variables in the analysis of bureaucratic structure and dynamics.

The venue for the analysis was the Indian State of Tamil Nadu (formerly Madras State), and more particularly within that State, the North Arcot District. The evidence for the study was collected during field research in 1967 and 1968. The specific focus was on that part of the development administration of Tamil Nadu which was responsible for implementing a program to popularize the use of high-yielding varieties of rice in the villages. The descriptions of the patterns of bureaucratic behavior in Tamil Nadu are enlivened and made most convincing by numerous accounts of the author's personal research experience in Tamil Nadu while he was collecting his material.

In order to explain the behavioral patterns he observed, the author constructs four different models which he calls cognitive models of organization. Each model attempts to catch a different constellation of norms and values about three interrelated sets of cognitive orientations: first, perceptions of the broad goals and dynamics of an organization; second, perceptions of the motivations, loyalty, and responsiveness to various incentives by people working at different levels within the organization; third, perceptions related to the clients the bureaucracy is supposed to serve. The four models are identified as traditional Indian ("dharmic"), traditional Western ("British colonial"), modern Indian ("Gandhian"), and modern western ("community development"). The differences and similarities among these cognitive models are used to explain some of the problems involved in implementing the development program in Tamil Nadu. For example, one of the major problems was the reluctance to allow the extension workers in the villages sufficient flexibility to be able to respond effectively to the individual requirements of different villages. This problem is explained in terms of the cognitive models. In both the dharmic and British colonial models, the general goal orientation is the maintenance of balance and order in the system, whereas in the community development and Gandhian models the goal is to bring about change. In consequence, in the dharmic model duties undergo little change, they can be specified in detail, and little initiative is required in their performance. The British colonial tradition's elaborate rules and regulations laying down precise procedures to be followed are the functional equivalent of these dharmic prescriptions. Where such cognitive models reign, community development programs requiring flexibility are difficult to implement. Thus, "... the community development movement was swamped by the pervasive domination of colonial and dharmic models of compliance systems. The result was not simply a routinization of procedure, but a severe and progressive deterioration of performance, integrity and morale" (p. 172). Frequently throughout the book, cultural variables are used in this way to explain bureaucratic behavior and performance.

Much of the analysis hinges on the four cognitive models; yet it is precisely here that the author will perhaps seem least convincing to both the India expert and the student of comparative politics. The India expert will no doubt focus on his dharmic model. Heginbotham devotes considerable attention to its salient features, and leans heavily on it in his analysis. But there are severe problems in attempting to press the entire range of traditional Hindu values into a single cognitive model. Nor is the notion of dharma sufficient in and of itself to incorporate all that is important in the Hindu tradition; after all, dharma is only one of a conventional fourfold classification: artha, kama, dharma, moksha. Furthermore, the Indian cultural tradition is not only relevant in the villages, as is suggested. One could go on, but the point is that a single model for Indian traditional culture is probably too blunt an instrument for analysis. Indeed, one of the problems with such cultural explanations is to get agreement on how to conceptualize what the culture is. For the student of comparative politics, critical attention will focus on whether or not the author has made headway in overcoming the enormous problems in establishing categories that permit the comparison of similar aspects of two cultures without grossly distorting the cultural reality in any one. The author is alive to the complexities involved, and candidly reviews the difficulties that have been encountered in comparative cultural studies. The construction of four cognitive models of culture (rather than just one) and the way the contradictions and reinforcements between them are used in the analysis are valuable and suggestive, but I fear that the author does not take us much further with the problems of comparison across cultures.

These problems, however, should not obscure the fact that this book is a major achievement. It provides one of the best general descriptions of bureaucratic behavior in India that I have seen. The framework of analysis is highly original. It is also relevant to important issues in comparative politics.

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Arab and Israeli Elite Perceptions. By Daniel Heradstveit. (Oslo: Universitetsforlaget Publishers and New York: Humanities Press, 1974. Pp. 146. \$9.50, paper.)

Professor Heradstveit's principal goals are to arrive at an estimation of "the possibilities of a settlement of the Arab-Israeli conflict" and to contribute to the understanding of Middle Eastern conflict and conflict in general. To accomplish these tasks, ninety-four representatives of Egyptian, Jordanian, Syrian, Lebanese, Palestinian, and Israeli "elites" were interviewed by the author in order to generate data on the perceptions, goals, problems, strategies, and minimum negotiating conditions of the main parties involved in the conflict.

Invariably, assessments of the perceptions of parties involved in serious conflict tend to yield depressing catalogues of misperceptions. The Arab-Israeli conflict is no exception. Among the classical misperceptions revealed in Heradstveit's data are: (1) cultural and ideological stereotypes; (2) underestimation of the adversary's sense of threat; (3) reference to past events referred to as evidence that both sides will eventually reach their goals (at the same time, each party also gives a very optimistic evaluation of its own situation with a correspondingly pessimistic evaluation of that of the adversary); (4) strong tendencies to ignore information which does not fit with the established image of the adversary - in addition, the regarding of extremist groups or extremist signals that have very low credibility as indicating the intentions of the adversary as a whole; and (5) the tendency to describe the intentions of one side's Great Power backers as being much more sympathetic than the intentions of the adversary's backers, while both sides maintain exaggerated notions about the dependency of the adversary on their respective backer states.

Vis-a-vis the possibilities for negotiations, Heradstveit finds internal struggles among the competing strategies of political solutions, military solutions, and status quo (neither war nor peace) maintenance. The author concludes that within the Arab world, both the political and the military strategies have strong followings and that the relative strength of the two alternatives fluctuates with the general political situation, the strength of the guerrillas, and the calculations concerning the impact of negotiations with Israel on internal political stability. Further complicating the Arab side of the conflict are the different and contradictory interests (summarized by the author as territorial retrieval vs. reduction of the conflict level) perceived by different Arab actors. For the Israelis, the parallel struggle is between negotiated settlement and status quo maintenance, with the latter appearing more attractive since the expected gains from negotiation appeared to be slight. In sum, Heradstveit observes that some potential for negotiation has been present and that the outlook for negotiation will probably depend on changes in the distribution of Arab-Israeli capabilities and the development of a widespread feeling that the political and military costs of maintaining the status quo are too high.

The book achieves some success in realizing its goals by presenting data-based information on the cognitive dimension of an important conflict which has not been known for producing an abundance of objective and empirical studies. Nevertheless, the book definitely has its liabilities. Perhaps most fundamental is the "elite" foundation upon which the interviews were based. Very little attention is given to who belongs to the elite sector in the Middle East or for that matter, what role elite opinion plays in the Middle East. Instead, Heradstveit interviewed a "purposive sampling" (outlined in the table below), which often seems to represent a euphemism for talking to whomever one can. Given the difficulties of doing field research in the Middle East, particularly on this sensitive topic, this result may be about as much as can be realistically expected. Still, the reader is left with the distinctly uncomfortable feeling that while the conclusions seem fairly accurate, it is not clear to what extent the ninety-four respondents represent elite opinion in their respective countries nor whether the respondents are cross-nationally comparable. Add to this the following problems: (a) the Arab interviews were conducted in March, 1970 (before the Jordanian suppression of the Palestinian guerrillas) and the Israeli interviews on March, 1972; (b) different respondents were apparently asked different questions; and (c) nowhere in the study are the data summarized in tabular form (the reader is given only a list of the questions and the author's interpretations and conclusions). The end result is an intriguing study that has restricted value because of the several problems in the research design and the reliability and validity of the data obtained. Furthermore, the study is only very weakly connected to the more general literature on conflict misperceptions and not at all to other studies of the Arab-Israeli conflict.

Finally, Heradstveit's book may be partially dated by the impact of the 1973 October War on elite perceptions and the possibilities for negotiations. This is an empirical question which requires further analysis. It is especially unfortunate, however, that the various sins of omission and commission associated with this study probably foreclose what might have been an exceptional opportunity to create a panel for follow-up surveys.

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National Monetary Policies and International Monetary Cooperation. By Donald R. Hodgman. (Boston: Little, Brown & Co., 1974. Pp. 266. \$12.95.)

Problems of a World Monetary Order. By Gerald M. Meier. (New York: Oxford University Press, 1974. Pp. 305. \$4.95, paper.)

A reviewer of books on monetary matters may be tempted to consider publications prior to 1973 as of historical interest. The year exuded a sense of before and after. Before the Yom Kippur War of October and the oil price hike of November, Finance Ministers could assemble at Nairobi to discuss the IMF Committee of Twenty's "First Outline of Reform" and set a deadline of July 31, 1974, for

agreement on the issues still outstanding. Since the war, Finance Ministers have either hedged on deadlines or concluded that the present system's resilience has been proved in practice.

To judge by Professor Hodgman's National Monetary Policies and International Monetary Cooperation, and Professor Meier's Problems of a World Monetary Order, the two approaches were latent in the old regime. "Mystery," "maze," and "muddle," writes Meier in the Preface, "are the words that most frequently follow 'international monetary.' But international monetary. national monetary problems are continually being resolved one way or another" (p. vii). More soberly, Hodgman concludes that "no system of regulation that applies uniformly to the entire (European) Community can remove the problem of reconciling diverse interests among individual members. Systems applied by individual countries are inconsistent with the goal of a unified Community-wide capital market as a main feature of monetary and economic integration" (p. 237).

Perhaps the nuances of difference between the two authors derive from differing foci and objectives. Meier's book is aptly conceived for teaching in the classroom and provides a broad brush introduction to international monetary experience since Bretton Woods. Hodgman's book specializes on the structural variety of Western European capital markets and monetary policies. Understandably, his judgments in respect of solutions are more reserved.

One thread common to both authors is the increasing subordination of domestic policies to the exigencies of world trade and capital flows. To some, the Bretton Woods system's failure to provide adequate liquidity restrained economic expansion in major industrial nations and impeded development in the poor. Fixed exchange rates were untenable as capital flows expanded and growth or inflation differentials induced changes in the international hierarchy of states. To others, the system was never

Heradstveit's Purposive Sample

	Active Politicians		Civil Servants		Editors/ Journalists		Professional Students		Guerrilla Leaders		Others		Total	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
Egypt	2	7.4	4	14.8	12	44.4	3	11.1	6	22.2	0		27	28.7
Jordan	1	9.1	1	9.1	0	_	4	36.4	3	27.3	2	18.2	11	11.7
Syria	0	_	1	33.3	0		0		0		2	66.7	3	3.2
Lebanon	2	13.3	0		2	13.3	9	60.0	1	6.7	1	6.7	15	16.0
Israel	7	18.4	3	7.9	9	23.7	15	39.5	NA	-	4	10.5	38	40.4
Total	12	12.8	9	9.6	23	24.5	31	33.0	10	10.6	9	9.6	94	100.0

Note: This table is constructed from information given by the author (pp. 135-137) in his list of respondents. Palestinian respondents are apparently attributed to the country in which they happened to be interviewed.

implemented. The United States played banker to the world economy and the dollar became the international medium of exchange. By the time that President Nixon declared the dollar's inconvertibility into gold in August 1971, foreign states were so implicated in the dollar's fate that they had no alternative to accepting the consequences of America's new economic policy.

An international approach to the monetary history of the 1960s - interestingly documented by Meier - starts with the rapid change from dollar gap to glut as capital movements were liberalized and the deficit on American capital account mounted. As dollars accumulated in European central banks, doubts grew with President de Gaulle's active encouragement - at the United States ability to honor its commitments. Each attempt to shore up confidence in the dollar aggravated Atlantic relations. Department of Defense arguments on liberal access to European arms markets were contrasted to its nationalist procurement policies. The success of interest equalization tax in discouraging foreign borrowers on to United States capital markets was set against the limited success of voluntary or direct controls on direct overseas investment by American corporations. With the institution of the twotier gold market in March 1968, and the first discussions on the Special Drawing Right in the IMF as the new unit of account, speculative pressures began to build up.

As one financial crisis succeeded another learned discussions were held over whether the United States should adjust to the growing lack of confidence in the dollar by devaluation against gold, or whether the dollar "hemorrhage" was not providing the world with needed liquidity. The first school of thought tended to stress the asymmetrical advantage that the United States derived from maintaining an overvalued currency through the purchase of foreign companies or the expansion of American overseas holdings. The second school of thought pointed to the benefits in trade which the United States was foregoing by maintaining an overvalued dollar and to the necessary burden of leadership in a rudderless world. Power politics and finance fostered many a mixed metaphor.

Imperceptibly, though, the Western world slid into a regime of floating exchange rates as no government — and especially the American — could stem capital movements in response to interest rate differentials. Only skeptics fastened on the fact that fluctuating exchange rates accentuated asymmetries between trading nations according to the degree of national

production engaged directly in the pattern of world exchanges. Meier's book fairly presents both viewpoints.

Hodgman's originality lies in tracing decreased government control over domestic economic environments from the viewpoint of the mixed economies of Western Europe. He studies in fascinating manner the various monetary and credit structures of Western European states, and illuminates detail with reference to the theoretical underpinnings of government objectives. He places himself squarely on the ground of the contribution of government monetary management to the attainment of the four major goals of national policy: full employment, growth, price stability, and balance of payments equilibrium.

A primary distinction drawn by Hodgman is between the implications for policy structures and instruments of monetary and credit management. Where policy emphasis is on the control of monetary aggregates, government direct intervention in the processes of credit markets is minimal. "By contrast, concern by the authorities with the structure of interest rates and credit flows always involves forms of administrative intervention into credit markets to modify the structure of interest rates and the allocation of credit that otherwise would result from market processes" (p. 199).

The paradigm of success in aggregate monetary management is obviously West Germany where the central bank has sought with increasing success to regulate liquidity of commercial banks and other credit institutions. Inflation rates are the lowest in Western Europe, as closer government controls since 1967 over budgetary policy has resulted in a freer hand for more vigorous monetary policy. The challenge to effective monetary policy from international capital flows was finally met in 1973 by the imposition of stiff disincentives to foreign speculators on the DM's revaluation and of draconian deflationary measures.

Hodgman provides a quarry of information and fresh insights into the difficulties confronting national monetary management in Western Europe. He carefully explains how Italian efforts to allocate credit have been thwarted by the growing need for central government finance. His account of France's giving priority to credit allocation and control of interest rates for privileged circuits illuminates the growing inflationary pressures in the French economy and the insistence of Paris on a Europe of the Fatherlands where each nation is responsible to its own development. His devastating critique of British capital policy is summarized in the pithy sentence: "The complexity of this

(United Kindgom) network of markets and institutions led the Radcliffe Committee to refer to the British financial system as sophisticated" (p. 206).

I cannot resist the temptation to recommend Professor Hodgman's book as indispensable reading for my (British) compatriots. He places the problem of Britain's national debt - accumulated over two world wars from 656 million pounds in 1913 to 27,000 million pounds in 1960 - at the center of British monetary dilemmas. He doubts the efficacy of the 1971 Credit Control "reform" by asking whether the government will be prepared to finance the government debt including new deficits by noninflationary means, accepting in this process the interest rates on government debt determined by market forces. "Attempts to stabilize the market price of long-term government debt hampered the execution of monetary policy in the United Kingdom throughout much of the past two decades... By contrast, repudiation of government debt inherited from the years of World War II in Germany has as one result the removal of a comparable constraint on the vigor of monetary policy in that country" (p. 214).

Curiously, Hodgman ends on a plea for comprehension for new forms of supranational organization. Yet he remains skeptical at European efforts to "harmonize" their varied national monetary and credit policies. Perhaps his distinguished career as teacher, author, and consultant to the Federal Reserve Board of Governors and to the United States Treasury has led him to verbal circumspection. That is not least of the attributes of this excellent book.

JONATHAN STORY

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The Middle East and North Africa in World Politics: A Documentary Record. Volume I: European Expansion, 1535—1914. Second Edition, Revised and Enlarged. Edited by J. C. Hurewitz. (New Haven, Conn.: Yale University Press, 1975. Pp. 616. \$30.00.)

This volume of Professor Hurewitz's enlarged documentary collection is twice as big as volume one of its predecessor, *Diplomacy in the Near and Middle East* (Princeton: Van Nostrand, 1956), and twice as useful. Of the seventy-odd new documents, about fifty concern Morocco, Algiers, Tunis, and Tripoli; nine are on Afghanistan. These areas are added to the original coverage of "non-Soviet southwest Asia," including Iran, plus Egypt and the

Sudan. There are also new documents on Iran and the Ottoman Empire. Of the 184 entries (some of two or more documents) all but 40 are post-1800.

The major problem in this edition remains the same as in the first — the general exclusion of the Balkans. That peninsula, like most of the real estate Hurewitz covers, was once within the Ottoman Empire, and was a focus of contention among the great powers; its omission means that the Eastern Question cannot be seen whole. Hurewitz does restore articles of the treaty of Kücük Kaynarca on the Balkans, and adds the treaty of Bucharest (1812). But the treaty of Arianople (1829) is missing, and the treaties of Paris (1856) and Berlin (1878) appear in part only. This principle of selection, emphasizing European imperialism in Islamic lands, is the editor's considered choice.

Each document is preceded, as in the first edition, by a concise and well-written note outlining the historical context and referring to scholarly bibliography. Many notes have been extensively revised. For instance, the French-Ottoman treaty of 1535, accepted in the first edition as an accomplished fact, is here correctly called a draft, lacking the Sultan's ratification. The bibliographical references are also updated, and a new thirty-page bibliography appended. There is no index, but a good table of contents. Unfortunately some editorial gremlin, which let a good many typographical errors slip by, also allowed incongruent page listings in the table for documents 38 through 41.

The documents are principally treaties of peace, commerce, navigation, alliance, or other formal agreements between governments, both Middle Eastern and European. There are also texts of capitulations, economic concessions, fermans, and statements of policy (mostly British), plus some other tidbits like first-hand French and Ottoman reports on the Algerian fly-whisk incident of 1827. All in all, this is a well-edited and most valuable compilation.

RODERIC H. DAVISON

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Vision Accomplished? The Enigma of Ho Chi Minh. By N. Khac Huyen. (New York: Collier Books, 1971. Pp. 377. \$8.95, cloth; \$2.95, paper.)

The incredible Ho Chi Minh, as he was called by his best-known alias, completely blended cosmopolitan and local roles over a long epoch greatly influenced by his own achievements. Inevitably his extremely varied dramatic experiences engendered many myths. Professor Nguyen Khac Huyen, chairman of the Department of Political Science at the College of St. Catherine in St. Paul, Minnesota and a Vietnamese who witnessed the early stages of the First Indochina War, diligently attempts to explicate the enigma of Ho's life history as it was intertwined with modern Vietnam's tragedies, triumphs, and myths.

Other biographies — e.g., those by Jean Lacouture, David Halberstam, Reinhold Neumann-Hoditz — and public relations tracts from Hanoi have provided details of Ho's life despite his famous comment to Bernard Fall that an old man should be allowed to have a few secrets. Huyen provides greater detail, though some facts remain secret and others remain debatable. He supplements the description of historic events with accounts of incidents he witnessed personally as a child — such as the 1946 National Assembly election — thereby amplifying important developments other biographies have treated sparsely.

Huyen stresses Ho's perseverance in pursuing a consistent plan from 1911 to 1969. Ho survived decades of Communist ideological conflicts, Hong Kong and Chinese prisons, frequent illness, the Mao-Chiang break, brutal policies of Chinese warlordism and French colonialism, the Tonkin maquis, and party Vietnamese debacles in 1930-31 and 1940-41 for which Huyen assumes Ho was responsible. Ho's 1945-46 cooperation with the French could have resulted in his assassination by Vietnamese non-Communist forces, by the Chinese, and particularly by dissident Viet Minh. Ruthlessness was a factor in Ho's survival. According to Huyen's account, Ho sold Phan Boi Chau to the French and had countless village elders assassinated, a generation of opposing Vietnamese leaders eliminated, and untold numbers of highlanders hunted down after 1954. Ho's strategy in both Indochina wars emphasized the tactical advantages of civilian casualties. This strategy led to deliberate actions by the Viet Minh to maximize the numbers of civilian casualties and to remake society by destroying the existing social and economic order.

As Huyen's biographical information proves, Ho, despite the enormous suffering in Vietnam resulting from his strategy, was unquestionably the most renowned hero in Vietnamese history. As a result of Ho's revolutionary leadership throughout Asia and later specifically in Indochina, his image took on legendary proportions. Independence (doc lap) was the foremost Vietnamese goal, and Ho, the incorruptible revolutionary, epitomized it. Nonetheless, invariably a major question carefully analyzed by Huyen has been presented frequently as the principal

enigma of Ho's life: was he primarily a nationalist or an international Communist? The enigma is analyzed in detail for each major phase of Ho's life in Huyen's biographical account. Huyen documents information on Ho's leadership of the entire Southern Bureau of the Comintern's South Asia Section, on his active role in numerous third International Asian and European leagues, on his actions as representative to the Comintern and Kresintern and as an advisor to Borodin, and as a student at the University of Toilers of the East. Huyen traces Ho's career as a founding member of the French Communist party and the Indochina Communist party and subsequent fronts and as a recruiter for revolutionaries and espionage agents in Siam, Hong Kong, China, and Europe. Huyen's theme is that although Ho's Communist credentials were unquestionable they were always questioned by many French leaders, Vietnamese nationalists, and scholarly Western accounts until Ho's death in 1969 because many observers refused to believe he could be simultaneously a nationalist and a Communist.

Huyen carefully documents the fact Ho was also a thorough-going Vietnamese chauvinist and expansionist. In fact Huyen reinforces my own observations it was Ho and not Ngo Dinh Diem who was in Denis Warner's terms the last Confucian. As Huyen shows, Ho, the son of a disappointed mandarin, received the mantle of the emperor in the popular view. He did this by being the father and sage possessing superb recognizable tuong (proper facial features) and by being the stern disciplinarian who sacrificed all for the classic Vietnamese ideal of a struggle for a united nation always advancing south and west. Huyen proves Ho retained this respect despite serious government failures of the Hanoi regime between 1954 and 1969 described in the biography.

Perhaps what Huyen proves more conclusively than other biographers is that Ho never doubted his vision would be accomplished. Huyen's title question seems to have been answered. Yet in fact probably the vision was broader than Vietnam, a theme Huyen alludes to often, and Ho's successors will in time fulfill that broader vision as Vietnamese push outward under leadership of hosts of cadres of new mandarins utilizing interlocking associations common both to Communists and to Vietnamese. That may be the answer to the enigma.

CHARLES A. JOINER

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China, Pakistan and Bangladesh. By J. P. Jain. (New Delhi: Radiant Publishers, 1974. Distributed by South Asia Books. Pp. v, 264. \$14.00.)

Indian and Pakistani authors writing about Sino-Bangladesh, Indo-Pakistan, or Bangladesh-Pakistan relations can hardly overcome the built-in biases which permeate the political atmospheres in their respective countries. Both Pakistani and Indian writers usually display a lack of scholarly detachment and objectivity, their works being heavily loaded with patriotic partisanship. This tendency is even more evident in the recent literature provoked by the Bangladesh issue. Thus, the reading of "an objective and scholarly study of Chinese relations with Pakistan and Bangladesh" (as the flap of the book under review rightly claims) by an Indian scholar is a refreshing experience.

In the first half of the book, Dr. Jain delineates how the pressure of events rather than cool-headed calculation brought mid-'sixties China and Pakistan closer. From the very beginning, he tells us, Pakistan made overtures to China to develop some sort of understanding. Although alignment with the West was the sheet anchor of Pakistan's foreign policy in the 'fifties, the rulers of Pakistan were successful in convincing China that the Western alliances were not in any way directed against China; and, as a result, Peking adopted an attitude of noninvolvement in the crucial Kashmir issue. Dr. Jain notes that Pakistan became disillusioned with the Western allies after the Sino-Indian border war in 1962 when, ignoring Pakistan's protest, both the U.S.A. and the U.K. together with the U.S.S.R. rushed massive military aid to India. This response led Pakistan rulers to a fundamental rethinking of Pakistan's whole pattern of relations with others. The Chinese got alarmed and wanted to have close allies in the immediate neighborhood to avoid complete encirclement. According to Dr. Jain, the interests of Pakistan and China converged, leading to the Sino-Pakistan boundary agreement in March 1963, to the China-Pakistan accord on air-service in August 1963, and to Peking's all-out support of Pakistan's stand on Kashmir in February 1964.

Dr. Jain then argues that Chinese support encouraged Pakistan to send infiltrators to India-held Kashmir leading to the Indo-Pakistan conflict of 1965. China's threatening posture, however, prompted the two superpowers — the U.S. and U.S.S.R. — to take joint action in bringing about the ceasefire.

Dr. Jain again argues persuasively that Russian proposal for "Asian collective security"

exacerbated Peking's fear of encirclement and led Peking to entice Pakistan again to her side and that new relationship developed among Peking-Islamabad-Washington following Dr. Kissinger's secret visit to Peking via Islamabad emboldened Yahya Khan to use military force in East Pakistan. Dr. Jain, however, takes pains to dig deep into the Chinese official pronouncements in order to prove that the Chinese tirades were mainly directed against Russia. He points out that "in emphasizing a reasonable settlement ... the attitude of China was not very much different from other superpowers - the United States and the Soviet Union" (p. 175) and that. "China has always cherished profound friendly sentiments for the people of Bangladesh" (p. 201).

Dr. Jain rightly concludes that China would continue to cultivate Pakistan because "without a foothold in Pakistan, China would be facing the solid phalanx of the U.S.S.R.-Afghanistan-India single-handed" (p. 231), but China would also keep her options open toward Bangladesh, a strategically important area, bordering near the two fingers of "the Chinese Tibetan palm."

Dr. Jain does not have the elegant style of Maxwell (India's China War [London: Jonathan Cape, 1971]) or Rowland (A History of Sino-India Relations: Hostile Co-existence [London: D. Van Nostrand Co., 1967]). The format, printing, and paper of the book are also inferior to those of the treatises on similar subjects published in the U.S.A. and U.K. These limitations of the book, however, do not detract from the substantive point that it is one of the few in-depth scholarly studies of Chinese involvement in the South Asian scene.

TALUKDER MANIRUZZAMAN

University of Dacca, Bangladesh

India and Disarmament: Volume I, Nehru Era, An Analytical Study. By J. P. Jain. (New Delhi: Radiant Publishers, 1974. Pp. 211. \$10.00.)

Universal and complete disarmament, regular run-of-the mill disarmament, arms control, arms limitation and nuclear test ban and nonproliferation, were the various captions used to describe the international negotiations dealing with the arms race since Hiroshima and Nagasaki. As these very names given to the successive efforts clearly suggest, there has been a steady decline in "idealism" and a corresponding elevation of "realism" in the disarmament goals the world community set for itself. Mutual rivalries and suspicions among nations,

especially between the erstwhile Communist camp and the Western bloc, militated against disarmament. Neither the U.S. nor the U.S.S.R. was willing to "lower the guard" unless the other took the first move, and there was not much chance for progress till the atmosphere of suspicion and hostility was itself contained to manageable proportions. The tortuous and complex disarmament negotiations reveal any number of reversals and contradictions in the posture adopted by nations from time to time. J. P. Jain, Visiting Associate Professor, Department of Disarmament Studies, Jawaharlal Nehru University, seeks to tell the intricate and often exasperating story from the vantage point of recounting the "role" of one nation. Although not a big power herself, India under the leadership of Nehru took an active part in the world efforts to reduce the threats to peace and to live with the bomb. India's nonalignment helped her to play the role of an intermediary and a catalyst in the East-West negotiations during the cold war years.

As Jain points out, India did not hesitate to support one side or the other when it suited its interests. India worked for the avoidance of nuclear confrontation during the postwar era because the noncombatants were not immune from the "fallout" and other consequences of a nuclear war. It was India's hope and belief that if the arms race were curtailed, more resources would be available for economic development—the single most crucial goal of the nation.

From the very beginning, India consistently held the view that progress in arms reduction and disarmament would be impossible unless the nations that had the atomic weapons - the Big Powers – were willing and ready to act. India also insisted that no meaningful progress could be achieved unless China were drawn into and actively involved with the disarmament negotiations, as Jain documents carefully. The two key elements of these negotiations have been the chicken-and-egg duo: which should come first, disarmament or international inspection and control? This fundamental impasse disrupted many disarmament negotiating sessions. What is ironic is that the West and the East reversed themselves on this issue more than once.

India was all in favor of international control to the extent necessary to ensure peaceful uses of atomic energy. But, as Jain points out, India was emphatically opposed to any restriction on her sovereign right to utilize peacefully her own atomic energy resources. The study clearly indicates that in response to the Chinese bomb and the determined bid to develop delivery vehicles, India began to look upon the nuclear

option more favorably. Developing its own bomb was then regarded as the only possible course open to India to balance the might of China, unless the Big Two were prepared to offer suitable joint guarantees. This part of the story, Jain says, belongs to his forthcoming study. One hopes that it will be equally well documented and also somewhat more readable. The laborious presentation of an endless chain of quotations from the Indian leaders or delegates, and summaries of this or that relevant UN document need to be modified. This shortcoming, however, should not detract us from the utility of the present study as a source of ready reference for future scholars of disarmament in general and of India's role in the negotiations in particular.

B. RAMESH BABU

University of Bombay

The Lin Piao Affair: Power Politics and Military Coup. Edited by Michael Y. M. Kau. (New York: International Arts and Sciences Press, 1975. Pp. 591. \$20.00, cloth; \$8.95, paper.)

The Cultural Revolution and Lin Piao's abortive military coup, the editor of this book remarks, have probably been the most significant and dramatic developments in the political system of China since the Communists seized power in 1949. I would agree with this evaluation, and add that the Lin Piao affair should be viewed against the background of the Cultural Revolution, of which it was a consequence. During that revolution Mao Tse-tung had violated the rule that the party must always command the gun, and that the gun must never be allowed to command the party. Backed by the military establishment which Lin Piao led, in his capacity of defense minister, Mao demolished a party which had become resistant to his will. At the conclusion of the Cultural Revolution, with Mao's reconstruction of the party structure underway, the time came to force the military genie back into the bottle of party control. It was that endeavor which touched off the Lin Piao affair.

If the spectacle of Chairman Mao leading an uprising against his own party had bemused outside observers, its sequel, the Lin Piao affair, seemed almost equally strange. After all, the aged Mao Tse-tung whom Lin Piao allegedly attempted to assassinate had but recently arranged Lin's elevation to the position of sole vice chairman of the party, and his designation by name, in a revised party constitution, as the Chairman's "close comrade-in-arms and successor." Casual consideration might lead one to

dismiss such affairs as too mysterious for comprehension. But in actuality it is occurrences like the Lin Piao affair — occurrences too important and too eruptive to be kept entirely out of sight — which are susceptible of at least partial and tentative reconstruction, and of providing us with useful insights into what would otherwise be the hidden workings of upper-level Chinese politics.

Michael Y. M. Kau, in The Lin Piao Affair: Power Politics and Military Coup, provides upwards of 600 pages of translated source materials relating to that affair, together with an introduction of some 58 pages presenting his own reconstruction and analysis of it. The most important of the source materials are the translations, comprising some 68 pages, of eleven official documents issued by the party center in Peking. Included among them are a coup planning paper prepared by a group of plotters led by Lin Piao's son, Lin Li-kuo; an account of explanatory talks held by Mao Tse-tung with key regional commanders, in preparation for his intended showdown with Lin Piao; and excerpts from the report of an ad-hoc investigation team, set up by the party center in the wake of Lin Piao's flight and death. While all but one of these documents had been labeled "Top Secret," and though most of them first became available through Taiwan, they have generally, and I think correctly, come to be accepted as authentic though not necessarily reliable in terms of content.

The case against Lin Piao, basically made in the official documents, was elaborated in speeches by officials and articles in the printed media. The volume under review contains about 135 pages of such materials. They are followed by a concluding section, comprising over half the translated source materials, devoted to Lin Piao's speeches, writings, and instructions from the 1965-1970 years which spanned the period of the Cultural Revolution. It is difficult to regard many of the more extreme accusations leveled against Lin Piao, recorded in the speeches of officials and the published articles, as representing anything but a campaign designed to reverse his long-held reputation of revolutionary hero and loyal supporter of Mao Tse-tung. The real Lin Piao tends to emerge, however, from the speeches, writings, and instructions which comprise the book's final section.

I know of no single reconstruction and analysis of the Lin Piao affair which is more illuminating than Mr. Kau's in his introduction; and at least two of the items contained in the section devoted to the "Top Secret" documents

- the Lin coup plan outline and the summary of Mao's talks with the regional commanders make fascinating reading. The remainder of the book, in my opinion, will be of interest chiefly to those readers who wish to make their own reconstruction and analysis of the Lin Piao affair, or who are willing to sift through Lin's speeches, writings and instructions of pre-coup years in order to obtain the occasional nuggets of interesting information which they contain. Such readers will want to make up their own minds about whether Lin Piao actually ordered his coup plan put into effect, as the editor appears to assume (pages li, lii and lxv), or whether contrary statements elsewhere attributed to Chou En-lai may be more reliable; and they may ponder the question of whether the coup plotters' map of Tiao-yü t'ai refers to the island of that name (as a footnote on page 104 suggests) or to the similarly named official guesthouse compound in Peking. But they will be grateful to find in one volume so large a selection of translated source materials and so stimulating and generally reliable a body of editorial comments.

EDWARD E. RICE

University of California, Berkeley

Soviet-United States Naval Balance. By Robert A. Kilmarx. (Washington, D.C.: Georgetown University Center for Strategic and International Studies, 1975. Pp. 181. \$5.00, paper.)

During the first twenty-five years after World War II there were few published, English-language works on the Soviet Navy and only two could be considered "significant contributions" — the collection of essays published as The Soviet Navy (New York: Praeger, 1958) and Robert L. Herrick's excellent Soviet Naval Strategy (Annapolis, Md.: U.S. Naval Institute, 1968).

In the last six years there has been a profusion of books and monographs on the Soviet Navy few of which, in my opinion, have added significantly to our knowledge or understanding of the subject. This paperbound monograph is both useful and readable.

Dr. Kilmarx astutely notes that "the game of manipulating ship numbers, age, and capabilities can be played convincingly to a predetermined outcome by any skilled practitioner of naval theory, by mathematical models or logical progressions, depending on his bias and assumptions" (p. 1). How true! The author then starts with the basics — why the United States and the Soviet Union have navies and how they seek to employ them — and he points out the

importance of navies in peacetime — as Admiral Gorshkov claims — "to vividly demonstrate the economic and military power of a country beyond its borders" (p. 4).

In seeking a useful method of analyzing naval balance, Kilmarx considers four functions for general or multipurpose forces: (1) restrictive and preclusive operations, as when Soviet ships in Egyptian ports in October 1967 inhibited Israeli attacks and Soviet demands for port visiting rights in the Mediterranean; (2) supportive operations, as when the Soviets claimed that the presence of their ships near the Libyan coast alone prevented a Western-backed counter-coup when King Indris was overthrown in 1969; (3) protective operations, primarily protecting nationals overseas and commercialshipping, a "legitimate" rationale for the use of naval power; and (4) projection operations, both planned and improvised. The last function according to Kilmarx, reflects how "a sustained naval presence in an area can assure a major power a voice in the multilateral diplomacy that will shape the political future of the states in the area" (p. 9).

Whether or not one agrees completely with this analytical scheme, it does demonstrate the difficulty in understanding and categorizing modern naval operations, and their increasing importance.

After a comprehensive introductory chapter, Kilmarx provides separate chapters on the "strategic dimension" of the naval balance; on Europe's flanks; on the Indian Ocean; on the Pacific; and on the Caribbean. The strategic discussion is especially useful in the context of the SALT and Kissinger-versus-Schlesinger debates; also, the author explains why it probably would be impossible to "trail" Soviet missile submarines effectively should the USSR increase deployment to the level of U.S. Polaris/Poseidon operations (just over 50 per cent of the force at sea). This is significant because most Western writers on the subject cite "trail" as the most effective method of ASW against strategic missile submarines.

Unfortunately, there is only a brief summary and no chapter for Kilmarx's conclusions. Perhaps this omission represents his efforts to maintain a degree of objectivity, allowing readers to form their own conclusions. But the message is, painfully for the West, obvious: virtually all trends in the naval balance now favor the USSR. This is not to say that the Soviet Navy is "ahead" in all or even a majority of measurements. Rather, by almost all criteria, whereas a decade or two ago the U.S. Navy was preeminent in essentially all categories, we have today lost in some and are losing in others.

Kilmarx can be criticized for mentioning research, merchant, and fishing fleets, as well as other nonnaval elements of "sea power," without discussing them sufficiently. He has narrowed his margins, however, by use of the term "naval" rather than "maritime" in the title. Also, the book contains a disturbing number of small errors in the description of ships and weapons considering the plethora of unclassified material available. For example, by 1975, it was known that there were only thirty-three or thirty-four Yankee-class submarines, not thirtyeight (p. 67); the Krivak destroyer was then believed to carry the SS-N-10 missile (now the SS-N-14), but never the SS-N-9 (p. 70); the Baltic Fleet has no "ocean-going capability" on the basis of observed exercises and other indicators (p. 71); the U.S. mine warfare ships based in Japan were mine countermeasure units and not a "mining force," and it was disbanded in the early 1970s (p. 137).

Again, given the large number of words being published on Soviet naval matters, this small volume is both useful and readable. In a period when American use of the sea is probably more important than ever before in the nation's 200-year history, it is also highly relevant.

NORMAN POLMAR

Editor, United States Sections Jane's Fighting Ships (1967-1976) and Ships and Aircraft of the U.S. Fleet

A Nation Writ Large? Foreign Policy Problems Before the European Community. By Max Kohnstamm and Wolfgang Hager. (New York: Macmillan, 1975. Pp. 275. \$17.00.)

When Carl J. Friedrich wrote a book under an almost identical title (Europe: An Emergent Nation? [New York: Harper & Row, 1969]), he addressed himself to the relevant question of the integrability of the communities which might form a future Federation, i.e., business, agricultural, labor, or elite communities. He did not merely limit the examination, as the book under review does to the potential foreign policy of the yet unborn and perhaps never-tobe-born European nation. Why this title? One surmises that because at the time this book was being written some European statesmen still visualized the purpose of the integration of Europe as the formation of an independent "Third Power." therefore the authors and editors saw as their first duty to reassert the iron laws of interdependence. This limited task the authors have accomplished with perspicacity.

Indeed, the danger of ephemerality inherent in all studies of policy making and especially of foreign-policy making has not affected the Kohnstamm-Hager book very much. On the other hand, in the case of the studies on "The Community and the Less Developed Countries" by Henry Perroy and "East-West Relations" by Michael Tatu, the integrationist aspects of the progress made since 1973 by the Community have proved greater, or speedier, than forecast by the authors. For instance the Lomé convention, too late (1974) to be dealt with in the first of these studies, broke much new ground. The success of the long negotiations was due to: (1) institutional imagination in setting up the intricate integrative mechanism, and (2) the practical ingenuity with which the new Treaty transformed the old relations between the individual Western Powers and individual decolonized territories into a global functional relation between the community and a group of "new states." Similarly, Michel Tatu's hopes, on the eve of the European Security Conference, that the Nine would agree on a common attitude toward the Warsaw Treaty organization were fully confirmed. Moreover, the unity of the Nine rendered the proposal to attend the conference more palatable to the United States. It also facilitated, in 1973-74, the transfer, as prescribed by the Treaty of Rome, of the sovereign right of the individual member states to sign bilateral trade agreements with individual "state trading" [Communist] countries to the Community. This was a significant "acquis communautaire."

The analyses by F. Duchêne of "The European Community and the Uncertainties of Independence," by Signor Gazzo of "The Relations between the European Community and the United States" and by Sir Bernard Burrows of "The European Security," share the same premise. The premise is that since the European Community will not become in the future either a superpower or a neutral state, it might be better visualized as an ongoing process conditioned by major relations of interdependence, above all with the United States. Moreover, in the conclusion, Professors Kohnstamm and Hager indicate that "at the present stage of European unity it is first necessary to build a working consensus on the role of the Community in the world" (pp. 254-255). Whereas the short-term collaboration between the U.S. and the European Community has greatly improved since the breakdown of 1973, the prospects of long-term collaboration still require a clear formulation of all the desiderata of an ideal-type European foreign policy, directed, as the Gaullist expression goes, à tous azimuts.

This could afterward be adjusted and reframed within the possibilities of the indispensable relation with the U.S.A. But for the time being, nobody, not even European public opinion, knows what a "perfect" foreign policy could be for the E.C.

The conclusion also raises, though only at the very end of the book, and too tentatively to be sufficiently noticed, the two most important questions of 1977, especially for political scientists looking at the future of the European Community. When they state that "a broad public debate on the issues is essential. Such a debate would also help the Commission to reflect and interpret European public opinion" (p. 255), the editors touch on the supremely important question of the breakdown of communications between European institutions and European public opinion. The information provided by the Community is kept at the lowest common denominator or, to be precise, at that particularly low level at which all member states in the Council of Ministers and all political groups in the European Parliament can agree on the texts to be released. But then such texts are so innocuous and vague that they scarcely arouse the interest of national and European public opinion. What is needed is information, especially about the new European solutions to the overwhelming socioeconomic problems of Europe, as distinct from those experimented with by the national governments.

For a European public opinion concerned above all with massive socioeconomic problems, the apparent concentration both of the Community and of its information policy on mainly intergovernmental issues seems to be not only irrelevant but also futile. The conclusion rightly asserts that "the Community's capacity to act constructively in the world depends on the last resort on its success in establishing within its boundaries a more united and more just society" (p. 256). But insofar as this is said in, literally, the last line of the book, one can only conclude that what is needed is yet another book starting where this one has prematurely left off.

GHITA IONESCU

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The Forgotten Friendship: Israel and the Soviet Bloc 1947—1953. By Arnold Krammer. (Urbana: University of Illinois Press, 1974. Pp. 224. \$10.00.)

Arnold Krammer's book The Forgotten

Friendship sets out to review the fascinating story of Soviet and East European relations with the state of Israel during the first years of its existence. In May 1947, the Soviet Union astonished the world by supporting the Zionist aspirations for the establishment and consolidation of a Jewish state in Palestine. Equally astonishing was its ideological about-face regarding the Zionist movement, until then a major focal point of Soviet distrust and criticism. For two years a unique entente existed between the Soviet Bloc and the infant state of Israel, with the former constituting the sole source of vital armaments and military training for the embattled Jews. In late 1949, a reverse trend became apparent, with relations deteriorating into what the author calls "a period of indifferent neutrality." Relations reached their lowest point during the East European purge trials, particularly during the 1952 Slansky trial in Czechoslovakia, which took on explicit anti-Semitic and anti-Zionist overtones. On February 12, 1953, after an exchange of mutual recriminations culminating in an explosion of a home-made bomb in the courtyard of the Soviet legation in Tel Aviv, the Soviet Union severed its diplomatic ties with Israel. Relations were resumed only on July 20, 1953, as part of a reappraisal of Soviet policy following Stalin's death.

The period examined in Professor Krammer's book is undoubtedly as important as it is interesting. For its exploration might well have provided meaningful insights into the goals and mechanics of Soviet foreign policy. Moreover, this period, devoid of much serious, comprehensive inquiry to date, is wide open to scholarly research. The need for a thorough investigation of the subject arises principally because a great many of the materials published so far are memoirs or personal accounts written by those (mainly Israelis) who actually participated in the events — recollections which, for all their appeal, tend to be subjective and discursive.

Professor Krammer obviously put considerable effort into compiling factual data. His survey encompasses a wide array of literature including Western, Soviet, East European, and Israeli press and public pronouncements. These, conveyed by the author in a flowing and lucid style, are complemented by three research trips undertaken by Professor Krammer to the Middle East and Eastern Europe in order to examine official and private archives, as well as to interview the surviving participants in the events set forth in the book.

It is perhaps ironic that the wealth of facts and information that stands out as the book's most impressive achievement should also become its major pitfall. Thus, the perceived need to incorporate the vast number of factual details into the discussion overshadows the clear definition and rigorous development of arguments and analysis. Too often, the dramatic and fascinating narrative (particularly so to readers unfamiliar with other accounts of the period) lapses into irrelevant digressions.

The book lacks a central thesis or analytical framework which might have produced a wellstructured, comprehensive study that transcended a purely historical survey of events. The author presents the various determinants of Soviet foreign-policy behavior as independent, polarized factors (ideology versus pragmatism; domestic versus foreign-policy considerations; economic versus political factors, and so on). There is no attempt to analyze the interplay among these factors or to determine the relative impact of each factor as it affects the course of foreign policy within the case study. Hence, many conceptual contradictions arise. Moreover, some of the most significant ramifications of the events surveyed are left unexplored. Instead, Mr. Krammer prefers to repeat the standard observations about Soviet political behavior, observations which, for all their generality, are not always implicit in or relevant to the actual findings of his research.

In sum, the book presents us with an extremely rich ore, which, however, remains to be refined.

ILANA KASS

The Hebrew University, Israel

Japanese Radicals Revisited: Student Protest in Postwar Japan. By Ellis S. Krauss. (Berkeley: University of California Press, 1974. Pp. 210. \$10.00.)

Several thousand Japanese students were directly involved in various activities related to the great Ampo struggle of 1960 which attracted international attention and was generally regarded as a severe domestic crisis in Japan. As Professor Krauss notes:

Four years before the Free Speech Movement at Berkeley portended the waves of student protest in the United States, Japanese students had proved the potential power of students and student movements in opposition politics. In Japan itself, the experience of the 1960 treaty crisis was so salient as to create a generation of students who, to this day, are referred to as the 'Ampo generation' and whose leaders' names became household words in Japan (p. 5).

Krauss asks the obvious question: "What has become of these student activists of 1960?" In order to answer this and other related questions, Krauss personally interviewed fifty-three former students of the Ampo generation. In his analysis and interpretation of those interviews, he develops a number of interesting ideas and insights. Unfortunately, he also tends to drift into areas which strike me as being off-target.

More specifically, I find much of the discussion dealing with "political socialization" dreary, repetitious, and unnecessary. While many of the issues raised may be of interest to those who see the study of political socialization as a subfield of political science, I feel that it does not contribute positively to the study's goal of understanding what has become of student activists of the 1960s and the significance of student discontent and protest in the 1970s and beyond. In contemporary Japan. discontent and protest are widespread and the legitimacy of the "Regime of 1955" is being seriously challenged. Workers, farmers, intellectuals, doctors, urban dwellers, housewives, and various other social groups have contributed to the general turbulence of Japanese political life. Placing student discontent and protest in historical context is an important scholarly task, and political socialization can be a useful concept for understanding one aspect of that context. In choosing, however, to define political socialization as "the process by which political beliefs and behavior are acquired, maintained, and changed throughout the Efe cycle" (p. 16), Krauss makes it a conceptual catch-all which deflects attention from more to less relevant issues. Much of the book seems out-of-focus as a result.

The Japanese version of the ideological *urnabout myth, shūshoku tenkō, or "employment conversion," has been perpetuated almost without challenge. As indicated by Krauss: "Conventional folk wisdom that student radicalism is merely a passing phase of youth, and the publicity given the dramatic tenko of some former Zengakuren leaders, have together given rise to a widespread belief in Japan about the inevitable fate of the Ampo student activists" (p. 7), - i.e., that student radicals tend to enter large business concerns or government service and shed their radical ideas shortly thereafter. Krauss does challenge this myth persuasively but not conclusively. The problem is that he simply does not have the evidence (not that the evidence in support of the myth is any better).

As much as he attempts to ignore the fact, Krauss cannot get around the limitations of "the data" on which he chooses to base the major part of his analysis. One cannot help but

admire the effort involved in tracking down and talking with fifty-three members of the Ampo generation. The problem is that fifty-three "anonymous men" (no women were included) cannot be taken as representative of a "generation" unless they are selected according to some generally accepted method. The method used by Krauss is arbitrary, and his sample cannot be taken as representative of anything but itself.

There are other troublesome methodological limitations. Some are inherent in survey research methods but others are a consequence of the unreflective and injudicious use of such methods. For example, in his analysis of the family role in the political socialization of his respondents, Krauss does not question the reliability of their recollections of family life. In other places, Krauss indicates that he has "tested" certain hypotheses. I am not persuaded, however, that his procedures constitute valid "tests."

I find most troubling Krauss's treatment of the Marxist aspects of political consciousness in Japan in general and among his respondents in particular. His juxtaposition of "Marxism" and "humanism," and his effort to reduce the former to four premises and to classify his respondents as "total Marxists," "eclectic Marxists," and "non-Marxists" in terms of their response to interview questions supposedly based on those premises is superficial The "revisit" of the Ampo generation students by Ellis S. Krauss is a fascinating study but seriously distorted by the simplistic treatment of Japanese Marxism.

WILLIAM E. STESLICKE

University of Michigan

A Nation Observed: Perspectives on America's World Role. Edited by Donald R. Lesh. (Washington, D.C.: Potomac Associates, Inc., 1974. Pp. 150. \$8.95, cloth; \$3.95, paper.)

It is important to recognize immediately the value of this relatively small publication, since the introduction not only does not promise the study to be academic but itself fails to capture the real depth evident in the selections following. The text proves to be a seasoned overview and taken as a unit becomes increasingly significant. The editor states an assumption that readers will have an understanding of U.S. foreign relations which itself recognizes and verifies the level of analysis evident.

The text incorporates six essays, offering the perspective of six experienced foreign observers

of the United States, each representing a different view. The majority have had academic affiliations; all have been associated professionally as analysts of U.S. foreign policy. The publishers also commissioned a national opinion poll to test the attitudes of Americans on the strength and importance of the U.S., particularly as evident in sentiment regarding internationalism and isolationism. These findings are recorded in the final section.

A collection of this type has a potential to date itself. This would be underscored had Secretary Kissinger not remained as a central figure following Nixon's resignation. Reference to the Nixon-Kissinger Doctrine remains valid, vielding clarification regarding the dominant role of the Secretary of State. Donald Lesh, the editor, refers to him as the sixth superpower. John Holmes, director general of the Canadian Institute of International Affairs, in a concise analysis, concludes that the Nixon-Kissinger Doctrine is a design for a new era rather than for a single administration. The doctrine is credited with recognizing America's inescapable responsibility for leadership, combined with a plea for a more equitable sharing of burdens, policy making, and responsibilities.

Central to the selections is a recognition of a change in the international system that reflects a limit to U.S. power. The U.S. maintains a unique position, but its growing dependence on foreign resources has altered its bargaining position. While dependent upon the maintenance of a military balance, issues relating to security have made room for those focused on economics. The new international system reflects the U.S. as seeking a managing role, aware of a vulnerability evident in the constitutional crisis during Watergate and the balance-of-payments difficulties which lead to the collapse of the monetary system created at Bretton Woods.

Pierre Hassner, senior research associate at the Centre d'Etude des Relations Internationales in Paris, refers to a Europeanization of American policy in which the U.S. has moved from occupying a superior position in a multilateral structure to a vulnerable, yet sustained, position of initiative, conditioned by a Bismarckian alertness with Dr. Kissinger managing a revised capacity for U.S. leadership.

The dichotomy evident in the selections, confirmed in the opinion poll commissioned for this publication, is that "Middle America," while desiring to reassert a dominant position for the U.S., has moved toward an isolationist view. The U.S. public is increasingly preoccupied with domestic problems. Hans Herbert Götz, Brussels correspondent for Frankfurter Allgemeine, notes that the subject matter in

foreign and domestic politics has itself undergone change, suggesting the evolution of a world domestic policy. John Holmes asserts further that most of the substance of current international negotiation is now concerned with economic and social issues, within which allies shift positions.

It is evident in these readings that the U.S. is confronted with a redefinition of its role in world affairs. The combination of perspectives presented yields an awareness essential to the contemporary analyst. The text offers more than an observation; it provides an assessment. Beginning with a careful reminder by Georgy A. Arbatov, director of the Institute for the Study of the U.S.A. in Moscow, that Americans are less often considering military might as a basis for foreign policy, the reader is reminded that our transition from empire to nation now involves more commitment to economic considerations. Yukio Matsuyama, political reporter for Japan's Asahi Shimbun, concludes that U.S. foreign policy has been dominated by too many lawyers, each staking out a ground of legitimacy giving emphasis to an outdated status quo. The contributors perceive instead a lack of a single appropriate description for the structure of contemporary affairs. Mariano C. Grondona, Argentine lecturer and political commentator, recognizes a new multipolar world in which the U.S. maintains a role as rector. He concludes that what counts now is the effective negotiating power of each country. Within this environment the U.S. is left with a capacity and challenge for leadership, while domestic support remains potentially thin. A text which verifies this is a must in our field. This presentation is an alert overview for foreign policy analysts engaged in assessing U.S. priorities and the significant impact of Secretary Kissinger.

HAROLD S. JOHNSON

Michigan State University

The Soviet Impact on World Politics. Edited by Kurt London. (New York: Hawthorn Books, Inc., 1974. Pp. 312. \$13.95.)

This collective work purports to offer an analysis of Soviet policies in all areas of the world, except for Africa. Its several authors differ from each other regarding the basic Soviet motivation: is the USSR engaged in the pursuit of its national interests or is it primarily inspired by the eschatological image of a universal victory of Soviet-like socialism? The editor has no doubts on this score (Preface and Conclusions, pp. vii—viii and 271—300). The

USSR remains for him a revolutionary power committed to the goal of a world revolution; hence, no real detente is possible with it. Brandt's Eastern policy stands condemned in his eyes, because it allegedly opened the door to the Soviet penetration into Western Europe.

Professor Leonard Schapiro of the London School of Economics fully subscribes to the editor's views (pp. 3-21). He advances a debatable proposition that the Secretary General of the C.P.S.U., be he Khruschev Brezhnev, has always enjoyed the monopolistic power to formulate foreign policy, thus discounting any important role of the Political Bureau in this respect (p. 14). He says rather peremptorily that: "The speculations over dissent on foreign policy inside the Politbureau ... are extravagant" (p. 4). The Soviet Union, committed to "the world of Communist power," pursues the policy of an "aggressive, chauvinistic, expansionist world power" (pp. 6-7). Schapiro advances another debatable proposition that the Party Secretary General may "ignore the advice of his military leaders" (p. 13). He believes that one may extract political concessions from the USSR and China in exchange for trade facilities and credits (p. 14). The failure of Senator Jackson to extract such concessions (free Jewish emigration) does not support this contention. Schapiro too lightly dismisses the studies by Soviet academic specialists on international problems as "nonsense" (p. 15), although he concedes an improvement during the past decade (p. 18).

Professor Adam B. Ulam provides a summary of the history of Soviet foreign policy (pp. 22-45), a sort of resume of his excellent book on this subject. He is inclined to share the editor's outlook, also believing that the Soviet policy of co-existence with the West is nothing more than the old posture of "neither war nor peace" (p. 44).

The subsequent chapters, devoted to regional studies, are more matter-of-fact and less prone to indulge in sweeping generalizations. Professor Hugh Seton-Watson of the University of London writes about Eastern Europe, an area difficult to generalize about because of the different historical and cultural backgrounds of its component countries and also because of notable differences among its Communist regimes. He notes the postwar achievements such as economic development, urbanization and mass education (with the resulting social mobility), which transformed "the peoples of Eastern Europe ... [into] modern European nations" (p. 69). He points out the examples of survival of old national discords such as the Romanian-Hungarian over Transylvania, the

Yugoslav-Bulgarian over Macedonia, and the Yugoslav-Albanian hostility. The advent of Communist regimes has not altered the East European problems in this respect, although the alteration of frontiers and mass population shifts made of Poland a homogeneous state for the first time in its history.

Professor Alfred Grosser of the Institut d'Études Politiques competently writes about the French-Soviet and West-German-Soviet relations, although he makes one mistake in stating that West Berlin "is internationally represented by Bonn" (p. 82), this being true only regarding nonpolitical matters. Unlike the editor, he evaluates favorably Brandt's Eastern policy as contributing to the general detente as well as to the greater independence of the Federal Republic which needs no longer any intermediaries in its relations with the Soviet Union and Eastern Europe (p. 89).

Mr. Malcolm MacKintosh of the London International Institute for Strategic Studies offers a historical summary of Soviet policy toward the Middle East (pp. 94–112), stressing the two assets of that policy: the Arab-Israeli hostility, and the American commitment to Israel. He rightly points out the importance of the Suez Canal for the Soviet Navy and Merchant Marine. He is far from wrong in stating that "perhaps the least favorable situation for the Soviet Union would be a peace settlement between Israel and at least one of its neighbors [Egypt?], for this would reduce the indispensability of the Soviet Union for the Arab states" (p. 106).

Professor Harold C. Hinton of George Washington University devotes his attention to the Soviet policy in East Asia, pointing out the unfavorable factors such as the quarrel with Japan over the Kurile islands fishing rights, and the dispute with China. He notes the Soviet success in winning the Indian friendship due to the hostility of both countries to China and the Soviet support of India against Pakistan. He might be right in predicting "a trend toward accommodation [between China and the USSR following Mao's passing] since it is desirable on both sides on a number of practical grounds" (p. 137). One of those grounds of accommodation is the elbow room which China would gain in maneuvering among the three other Far-Eastern powers, the U.S., the USSR, and Japan.

Dr. Guy J. Parker of the RAND Corporation offers his views on Soviet policy in Southeast Asia (pp. 138-155). By the way, he rejects the editor's view that the Soviet main objective is the spread of communism as being "captive of an outlook shaped in the days of the Comintern" (p. 150). He is probably right in thinking

that the Soviet ultimate objective in Indochina is Hanoi's supremacy as a counterweight to the Chinese influence (p. 151).

Mr. William J. Bernds of the Council on Foreign Relations concludes his study on South Asia by stating that the Soviet Union achieved in 1971 the status of the ranking power in that part of the world by its support of India against China and Pakistan (p. 169).

Dr. Leon Goure of the University of Miami analyzes Soviet policy toward Latin America (pp. 182-207), evaluating it as "pragmatic and unencumbered by heavy ideological baggage" (p. 193). Yet he thinks that the USSR has not so far achieved any spectacular successes in spite of the rise in Latin-American nationalism with its anti-U.S. sentiment and in spite of the Latin-American inclination to develop economic relations with the Soviet bloc.

Dr. Charles Burton Marshall of The Johns Hopkins University supplies a not very original historical survey of the American-Soviet relations (pp. 211–233). He too disagrees with the editor, saying that: "No one I ever knew or heard of in the U.S. policy nexus would [assume] ... that the Soviet ruling group would be inclined to sacrifice national interests in deference to revolutionary purposes" (p. 219). He quotes approvingly Jacob Beam, former Ambassador to Moscow, who said that the Soviet leaders "are not revolutionaries any more. They are hard-headed practical people and know that the total Communist victory is not going to happen ever" (p. 231).

The chapter on Soviet military forces by Thomas W. Wolfe (pp. 237—268) supplies very valuable data not only on those forces but also on the Soviet military aid to non-Communist countries such as the Arab states or India.

All in all, the authors of this book have provided chapters of unequal value, the best being those which limit themselves to matter-of-fact expositions and are free of cold-war ideology.

W. W. KULSKI

Duke University

The Angels' Game: A Handbook of Modern Diplomacy. By William Macomber. (New York: Stein and Day, 1975. Pp. 225. \$10.00.)

William Macomber, recently U.S. Ambassador to Turkey and formerly holder of top State Department executive posts, including that of Deputy Under Secretary for Administration, states the case in this book for the development of a new breed of Foreign Service Officer: the "manager-diplomat." As a cham-

pion of the "Young Turks" (the Washington, D.C., variety of the late 1960s) and the impresario of a notable State Department effort at "reform from within" (the "Macomber Task Force" reports published in Diplomacy for the 70's: A Program of Management Reform for the Department of State [1970]), he writes from unusual authority and experience.

The reader who expects to find in this book the details of this attempted "managerial evolution" will be disappointed. Neither diplomatic memoir nor technical treatise, The Angels' Game (whose title is derived from Harold Nicolson's observation that the first diplomats were angels - angeloi, diplomatic "shuttlers" between heaven and earth) might easily be dismissed as another diplomat's commonplace book, full of aphoristic rules-of-thumb and well-meaning exhortations. The diplomatic practitioner is, for example, told: "arrange things in such a way that the disputes which arise between nations are losable" (p. 70); "always seek to avoid disputes over principle" (p. 70); "no nation should be regarded, or treated, as a permanent enemy" (p. 72); "be certain that policy makers never lead the country into believing that major problems will fade after one last effort, one last battle" (p. 74). The scientific value of such counsel which, incidentally, shows how easily traditional diplomacy lends itself to a policy of pragmatic détente - is not established by careful specification of the conditions under which it is applicable. The author, sensibly but uninstructively, states: "There are no principles in diplomacy, no matter how sound, that cannot be violated if circumstances warrant" (p. 74).

What makes this book worth reading are the description of the altered institutional environment in which the professional diplomat finds himself and the prescription of methods for him to survive in it. The professional outlook of most diplomats, he argues, has not evolved with the demands of changing circumstances. They "tend to have a far better understanding of what their job used to be than what it is today" (p. 7). Accustomed to "hunch playing" and "lone-wolf activities," which rely heavily on personal "feel" and experience, they become uncomfortable under the requirements of "systematically coordinating large-scale operations" and "managing and being managed by others" (p. 80).

For this they are not entirely to blame. Unlike officers elsewhere in government service, particularly in the military, they receive little training in management early in their careers. As a consequence, they find themselves in

important managerial positions – ambassadorships and other high-level embassy jobs – "with almost no previous managerial experience to fall back on" (p. 82).

Paradoxically, Macomber points out, the "management lag" tends to be greatest during "the eras of strong secretaries of state" (p. 88) - one of the few implied ad hominem criticisms in the book. The reasoning here is interesting: It is not simply that Secretaries Acheson and Dulles (and presumably Kissinger) chose to rely on their own virtuosity and neglected to develop and utilize the skills of others in the Department. It is that the Department's lesser personnel, basking in the "reflected authority" of the high-flying immortals above them, have let the Seventh Floor's dominance of the Washington foreign policymaking scene substitute for genuine institutional superiority. Their feeling of confidence is thus illusory. Macomber warns his colleagues: "leadership, and the effective and sensible coordination of the whole range of a nation's foreign affairs, clearly require more than a high morale and a combative stance in intramural disputes" (p. 89).

How can the State Department, with its relatively small number of employees, small budget, and small domestic constituency, hope to provide leadership and coordination for all the departments and agencies now involved in foreign affairs? The answers Macomber gives in The Angels' Game are sound. The "key to leadership," he affirms, is "having the ideas" (p. 75). The flow of these should be increased by a vigorous "adversary/devil's advocate system" and by greater openness toward "outside ideas" from the academic and other communities. Coordination should be improved by operation of the Seventh Floor as a "collegium" (p. 100) and by expansion of the State Department's liaisons with other departments and with Congress.

If only those the angels were playing with were angels too....

ALAN K. HENRIKSON

Fletcher School of Law and Diplomacy

Kenneth Kaunda of Zambia: The Times and The Man. By Fergus MacPherson. (New York: Oxford University Press, 1975. Pp. 478. \$10.75.)

The lives of only a few leaders are complete-

ly intertwined in the evolution of their country from foreign domination to independence and in the early years of the new state's independence. Their lives record a significant part of the history of a country and a people. Such is the life of Kenneth David Kaunda, President of the Republic of Zambia. Fergus MacPherson, in his Kenneth Kaunda of Zambia, has recorded in one volume the colonial background of the period of Zambia which was the era when the Zambian president was born. He mixes this with facts about President Kaunda's parents and early family life not known to the general public. The book is at its best in recording in some detail the early political life of President Kaunda up to independence. The authors has captured the fast-moving trend that took Kaunda from the African National Congress to the State House in 1964. Professor MacPherson has also managed to catch the special sparkle of Kenneth Kaunda. The readers obtain a sense of Kaunda's personality from this book. While the focus of the book is naturally Kenneth Kaunda, I regret that the author was not able to give more attention to his wife, Betty. Some of us who have closely followed Kaunda's career believe that his wife has had more than routine influence on his political values.

This book is well documented and is valuable for students and others interested in pursuing specific matters in greater detail. It would have been more comprehensive if the author had been able to include a chapter on the relationships of this African leader with leaders in other parts of the world. Kaunda not only corresponds with leaders in all parts of the world but influences them.

In my opinion, history has already guaranteed a place in the hierarchy for Kaunda: in his short lifetime, he has been the founding father of a nonracial state in southern Africa! Now he faces even greater challenges as he uses his considerable skills to find just solutions for the problems of Rhodesia and South Africa. The author was not able to include these subjects in his book because Kaunda's role was just beginning to evolve when he wrote this book. The events in southern Africa in 1974, 1975 and 1976 give every indication that Kaunda may be able to play a historic role in resolving some of the tensions that have plagued this part of the world too long.

MacPherson's work is a first-rate book on a man of considerable nobility. It should be required reading not only for students of African affairs but for all interested in one of the most strategically located countries in Africa. Although several biographies of Kaunda have been published by now, MacPherson's is the first major comprehensive work on Kenneth David Kaunda of Zambia.

THOMAS PATRICK MELADY

Sacred Heart University, Bridgeport, Ct.

The Middle East in Soviet Policy. By R. D. McLaurin. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 206. \$13.50.)

This work may best be described as a series of fact sheets about Soviet aims and activities in the Middle East since the mid-1950s, apparently intended, since the research was sponsored by the Defense Department, to introduce nonspecialist government officials to the subject. As such, it is not likely to prove of much interest to specialists on the Soviet Union or the Middle East, because the material presented presupposes little more than minimal knowledge of the area, the key actors, and developments since 1945.

The approach is macrostrategic at its least impressive: the Middle East is viewed from a perspective so distant that differentiation, complexity, and the dilemmas faced by the Soviets are barely, if at all, discernible.

There are eight chapters: these introduce the general subject, provide some historical background, examine domestic constraints on Soviet policy, describe the political, economic, military, and cultural dimensions of Soviet activities in the Middle East, and conclude with some observations. Most of the material is rudimentary. For example, the chapter on Soviet domestic constraints on policy toward the Middle East contains short sections on the nationalities, the Party, industrial and defense industry groups, the military establishment, and Middle East government bureaucracy - all covered in four pages under the subheading, "Interest Groups"; also mentioned in the same sketchy, uninformative fashion are "bureaucratic politics," "public opinion," ideology, and regional and international constraints. The chapter on "Military Activities" provides the most solid factual fare, but its pre-1972 vintage makes it a bit stale. It relies on the data compiled by SIPRI (Stockholm International Peace Research Institute) and offers some interesting tables.

The conclusions are generally sound, though they seldom derive from the data that preceded them. Thus, the author notes that "Soviet influence in the Middle East is amorphous: it may be employed with great effect relative to international issues to the extent they are not regionally important, but in questions of signifi-

cance to the Middle East all evidence indicates that local governments continue to make decisions on the basis of perceived self-interest" (p. 147); that the Soviet Union seeks to reduce Western influence; and that it has achieved most of its post-1945 security objectives: "The CENTO alliance is no longer a threat; Iran and Turkey enjoy relatively good relations with their northern neighbor; support for U.S. global initiatives from Middle East regimes is scarce; fewer and fewer ports welcome vessels of the Sixth Fleet; and other strategic forces considered hostile by the Soviet Union have diminished in size."

In light of Soviet policy during and after the October War, however, it is not accurate to say that "the USSR has not shown itself willing to court a risk in pursuit of its Middle East objectives" or that "in the supply of weapons, qualitative limitation has been a consistent element" (p. 148). Moscow is in the Middle East for the long haul and it is not averse to raising the stakes.

ALVIN Z. RUBINSTEIN

University of Pennsylvania

Malawi: Foreign Policy and Development. By Carolyn McMaster. (New York: St. Martin's Press, 1974. Pp. 246. \$15.95.)

In a theoretical article still relevant for understanding the foreign policies of new states Robert C. Good argued that state-building is the principal determinant of their external relations. Good said:

Foreign policy perpetuates the cohesive role of the revolution against colonialism; underscores the existence and integrity of the postcolonial state detached from the identity of its former metropole; enhances the prestige of the national leader at home while reducing the effectiveness of his opposition; and provides opportunities for diversifying the new state's reliance on external assistance.... At every point, then, the requirements of state-building impinge upon foreign affairs. For a new state, foreign policy is domestic policy pursued by other means; it is domestic policy carried beyond the boundaries of the state. (State-Building as a Determinant of Foreign Policy in the New States, in L. W. Martin (ed.), Neutralism and Non-alignment, N.Y.: Praeger, 1962, pp. 11-12.)

While Miss McMaster appears to be unaware of Good's penetrating insight (he is not cited in either the notes or references) her case study of Malawi's foreign policy represents an interesting, thorough, and particularly well-written "test" of Good's thesis.

Embedded by geography, colonial history, and economic relations within the white dominated southern African region, Malawi is of particular interest because it "was the first black African country heavily dependent upon southern Africa to become independent" (p. 53) and because of the exceptional character and personality of its Life President, Ngwazi ("Chief of Chiefs") Dr. H. Kamuzu Banda.

Miss McMaster's approach is first to describe the political evolution of Nyasaland up to its achievement of independence in 1964 in her first two chapters and then to present five topical chapters and a concluding assessment of Malawi's foreign policies from 1964 to 1973. Topics covered include: the 1964 cabinet crisis that challenged Dr. Banda's leadership; patterns of development assistance generated by Banda's policies; relations with South Africa, Rhodesia, and Portugal; and policies toward Black Africa. Appendices are provided on constitutional changes and economic relations. Detailed and often enlightening footnotes run to more than forty pages, and a useful bibliography and index complete this brief volume.

Malawi: Foreign Policy and Development is a model narrative and descriptive case study of one small, dependent state's foreign policies. Using interviews, newspapers, government publications, and secondary sources Miss McMaster tells her story well. She is particularly fair and sympathetic to Dr. Banda, a very controversial figure. Thus, although McMaster correctly regards Dr. Banda as "a despot" (p. 169), Banda's state-building goals (development and stability) and the foreign policies that flow from them are objectively described, leaving the reader with some degree of sympathy for the dilemmas facing any leader of government in Malawi, even Dr. Banda.

Of considerable theoretical interest is the question whether environmental factors or Dr. Banda's values and perceptions are the prime cause of his policies of increased cooperation with South Africa and its white-dominated allies. In an effective comparison with Zambia and Botswana, which share Malawi's southern African environment but differ from Malawi in external behavior, McMaster convincingly argues that environmental factors must make any leader of Malawi coexist with the neighboring racist regimes but that Dr. Banda's values and perceptions are responsible for the increased linkages with the white South that have been created since 1964.

Thus, this book clearly supports Good's hypothesis that state-building is the major determinant of new state foreign policy and the "pre-theoretical" prediction of James N.

Rosenau that in small, poor, and authoritarian states individual and systemic (environmental) variables are most important in accounting for foreign policy behavior (*The Scientific Study of Foreign Policy*. New York: Free Press, 1971. p. 113).

One criticism must be directed at the publishers. St. Martin's Press: at \$15.95 for a cloth version this book will not reach the audience its quality merits. A cheaper paperback edition is very much warranted.

PATRICK J. McGowan

University of Southern California

Changing Perspectives in East-West Commerce. Edited by Carl H. McMillan. (Lexington, Mass.: D.C. Heath and Co., 1974. Pp. 207. \$14.00.)

An overview of the factors which underlie East-West commercial exchanges, this book provides scholarly explanations of what happened in recent years, why it happened, and what these events meant for the policies of different countries. Edited by Professor Carl H. McMillan of Carleton University, the book includes thoughtful analyses by eleven eminently qualified contributors - eight of whom teach at Canadian and British universities, and three who are researchers in the United States. They focus mainly on changing policy perspectives of the principal trading entities; changing legal, commercial, and financial perspectives; and the impact on Western institutions. The analyses are clear, convincing, and stimulating.

In recent years, East-West commerce benefitted from the quantum jump in negotiating flexibility and in the kind of cooperation not practiced before. The two-way volume of East-West trade increased to a 1974 total of \$43.5 billion, or about six times the 1963 level. While the overall U.S. share was relatively small, the growth of trade turnover between other West-ern countries and the socialist countries during the last decade was spectacular. Emphasizing the import of capital along with the import of commodities, the Eastern countries borrowed heavily in Western capital markets, using the money to help finance sizeable, widening deficits in trade with the West.

The ten chapters of the compendium assess the various Western perspectives on an orderly and fruitful increase in commercial relations with the socialist Eastern countries. They document the substantive change both at the "macro" level of governmental economic policy and at the "micro" level of participating firms and financial institutions.

The contributors show that the expansion in

East-West commerce has been more than a steady flow of export and import transactions between Western firms and the foreign trade organizations of the Eastern countries. Western governments have entered into an intricate network of bilateral agreements with Eastern governments and have created an elaborate structure of implementing machinery. Extending over virtually the entire spectrum of Western technology, more than 1,000 agreements were concluded between Western firms and socialist countries, binding them into longer term relationships known as "industrial cooperation arrangements," and leading to mutual economic penetration on a considerable scale. With business perspectives guiding the Western nations' policies, the large multinational enterprises became deeply involved in the trade transactions and in the cooperative undertakings with Eastern countries. The involvement in East-West commercial exchanges became part of the standard operating procedures for a steadily growing number of firms and organizations, both in the East and in the West. The impact of these measures on Western institutions is the subject of the last two chapters.

While it reflects the contributors' command of the controversial issues involved, the volume pays little attention to East-West trade in agricultural products — despite the fact that agricultural commodities dominate the United States' and Canada's commerce with Eastern countries, and inflamed passions fuel the vigorous public controversy surrounding the agricultural export issue in the United States. Large segments of American farmers perceive agricultural exports to Eastern countries as a significant factor in world commerce, and get upset when trading opportunities are tampered with. Perhaps agricultural trade issues are so complex that they require special treatment.

Anyone sincerely concerned about the changing perspectives in East-West commercial relations should welcome this valuable overview. Moreover, the erudite explanations are of importance to members of government and business communities, as well as to scholars.

JOSEPH HAJDA

Kansas State University

Labour in Irish Politics, 1890-1930. By Arthur Mitchell. (New York: Barnes and Noble Books, 1974. Pp. 317. \$12.50.)

This study traces the rise and fortunes of the labor movement during a time of turbulence and in a location where constitutional issues have tended always to throw a long shadow over political beliefs and distort any rational pattern of voting among the working-class people. Dr. Mitchell offers an account which is always scholarly yet eminently readable. There are clear signs that the work was originally a thesis, but it has been updated and the many documentary sources cited are supported by reference to twenty interviews with prominent Irish personalities undertaken over a period of eight years. The study is lucidly presented, and the reader obtains a vivid impression of the role of Larkin, Connelly, Griffith, Thomas Johnson, and other key figures in the Irish labor movement.

One can, nevertheless, take issue with the author on certain points. The dates chosen seem, in a sense, almost arbitrary, in that many of the events leading up to the struggle described took place before 1890 - and indeed continued after 1930. As one who has lived for the past seven years in Northern Ireland, I consider that the North and its contribution has, in places, been somewhat scantily treated, especially because Belfast had become such a great industrial center at the end of the nineteenth century, as the author recognizes on p. 285. The conclusion, while adequate, is perhaps somewhat too concise and generally a little disappointing in view of the wealth of facts amassed and presented in Chapters 1-11. The alphabetization of names beginning with 'Mc' is unusual, both in the bibliography and the index.

The total study undoubtedly makes a contribution to the investigation of this crucial period in Irish history. Despite the date of publication, there is no direct reference to the fact that the Labour Party is now in power in the Republic of Ireland, albeit in a coalition with Fine Gael. If, when Dr. Mitchell refers to "the untried alternative of socialism," he means more radical labor policies and success than this or a party holding a majority in its own right, then he is likely to remain disappointed, for Ireland — despite some posters which greet one in Dublin — is basically (and in many senses of the word) a conservative state.

Notwithstanding these grumbles, this study is clearly the product of thorough research and will certainly be of value to lecturers and students in the field of twentieth-century Irish political history.

A. MALTBY

Queen's University, Belfast, Ireland

Democratic Campaigning in Venezuela: Caldera's Victory. By David J. Myers. (Caracas, Venezuela: Fundación La Salle de Ciencias Naturales, 1973. Pp. 259. 20 bolivares.)

It is easy to be superficially critical of this careful work, for its title misleads. It contains two different volumes, each short and experimental, with little linkage between them. The title describes only the first; many readers possibly will feel the second, which is simply titled "Epilogue," is the stronger. Myers has subordinated an impressive anecdotal knowledge of Venezuelan political history and current issues, either in the first volume's application of a strategic game framework that adapts work by Richard Snyder, or in an analysis of six cleavages in the second based on work suggested by Seymour Lipset and Stein Rokkan. As a whole, the book is not designed for a beginner; it contains little systematic instruction about the problems that have moved the country since the present system was established in 1958. Throughout, the impression is that Myers reined himself in. I wish he had not been so restrained, for eventually the restraint harms the work.

The first volume sets its material into an overarching frame, applying to the 1968 quinquennial general election a comparative analytical pattern that Myers feels has been given only limited use in the literature of comparative politics. Much of the construct is his own. He argues that the most important issues to be discussed are the institutional setting, the rules of the game, the nature of the participating units, strategies and tactics, stakes, and information. He examines each category in the light of its own subsets, each in its own chapter complete with illustrative material. A concluding chapter incorporates summaries of the votes cast and recapitulates the application of the matrix to the available information. In concluding, Myers suggests leads for research in a much too brief offering of implications for the future.

The second volume is subtitled "Structural Cleavages, Parties and Party Systems in Latin America: The Case of Venezuela." Myers again develops his own structure of operational theory, building his strongly quantitative analysis around six structural cleavages: center-periphery, clerical-anticlerical, traditional-modern, urban-rural, owner-worker, and poor-others. Each cleavage is described in general terms, and with some specific references to Latin America, and is then applied to the Venezuelan case. The census of 1961 supplies socioeconomic data by municipio, the smallest political subdivision; each of these 642 units is relatively homogeneous, although in the less-settled ones there

may be some dichotomizing of attitudes. Setting up the materials for multiple regression analysis, Myers draws on the census data for independent variables, while the voting statistics become dependent variables. Separate regressions are run for each of the more important parties in the 1958 and 1963 elections. The analysis tests the validity of hypotheses that initially governed the drawing of proxies from the socioeconomic data. An extension of simpler calculations allows judgments to be made about the other smaller parties, and about the coalitions that were organized in these two elections.

The second volume is concluded by extended comments on the 1968 election (pp. 227-240). In some respects this is the most useful portion of the volume, for it applies the hypotheses to the 1968 voting data and offers some predictive suggestions for the 1973 election. But the conclusion also is frustrating on several counts. My earlier criticism, that the two sections are not knit together, here gains force because Myers pays so little attention to the issues that give shape to the structural data.

The 1973 election demonstrated that some of Myers' less likely possibilities were actually more likely. He had suggested that the mostvoted parties (Acción Democrática received 48 per cent in 1973, the Social Christian COPEI received 37 per cent) are probably viewed by Venezuela's mobile and modernizing voters as old-fashioned and of decreasing relevance. He also suggested that the heated 1968 campaign may have cost the system legitimacy. The 1973 election outcome actually suggested the reverse. It seems logical to suggest that issue examination would have helped Myers' study; the 1973 results may have been perceived as a continuation of trends rather than a reversal.

Myers is descriptively very helpful. An appendix contains biographical notes on many principal actors. Maps and tables abound, and an early chapter sketches the institutional history of this century. Footnote citations, especially in the Epilogue, are exhaustive.

The careful balance of this work sets it in clear and advantageous contrast to several earlier volumes. For example, the heavily funded work by Frank Bonilla and Jesús Silva Michelena (appearing as two volumes of a three volume set, A Strategy for Research on Social Policy [Cambridge, Mass.: M.I.T. Press, 1967–70]) fell victim to their ideologies. Myers sets scholarly criteria for nearly all comparative analysis.

PHILIP B. TAYLOR, JR.

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Collective Security and the United Nations: A Definition of the UN Security System. By M. V. Naidu. (New York: St. Martin's Press, 1975. Pp. xv, 164. \$12.95.)

In this brief volume, Professor Naidu seeks to clarify the concept of collective security, to demonstrate the inappropriateness of that concept in discussing the theory and practice of the United Nations, and to suggest other terms which he feels are more applicable to the UN. Unfortunately, students already familiar with the existing work on collective security, especially that of Inis Claude and Ernst Haas, will find little of additional value in this new book.

The book begins with a familiar review of the varied, imprecise, and frequently contradictory uses of the term collective security. It then defines the "ideal" type, essentially following Cluade's definition; suggests the "analytical components" involved; and presents the "subjective" and "objective" conditions necessary for the successful operation of the ideal type as defined. While some of this discussion is useful, very little of it goes substantially beyond the existing literature.

The third and fourth chapters deal largely with the United Nations. The security system of the UN Charter is seen as an advancement over that of the League of Nations, but as falling short of collective security. The "most serious procedural limitations" of the UN system are "the voluntary nature of the Special Agreements through which the military forces for the United Nations are to be created, the right of individual and collective self-defense, the lack of a definition, and the right of veto given to the Permanent Members of the Security Council" (p. 62). Moreover, says Naidu, by failing also to meet the objective and subjective prerequisites, the actual practice of the UN has departed even further from the ideal type.

Professor Naidu suggests the term "collective measures" as most useful in characterizing the UN security system. This involves three kinds of activities: "collective military measures (the Korean type), collective police measures (the UNEF and ONUC type) and collective peacekeeping measures (the UNOGIL type)" (p. 76). All of this seems reasonable enough, although the definitions provided would not be easy to apply. Professor Naidu then goes on to distinguish these collective measures from preventive diplomacy, pacific settlement, and economic and diplomatic sanctions. His distinctions here, especially with regard to sanctions, are sometimes unclear. Also, the reader is left without an explanation of why collective measures are part of the security system of the UN, while these other instruments apparently are not.

The concluding portion of the text is essentially an extended summary, ending with an expression of the author's hope that acceptance of collective measures by the world community will lead to "an ultimate acceptance of the *ideal* type of collective security system" (p. 91). No attempt is made to suggest the processes by which the linkages here might come about.

The final summary is followed by forty pages of appendices, including excerpts from the League Covenant and the UN Charter, portions of two reports by Dag Hammarskjold and an address by U Thant, the texts of several General Assembly and Security Council Resolutions, and the text of the agreement between the UN and the Government of the Congo concerning ONUC. This is a limited but useful collection of documents.

While the book is generally accurate, it does contain occasional factual errors. At one point, for instance (p. 98), Naidu says that the Security Council declared China an aggressor in Korea. This action, of course, was taken by the General Assembly. A more glaring inaccuracy is the following statement: "In that tiny strip of a country, Vietnam, more persons have been killed, more land and property has been destroyed than in the entire Second World War" (p. 55). Such an extreme error should certainly have been caught before publication.

Overall, then, the book does not have a great deal to recommend it. Some worthwhile distinctions are made and some useful (but not unusual) documents are gathered in a single place. There is little, however, that is original and much that is obscure.

EDWARD THOMAS ROWE

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The United States and Britain. By H. G. Nicholas. The United States in the World: Foreign Perspectives Series. (Chicago: University of Chicago Press, 1975. Pp. 195. \$10.00.)

Publishers — especially, it seems, academic publishers — are incurable optimists. In recent years they have persuaded themselves that there is a real demand for "short" books on almost any aspect of American involvement in world affairs.

The books in this series propose to examine American relations with other countries through the perspective afforded by non-American scholars — in this instance the Rhodes Professor of American History and

Institutions at Oxford University. It is not a book for the specialist, and should not be judged as such. At the same time any author who accepts such a challenge wants to do more than merely provide undergraduates with a shortcut to "knowledge." And H. G. Nicholas clearly has such ambitions.

In places, he succeeds. Despite a tendency to write at relatively great length about relations during war crises at the expense of other aspects of the Anglo-American connection (ten pages for the Civil War, but only nine for the entire "imperial" era from 1895 to World War I), Nicholas covers the territory well, and even dots the landscape with new insights not found in either the "standard" works or in specialized studies. He is at his best in the sections that deal with Anglo-American relations from 1776 through the Civil War and in those that pick up the story from 1939 to the present. He is weakest in the middle section, where the reader is surprised by the absence of any reference to Anglo-American differences over Far Eastern policy or, stranger still, over the Mexican revolution. It is somewhat surprising, too, after all the recent monographic work on the Wilson Administrations to find the author of an up-to-date synthesis still treating Woodrow Wilson as a previous generation of scholars did i.e., as a disembodied idealist who floated about two feet above the ground, dispensing justice to all the world without a concern for America's true national interests.

While seldom straying very far from what might be called low-church orthodoxy, Nicholas nevertheless poses some real teasers. He suggests, for example, that Churchill's decision to send aid to Soviet Russia after Hitler's attack may have tipped the balance in favor of similar aid from the United States. Nicholas also makes an interesting case for British leadership in meeting the Soviet "threat" in the early days of the cold war. A Bevin-Churchill combination in the United Nations and at Fulton, Missouri, he implies, helped to overcome a strong disposition, "especially in the State Department, to keep outright confrontation with Russia to the minimum" (p. 115). That assertion will no doubt raise many eyebrows. It was a different story altogether, however, when it came to the cold war in the Far East. There British and American interests diverged, right from the time of General MacArthur's shogunate to Johnson's War in Vietnam. "Britain's overwhelming concern, second only to that of not getting involved herself, was to prevent the U.S.A. from becoming bogged down in a hopeless and costly struggle that would divert her attention, her will, and her primary tasks, as Britain saw them, elsewhere (p. 167).

Trying to explain Anglo-American differences over Asian policy, Nicholas remarks that Washington's sense of mission began to resemble the self-conceived role of the British raj in India. But where Britain had remained flexible out of necessity, in order to protect real economic interests, America's "Pacific Firsters" did not have that advantage. Lacking any "strong occupational or interest group base," the "Pacific Firsters" attracted the "Frustrated and Disaffected, the adventurers and the alsorans of every kind" (p. 133). Isolationism. denied its traditional role in the postwar world, "found a substitute existence for itself in a concern for America's place in the Pacific and in Asia, an area where the tentacles of Europe did not reach and the demands of allies were less vocal or persistent" (Ibid.).

After Vietnam it is hard to respond to arguments that stress the irrational, yet Nicholas himself points out that there was more to the American raj than misplaced idealism. "By the 1960s the U.S.A. was providing directly about half the capital required for investment in the developing countries of the Commonwealth, apart from the substantial provision made by the United Nations and its related agencies, most of which was ultimately traceable to the American Treasury" (p. 162). Perhaps both explanations are correct. At any rate, it is a problem more for the reviewer than the intended audiences.

LLOYD C. GARDNER

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Deutschland und der Kalte Krieg. By Ernst Nolte. (Munich: Piper, 1974. Pp. 760. DM 64.)

This is a large book: large in size, large in conception, large in shortcomings. The strengths and weaknesses of this work stem in good measure from its being essentially an effort in the history of ideas. Professor Nolte, well known as the author of books on fascism, applies a phenomenology of ideas to the foreign policy conduct of the major actors he deals with in this study: the United States, the Soviet Union, Germany. This method is Nolte's answer to what he calls the question about the "basic character" of the cold war. In raising this question, Nolte suggests

All other aspects can be pulled together, and the possible answers range between the narrowest possible — [the cold war seen as] a conflict, clothed in ideology, carried on from 1947 to 1953 between the only remaining world powers, the United States and the Soviet

Union, over European territories not unambiguously located in their respective realms (zugeordnet)—and the widest possible—[the cold war seen as] an ideological and power-political struggle (begun in 1917, and incipiently in 1776, extending into the unforeseeable future) about the future shape of a unitary world among several militant universalisms, of which each is in the possession of at least one great state (p. 39).

Nolte leans toward the "widest possible" end of the spectrum: "The Cold War belongs to the species of struggles between states or groups of states that are totally or partially determined by ideology. The narrowest possible definition is, therefore, to be excluded" (p. 45). This ideological characterization of the cold war, qualified as it is by the term "partially," leads Nolte to an examination of the intellectual and political "prefigurations" of the eighteenth and nineteenth centuries that he sees as ultimately culminating in the cold war. Early chapters in the book deal with "The European System, Its Left and Right"; "The First State of the Left: The United States"; "The Most Successful Left: Marxism"; "Marxism as a State: The USSR"; the early encounters between the United States and the Soviet Union; fascism; the dealings among the Soviet Union, Germany and the United States prior to World War II. From there, Nolte turns to an overview of the uneasy wartime coalition between the Western Allies and the Soviet Union; the "preparatory" phase of the cold war in the immediate postwar years; the Berlin Blockade; the height of the cold war and the partition of Germany; the origins of the Federal Republic and the GDR; the Korean War and the debate within West Germany about rearmanent; the Twentieth Party Congress and its meaning for Soviet foreign policy; the two German states as dual focal points of the cold war; Kennedy's "new realism" and the Berlin Wall; the Cuban missile crisis; the Test Ban Treaty; and finally the "détente" phase of the cold war, as exemplified in the nonproliferation treaty, Bonn's Ostpolitik, and the Strategic Arms Limitation Talks. In addition, at more or less suitable junctures, Nolte interjects short chapters that deal with "illuminating parallels" (Japan, Austria, China, Korea); "parallels and contrasts" (Israel, Vietnam); "contrasts" (the Arab states, North Vietnam, the People's Republic of China); the renaissance of the Left in the West; John Kennedy, Lyndon Johnson and Vietnam; self-determination in the Third World; the potential for a "cold war" between the Third World and the industrialized states; and many other topics.

In order to deal successfully with such varied and intractable materials an author needs plausible organizing principles. For Nolte these principles derive, by and large, from the intellectual and ideological climate of the times. Since ideas, and especially ideological systems, develop and mature slowly, Nolte's examination reaches back to the Puritans and the Founding Fathers, the origins of Marxist thought, the predictions made in the nineteenth century about a future American-Russian conflict, and so forth. Some of these early chapters make for interesting and profitable reading. Nolte often succeeds in unearthing intellectual precedents and traces them along their (sometimes) tortuous paths until they illuminate a point he wishes to make; he condenses skillfully, he has an eye for the telling detail, and his style of writing is generally readable.

But one remains uneasy. The "prefigurations" and their modern ideological and political manifestations traced by Nolte are often murky and appear somewhat contrived: in dealing with ideas anything that is said tends to do injustice to things that are left unsaid. In the case of the United States, for example, it is very doubtful whether there exists an intellectual tradition (not to speak of an ideology) that extends over 200 years and that has demonstrably and continuously affected American foreign policy. Nolte's treatment of the United States prior to World War II appears to be an almost perfunctory exercise, shot through with unexplained contradictions, and seemingly uninformed about the work of such historians of ideas as Louis Hartz, Daniel Boorstin, David Potter, and many others who have dealt with the impact (or lack of it) of "ideology" on American foreign policy.

When Nolte turns to the cold war years, his eclectic methodology leads to even more serious omissions, and hence distortions. Despite the wide range of topics dealt with in the book, there is no sustained discussion of militarystrategic matters; of economic and monetary arrangements and their imperial implications; or of European integration and its function for German recovery, for the cold war in general, and for shaping an alternative to existing political ideologies. Even in chapters that connect ideological "prefigurations" with cold war perceptions and attitudes, Nolte's argument tends to run parallel with historical reality and does not intersect with it, leaving unexplained how and why critical foreign policy decisions were made. In these parts of the book it isn't so much that one feels compelled to disagree with what is said; rather, one wishes to be shown if and where the world of thought and action

meet and merge. (An example: Nolte devctes considerable space to what is generally a balanced assessment of the development and revisions of Marxist-Leninist thought, but he shies away from dealing concretely with some of the central elements of Marxist thinking — national and international economic processes and socioeconomic configurations — as they exist in the "real" world.) Nolte's analytical approach is somehow disarming because normal standards of criticism seem inapplicable: questions of "correct" or "false" statements, or even interpretations, cannot properly arise, since very often one plausible generality is being contradicted by another plausible generality.

There is no reason to argue with Nolte's basic contention that ideas do matter in the conduct of foreign policy. But an approach which denigrates so insistently the dictates of circumstance, of power politics, of power economics in favor of the dictates of thought and ideology, should be more responsive to its own demands and possibilities. Perhaps that would require turning a set of ideas into a paradox. For it is at least conceivable that the wielding of power develops its own ideology, impervious to correction and immune to any dialectic except its own; and that imperial (as well as bureaucratic) compulsions derive as much from opportunity as from design.

WOLFRAM F. HANRIEDER

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South Africa in Africa: A Study of Ideology and Foreign Policy. By Sam C. Nolutshungu. (New York: Africana Publishing, 1975. Fp. ix, 329. \$20.00.)

"South Africa's policy towards the rest of Africa was a fundamental failure despite its apparent successes... Africa was not made safe for apartheid" (p. 298). This is the theme presented by a black South African political scientist in his lucid but lengthy analysis of the white regime's ambivalent response to decolonization in Africa. The volume is the latest attempt to describe, "explain and interpret" (p. vii) South Africa's foreign policy. The analysis is sensitive but inconclusive; it is only a modest advance on previous investigations, and it does not represent cumulative scholarship. It is a work of interpretation rather than a novel explanation or a theoretical presentation.

The book has two parts which adopt different levels of analysis, moving from national policy formation to regional relationships. The first section examines South Africa's policy toward colonial Africa and concentrates on its "great men," especially on Smuts, Malan,

Louw, and Verwoerd. The second half analyzes South Africa's regional interactions with the states of southern Africa, both black and white. Nolutshungu also deals with domestic constraints and changes within South Africa, especially with the evolution of its ideology. In particular he discusses white ethnic politics, characterized by "the growing emphasis on Realpolitik in South Africa and the supersession of some Afrikaner racial sensitivities by the ascendant principle of State interest" (p. 126).

The strengths of Nolutshungu's analysis are in his attention to detail and in his mastery of the subtleties of African international relations. The weaknesses of his approach, however, lie in his somewhat uneven and abstract style and in his propensity to tackle some rather obscure theoretical issues. Nevertheless, despite such unnecessary academic trimmings and the author's occasional tangential detour, this book does point to many of the contradictions and continuities in South Africa's interactions with Africa. In particular, it documents South Africa's continual but hopeless advocacy of a variety of economic and military regional organizations. It also provides insights into the. dynamics of regional politics, especially into the coexistence of conflict and cooperation in southern Africa. Nolutshungu usually adopts a dispassionate and objective mode of analysis which emphasizes continuities and constraints. Sometimes, however, he slips into a more critical form of inquiry in which he examines South Africa's "sub-imperial" role (pp. 171 and 302).

The author's focus on ideological conflict is not always sustained, and he remains concerned with economic and strategic factors; he does not confront alternative explanations and is generally uncritical about the economic forces behind South Africa's racist ideology. Moreover, his regional focus tends to reduce the importance given to South Africa's isolation in world politics and to its dependence on external trade and technology. The book contains several typographical and factual mistakes [Lesotho has not exchanged diplomats with South Africa (p. 89)]; and the tables and bibliography are rather dated and abbreviated.

Nevertheless, this volume deserves to be read by students of African and international politics; it is not a definitive work, but it does contain information and insights not readily available elsewhere. In particular, Nolutshungu discusses factional politics in South Africa in response to international crises and to a changing African environment. He also starts a useful comparative analysis of the foreign policies of

the black states, especially examining the different stances of Botswana, Lesotho, and Swaziland, and of Malawi and Zambia toward South Africa. His analysis of factionalism over Malan's African Charter and Vorster's "outward-looking policy" are informative; and his investigation of the "linkage politics" of conservatism in Black Africa are perceptive and suggestive (pp. 279-286). Despite the several limitations of his substantial manuscript, Nolutshungu does identify the major contradiction in South Africa's search for regional hegemony: "If the quest for influence in the continent arose out of a desire to facilitate the retention of white privilege within South Africa, white minority rule itself was the principal obstacle to South African policy" (p. 1).

TIMOTHY M. SHAW

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Integracion Andina. By Edgar Camacho Omiste, (La Paz: Editorial Los Amigos del Libro, 1975. Pp. 254. Price unknown.)

In recent years American political scientists have begun to recognize the utility of a political economy approach to political problems. Nowhere is this more evident than in the analysis of international phenomena. The interdisciplinary perspective adopted by Edgar Camacho Omiste, a Bolivian scholar and diplomat, in his study of Andean integration will thus be of some interest to the American academic community.

For Camacho, "to study an historical event from a predominantly political perspective, signifies that one is examining it in its intimate links with the nature of the State, the relation of classes which gave it rise and sustain it..." (p. 43). He shares the view put forth by Marxist theorist Nicos Poulantzas that the state reflects the political interests of dominant classes in the society (p. 230). From this perspective, regional integration can be seen to form part of the domestic political strategy of the governing social forces. Yet as Camacho points out, the Andean Group initially had the support of diverse political groups. How and why did this support come about?

"The principal barrier to unity and commerce between neighboring countries," Camacho argues, "is the prevailing international economic structure and the world division of labor which determines in large part the social and political conditions of each of the countries individually and of all of them as a whole" (p. 46). Most notable among these conditions is the distorted pattern of Latin American industrial development characterized by a high concentra-

tion of production in consumer goods, small scale, inefficient production and little technological assimilation. Those who manage this sector have become a ruling class "comfortable in their role as a protected minority shareholder" and have thus never developed into a dynamic national bourgeoisie capable of leading their countries toward self-sustaining and independent development (p. 87). To preserve their position, the state provided them with the necessary high protective tariff barriers.

In the 1950s the Economic Commission for Latin America (ECLA) advocated regional integration as a means to overcome the narrow markets of most Latin American countries and improve possibilities for further import-substituting industrialization. The Central American Common Market and the Latin American Free Trade Association were modeled on this approach. According to Camacho, integration in these two systems, however, did not promote development. Rather, it increased reliance on direct foreign investment and provided a windfall for multinational corporations who were the only actors capable of taking advantage of the opportunities offered by the enlarged. protected market. Because the net beneficiaries in these two systems were the multinational corporations, it became commonplace for radical critics to dismiss integration as a strategy for dependence rather than development. Camacho, however, argues that integration is a strategy which can be differentially used for conservative or progressive ends depending upon the type of state initiating and carrying it out (p. 231). But if an integrative system is to function as a progressive strategy, it must meet certain minimum requisites.

To overcome the problems of dependence, an integrative system must stimulate the development of technology within nationally owned industries and must regulate the process of direct foreign investment. In addition, where an equitable distribution of benefits is not forthcoming because of unequal levels of industrial development among the member countries, Camacho strongly advocates mechanisms to insure a more equitable distribution of benefits. The principal mechanism is joint industrial programming. Camacho thus explicitly rejects the argument that within the capitalist mode of production, balanced development is impossible and that the system will always be characterized by disquilibria among industrial sectors, among regions within a country, and among countries (p. 142).

The Andean Group embodies each of the minimum requirements for integration sketched above. It accepts the need to distribute equitably the benefits derived from integration and has an elaborate system of industrial programming in process. In Decision 24 of the Junta del Acuerdo de Cartagena there is a measure to stimulate technological development and control the process of direct foreign investment. In discussing this unique common code, however, Camacho argues that it "established that foreign firms would have to transform themselves gradually into national or mixed entities within fifteen years, extended to twenty in the case of Bolivia and Ecuador" (p. 175). Yet whether divestment is required of all foreign firms both old and new is hotly contended within the Andean Group today.

In his conclusions, Edgar Camacho Omiste returns to the theoretically interesting question of why diverse groups and classes lent their support to the Andean Group. His answer, however, presents us with something of a paradox. The extent to which multinational corporations had penetrated the Latin American political economy and the growing integration of Latin America "with the hegemonic centers of the capitalist system," he argues, were becoming obvious to the "underdeveloped industrial bourgeoisie" in these countries who saw such processes as tending to result not only in their absorption but in their very destruction. "Under such conditions, the viability of these social classes and of the national states that represent them would appear to depend upon the modalities of their insertion into the world scene" (p. 225). Integration, then, has a clear utility. Yet this argument assumes that the "underdeveloped industrial bourgeoisie" indeed favors a progressive form of regional integration. It does not explain why the bourgeoisie behind the Bolivian state, for example, sees Andean integration as a means of maintaining its dominant position - witnessed by their joining the Andean Group - but Bolivian industrialists have gone on record as opposing a mechanism like Decision 24 which could, according to Camacho, be an instrument for insuring their survival. Camacho has pointed the way for others to plunge deeper into the analysis of such contradictions.

LYNN KRIEGER MYTELKA

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International Marine Environment Policy: The Economic Dimension. By Charles S. Pearson. (Baltimore: The Johns Hopkins University Press, 1975. Pp. 127. \$9.00, cloth; \$2.65, paper.)

Can human beings survive on a planet with a

dead ocean? This book does not attempt to provide a scientific answer to that question, but it is concerned with programs and policies that may help future generations avoid having to live with that threat made real. The book, in brief, asks and seeks to provide answers to the basic questions concerning the maintenance of ecological stabilities of the world ocean. The author examines the nature of the problems facing the ocean environment today and in the future and describes and evaluates some of the major collective actions undertaken by global and regional groups to try to cope with them. The problems are approached mainly from an economic perspective, although political factors are also discussed.

Without question, protection of that great commons, the world ocean (look at a globe and you can see that all of the world's oceans are linked together) ranks high or highest in the order of environmental values and priorities. There is abundant evidence, however, that human beings in this technological age are polluting the ocean at a rapid pace, but how dangerously is an open question that United Nations' monitoring and research programs are attempting to discover.

The author, who is a professor in the School of Advanced International Studies of Johns Hopkins University, begins by making a case for governmental intervention. But the difficulty of dealing with marine pollution is that in many cases the problem — or at least the solution — is global or regional, whereas government, in the usual meaning of that term, is not. The author also demonstrates that private enterprise, operating within a market economy, is not suited for the job. In the absence of national governmental and private solutions, the conclusion is obvious that only cooperation and consensus engendered through the means provided by international organizations - public and private, regional and global - are available to meet the challenge.

Can international organization do the job of meeting the future challenges that most indicators predict will be staggeringly difficult as we near the 21st century? The author's review of what has transpired thus far during the 1970s to cope is partly encouraging and partly discouraging. The 1972 U.N. Conference on the Human Environment (Stockholm Conference) was a major motivating force, and the Declaration of Principles and Recommendations that was contained in its Action Plan clearly prescribes what states must be willing to do to avoid ocean catastrophe. Creation of the United Nations Environment Program (UNEP) with its subsequent operational activities spanning the

globe was a major step forward. Numerous international lawmaking treaties have also been concluded, but few have been put into effect with sufficient ratifications. Two are of particular importance, and the author has rightfully devoted a chapter of his book to each. The Convention on the Prevention of Marine Pollution by Dumping of Wastes and other Matter (Ocean Dumping Convention) was agreed to in November 1972, and the following year the International Convention for the Prevention of Pollution from Ships was concluded. Yet these conventions and the problems they seek to solve are only peripheral to the real pollution problems that are national in origin. The author recognizes this situation in noting that "the oceans lie below the continental land masses. and the laws of gravity make them the natural sinks for societies' wastes" (p. xi). Related, of course, are the difficult problems of achieving political agreement that will limit national actions within sovereign boundaries, and the failure to date of the world community to agree on the limits of national jurisdiction and the internationalization of the ocean and ocean floor beyond.

The book, although brief, is a valuable contribution to the growing literature in the ocean-environmental field. It contains a wealth of informative statistics in a number of tables. The author, however, fails to describe the important informal enforcement roles carried on by international agencies, such as the voluntary compliance with IMCO rules by shipping companies which results in lower rates charged by insurance companies like Lloyds of London that classify ships for insurance purposes. On balance, however, the book is a significant contribution to a field that will continue to increase in importance.

JACK C. PLANO

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A Memoir of China in Revolution: From the Boxer Rebellion to the People's Republic. By Chester Ronning. (New York: Pantheon Books, 1974. Pp. 306. \$10.00.)

Chester Ronning's life has included several careers. His father came from Norway, graduated from theological seminary in Minnesota, and became a missionary in Fancheng on the Han River in Hupei province where Chester was born in 1894. He grew up speaking Chinese with his schoolmates. His family escaped the Boxer year by the accident of being on leave in Norway and the United States. Accustomed to speaking Norwegian with their father, the Ronning children spoke English with their mother.

After she died in 1907, Chester and his brother became Americans at school in Iowa, but soon they joined their father, who had bought land in Canada, and the whole family became Canadians. Chester really grew up in Grande Prairie where he soon aimed to become a rancher, but to make a living he had to start teaching. Once he got through a Lutheran normal school, he found himself a sapper and then an air cadet in World War I, although the war ended before he saw combat. So in 1921 at the age of twentyseven he went back to China, had a year of language work in Peking, and became principal of a middle school in his home city of Fancheng. This was his first experience of revolutionary China - in the 1920s when the Nationalists and their Kuomintang were coming into power with a new regime and much hope. He saw something of the capacity of Chinese students to organize in protest against imperialism.

This first phase of his international experience was ended in 1927 by the missionary evacuation from China. Chester Ronning found himself back at the Lutheran college in Canada where he had learned his teaching, and for fifteen years (1927-1942) he served as a college president. Thus his career in the international world caught up with him only when he was almost fifty. In the war against Japan he was in the Canadian Air Force intelligence intercepting Japanese communications. Only in 1945 did the Canadian Department of External Affairs advertise for a Chinese-speaking assistant to the Canadian ambassador in Chungking. Ronning was asked first to be the examiner. but after VJ day he was sent to China as first secretary of the embassy for a trial year. From there on his career broadened out. He became indispensable in the embassy in China, acting sometimes as chargé. Later he returned to the Department of External Affairs in Ottawa, went as ambassador to Norway and then as high commissioner to India, and served as acting head of the delegations from Canada to the Geneva Conferences on Korea in 1954 and on Laos in 1961-62. Obviously, for those who are prepared with a multilingual, multinational background, the troubled world will have a place.

His book is a memoir in the proper sense, a reminiscence of various phases of his career. There are chapters on ancient China's collapse, on the 1911 revolution and the split in the united front of the 1920s ("the 1927 disaster"), and on the Communist long march and establishment at Yenan. These chapters make plain that Mr. Ronning had from the first been sympathetic to his revolutionary students of

the 1920s and to the real and urgent problems of the Chinese populace. Going to China in 1945 he ran into the Communist Party leader Tung Pi-wu in Calcutta, on his way back from the founding of the United Nations at San Francisco. Having left China just as the Communist-led social revolution was suffering betrayal doubly at the hands of Stalin and of Chiang Kai-shek, Mr. Ronning returned eighteen years later to find the revolutionary movement in full flower but no more in love with the Russians than it had been before. His journal in Chungking and later in Nanking from 1945 to 1949 gives interesting sidelights on the events of the time - the decline of the Nationalist regime, the beginning of civil war, the Marshall mediation, the eventual collapse and Communist victory. When Huang Hua, who became the permanent representative of the People's Republic of China to the United Nations, in late 1949 called together the foreign diplomats in Nanking, newly occupied by the Communist forces, he addressed them in Chinese and offered no English translation. This was to mark the end of China's subservience to imperialism. Chester Ronning was the only diplomat present capable of making the translation into English for his colleagues.

In the twenty-five years since the establishment of the People's Republic Mr. Ronning has been in numerous posts concerned with China. For example, when he was Canada's high commissioner to India he was much concerned with the Vietnam War and Chinese policy toward the American intervention there. At the Geneva Conference on Korea in 1954 he saw his old friend Chou En-lai and John Foster Dulles, among others, close at hand. He gives many interesting and personal sidelights on the American military efforts in China, in Korea, and in Vietnam. Without stooping to outright denunciation, Mr. Ronning cannot conceal his sense of dismay, bafflement, or outrage on numerous occasions when American policy seemed to him either woefully misguided or unconscionably aggressive. To an even greater degree than the American foreign service officers in China, Chester Ronning had experienced at first hand, as a close observer, certain distinctive phases of the Chinese revolution. He not only saw the old order at the end of the Ch'ing in his native city of Fancheng; he also superintended a middle school during the revolution of the 1920s. After 1945 he was intimately connected with Canada's China policy. He knew which way the winds of history were blowing in China. Below the level of ideological rhetoric he could see the futility and downright evil of certain American outworn attitudes and purblind policies. He helped Canada to record its continual misgivings, even when for reasons of realpolitik the Canadian policy had to go along with that of the United States or at least not counter it directly. Mr. Ronning is a kindly man but a man of conviction based on experience, and his picture of the American performance is not flattering.

J. K. FAIRBANK

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Defence Yearbook 1974. Edited by the Royal United Services Institute for Defence Studies. (New York: Praeger Publishers, 1974. Pp. 338. \$20.00.)

This compendium of articles on military issues and developments in military science belongs in the reference library next to the Yearbooks of the Stockholm International Peace Research Institute and the monographs of the International Institute of Strategic Studies. It is in the tradition of the best military writing, seeing the central issues of defense not solely in the classical terms of strategy and weaponry but in the broad context of international political and economic affairs.

Those whose interests lie in the direction of arms control and reduction may find the orientation of the Yearbook too military. It is difficult to assign a single classification to a work to which so many extremely knowledgeable experts have contributed, but I think it would be fair to say that the contributors tend to view the strategic issues of the times in terms of conflict rather than conflict resolution. This is, after all, a defense (read military) annual. It does not subscribe to a concept of diminished polarization of world politics. The issues that it identifies are seen essentially in terms of antagonisms, crisis, and military threat, and the means of addressing them as military means.

The eleven authors of Part I of the volume are primarily British and American, with the addition of a French and a German contributor. The author selection then is international but essentially Alliance international. This need not necessarily produce a we/they point of view, but the volume does quite often have a NATO ring to it, on guard against the Soviet threat, whether in relation to the Arab oil weapon which "it seems likely that the Kremlin encouraged - even if it did not indeed demand" (p. 79), or the Soviet threatened intervention in the 1973 Middle Eastern War, which "exposed the shallowness of her [Soviet] sincerity over detente" (p. 159). The NATO stamp is reenforced by the fact that Part II of the book, presumably prepared by the Institute staff. includes specific recommendations for the Western forces on weapons priorities.

Those qualifications aside, the high professional quality of the entire collection should be emphasized. The authors, who are identified at the start of each article with a brief descriptive paragraph on their backgrounds (a useful practice which more compendia should follow), are associated with universities or with institutions specializing in defense studies. All are distinguished specialists in the subjects on which they write.

The Yearbook itself can boast an impressive heritage. Previously Brassey's Annual, it began in 1886 as a British Naval Annual, intended to study the events of the year, draw lessons for the future, and stimulate an informed public debate. The Royal United Services Institute for Defence Studies, with which it is now joined in the publication of the present Yearbook, is the oldest institute of its kind in the world. Established in 1831 for the study of British defense and overseas policy, the Institute, like Brassey's, broadened its interests to cover defense in an international context and to reflect the views of the range of disciplines - political science, economics, behavioral and physical sciences - which today are concerned with military matters.

The scope and complexity of the subject matter in the 1974 Defence Yearbook make it impossible for a brief review to evaluate the individual presentations. A listing of topics in Part I should give a sense of the broad range of content: China's strategic posture in a changing world, the 1973 Middle East War, the continuing crisis in Southeast Asia, the strategic aspects of the energy crisis; also four essays dealing with the superpowers: their arms negotiations, confrontation at sea, strategic balance in the Mediterranean, and the global balance; and two focusing on France: the economics of French strategy, and French policy and the defense of Europe.

Each subject is treated in a thoughtful analytical discussion backed with historical and factual detail. The authors succeed in adding clarity and information even to those topics which have had constant attention in American journals and the press.

Part II of the volume is encyclopedic in its coverage of the state of the military arts. It covers the full range of weapons systems in service or under development, Soviet as well as Western. The publisher indicates that this section will be updated in successive editions. It will therefore serve as a continuing and invaluable source of information on the development of weapons technology.

Whether or not one feels the need to know such details as the tonnage of the principal classes of nuclear attack submarines and the range of all antitank missiles, the weapons section is worth perusal by anyone with even a remote curiosity about what is being bought these days with the \$60-70 billion that is spent annually for weapons in the world. It may be useful to know, for example, that the modern weapons inventory includes enhanced radiation mininukes that will kill people but spare buildings, unmanned aircraft that can be controlled by a pilot sitting in perfect comfort many miles away, smart bombs which home on the target by means of laser guidance with almost zero error. These are the kinds of things that have revolutionized warfare and that carry the promise that the next battlefield could very well be your backyard and mine. It is as good a reason as any to read a book.

RUTH LEGER SIVARD

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The King's Parliament of England. By George O. Sayles. (New York: W.W. Norton and Co., 1974. Pp. 164. \$7.95, cloth; \$2.95, paper.)

This monograph in a series entitled Historical Controversies summarizes forty years of research by Professor Sayles (aided by H. G. Richardson). Its significance to political scientists, in particular to students of legislatures and their development, lies in the challenge to a set of "myths" generated over several centuries regarding the genesis and early history of parliament in England, myths which no doubt influenced attempts to transplant parliamentary institutions in the British Empire and elsewhere. Professor Sayles seeks to dispel the view that the parliament of the seventeenth century had its roots in the medieval parliaments, by looking at parliament through the eyes of the men of the time, and examining their perceptions of its function. He emphasizes the minor, even negligible, part played by representatives of the people, asserting that parliament was but one of several devices used by monarchs to settle questions of law and administration.

Sayles attacks several studies which he believes distort seriously any understanding of the medieval parliaments of England. Seventeenth-century writers, he argues, were less concerned with what actually happened in the past than with those things they considered important in terms of the current political debate as to whether the king alone, or the king and parliament together, was to be supreme. Such constitutional controversies were not, however, those of the Middle Ages, the problem of

sovereignty never being posed in the form in which it appeared in their own day. He also charges that writers of official reports and major studies in the nineteenth century looked for the origins of democratic institutions at too early a date. Such "conventional wisdoms" about medieval parliaments were given further credence in 1951 by the foundation of the History of Parliament Trust. Sayles essentially argues that if we see in parliament "merely a public spectacle of political struggles between Crown and Commons and of democracy in embryo, we shall certainly never understand the medieval parliaments" (p. 18). Above all, it was the king's parliament, and agreement - not dispute - was the name of the political game. Sayles guides the reader through the evidence which sustains his interpretation — i.e., the medieval parliaments ended in strong monarchical rule, not in constitutional democracy. According to Sayles, Henry VIII, in 1529, deliberately selected parliament as an instrument to his liking, setting it on a new course as a legislative assembly, with no reason to suspect that cooperation would eventually turn into control. Medieval parliaments were the creatures of the king, summoned or not as he pleased, listened to or not as he deemed fit, and with no individual right of attendance - a device of government with an uncertain tenure, liable to be outmoded and superseded or remodeled and revivified.

This cogent, erudite argument for a history of parliament written not as the seed-bed of representative government, avoids the trap that historical continuity implies identity of function, despite the changing needs of society. Thus it provides an important lesson for students of political development no less than students of legislative assemblies.

JOHN D. LEES

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Freiheit — Die Anfänge der Bürgerlichen Emanzipationsbewegung in Deutschland im Spiegel ihres Leitwortes. By Jürgen Schlumbohm. (Düsseldorf, West Germany: Pädagogischer Verlag Schwann, 1975. Pp. 299. Price not listed.)

Jürgen Schlumbohm is concerned with analyzing the several meanings of the word "freedom" as this word reflected social and economic changes in German society between 1760 and 1800. His approach to this challenging task is Marxist. The author is quite precise in stating this. For example, on page 13 we find the following: "the contents of consciousness and their verbal expression are essentially con-

ditions through the constellations of social reality: however, thinking and speech in turn also have reciprocal effects upon social existence — only under these presuppositions does political speech and writing truly possess a real function." Throughout the work, Schlumbchm attempts to adhere to the method implicit in this statement. On the whole, he succeeds quite admirably.

By examining the writings of political publicists, Schlumbohm is able to demonstrate that there was indeed correspondence between changes in the ideational content of the word "freedom" and changes in society. For the somewhat antediluvian nobility - the supporters and beneficiaries of a half-mythical Ständestaat - freedom meant freedom from the several varieties of princely authority as well as the preservation of certain privileges, e.g., hunting or water rights. These remnants of a feudal social order were extent in Germany even as the nobility became a capitalistic class economically. For the bourgeoisie, advancing economically if not necessarily socially and politically, freedom became a far more complex concept. It implied freedom from feudal constraints upon social mobility. In this context it was "the advance toward [the] elimination of hindrances [to] social ascent which lay in the feudal social order" (p. 93). Freedom, however, also could and did imply such things as freedom of the press, freedom of conscience, and the freedom to advance economically. Obviously, according to Schlumbohm, this last "freedom" was of the greatest importance for the bourgeoisie.

Thus, the period under question saw the clashing of several notions of freedom, each representative of a particular class. The nobility and its supporters - particularly after 1789 sought to defend its rights and privileges by appealing to a more "traditional" conception of freedom. Buttressing this position was an increasing tendency, as exemplified by Friedrich Gentz, to call upon Germans to defend their own particular "national freedom." This was a potent weapon. "For when the bourgeois emancipation movement in Germany oriented its conceptions of freedom toward the model of progressive Western lands, the validity of such 'foreign' models, on the other hand, now was opposed [on the basis of] principle" (p. 70). Concepts of "Natural Law" could be used to defend the nobility's position; but usually they served the more critical function of picking apart the old social order by revealing its lack of congruence with a presumed natural order in which all men ought to be "free," if not necessarily equal. The Natural Law argument,

combined with an increasing emphasis upon a "Rechtsstaat" free from noble privilege or absolutist caprice, constituted the salient points of the bourgeois argument in favor of their particular permutation of freedom. Yet, despite the virulence of the clash between the several concepts of freedom, the bourgeoisie remained politically, and even to some extent socially, weak during this time. Hence, in the hiatus between things as they were and things as they ought to have been, there developed that peculiarly German (at least in the modern world) sense of "inner freedom," both a result of and a justification for, political inaction. Schlumbohm ends his work by considering the "radical" challenge to the proponents of the bourgeois conception of freedom - that of economic equality, i.e., of true social freedom. It was out of this confrontation that there developed the first great challenge to capitalism, even before large-scale capitalism predominated in Germany.

Schlumbohm has written a work that is of immense value to anyone — political scientist, historian, or philosopher — who is interested in how a word like "freedom," always so pregnant with meaning, can have its ideological and hence its ideational content radically altered by social and economic change. At the very least, Schlumbohm has vindicated the heuristic value of the Marxist method.

There are, however, several problems - and at least one irritant - in this work. Schlumbohm, perhaps out of some defensiveness arising from his Marxist approach, spends far too much time on methodological questions. In this regard, his introduction, which is only nine pages, reads as if it were thirty. Also, Schlumbohm from time to time raises issues in such a fashion as to suggest that he is about to offer a radical new interpretation of some phenomenon or other, but fails to follow through. For example, at the beginning of Chapter I, Schlumbohm seems to suggest that bourgeois economic development in the 1760s and 1770s has been grossly underestimated by conventional historians. By the end of the chapter, however, he has come around to the more traditional interpretation, viz., that the German bourgeoisie, while gaining in economic strength throughout Germany and actually strong in certain areas, was still relatively small and quite weak politically and socially. This conclusion is repeated at the end of the book. One problem which the author does not consider at all concerns the somewhat anomalous position of the German nobility. By the middle of the eighteenth century, this class had been practicing capitalistic agriculture for some time. Yet in

terms of the *social* relationships between it and other classes, the nobility remained happily ossified in feudalism. The problems raised here for Marxist interpretation in general and for Schlumbohm's in particular are great indeed. But, as far as I know, *no* Marxist ever has succeeded in dealing with this question convincingly.

Finally, Schlumbohm has a perhaps culturally conditioned tendency to carry on a substantial part of his argument in the footnotes. This is particularly annoying in the introduction and in Chapters III and V. Much of the material in his footnotes, replete with substantive argument and documentation, should go into the text. It is true, of course, that many classical authors, most notably Gibbon, added a certain amount of zest to their analyses through their pungent use of footnoting. But even bearing in mind the substantive qualification that this work is not in English, Schlumbohm, alas, is no Gibbon.

These criticisms notwithstanding, Schlumbohm's work is important for any scholar interested in the unhappy vicissitudes of the concept of "freedom" in Germany. Both because of the method employed and the wealth of hitherto unknown evidence provided, this book is highly recommended.

ROBERT A. POIS

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Fourth Shore: The Italian Colonization of Libya. By Claudio G. Segrè. (Chicago: The University of Chicago Press, 1975. Pp. 237. \$15.00.)

Political scientists and historians interested in European imperialism and in modern Italy will be grateful for Professor Segrè's work, which well deserves a place in the University of Chicago's Studies in Imperialism series. It is precisely what a good monograph should be: concise, clear, and based on extensive original research on an important topic. It analyzes Fascist policy from a perspective that has thus far received little attention; and it will provide useful data for comparative studies of European imperialism in North Africa. The first important work on this subject to appear in any language, Fourth Shore will be read with profit on both sides of the Atlantic.

Although Professor Segrè is primarily interested in the imperialist policies of Italy's Fascist era, particularly in the plans and activities of Volpi and Balbo, he is careful to place the Fascist colonization of Libya in historical perspective. The first part of his book shows

the remarkable degree of continuity that existed between the economic and demographic preoccupations of the liberal era, the Nationalist myth of "proletarian imperialism" (as distinct from the imperialism of the rich industrialized nations) and the Fascist attempt to turn Libya into a political and economic extension of the mother country.

In the second and much longer section of his work, Professor Segrè traces the history of Italian colonization in Libya from the end of Turkish rule in 1912 to the Fascist era. He shows that from the very beginning of Italian involvement, some politicians, economists, and intellectuals had grave doubts about the wisdom of holding on to what they derisively called "that big sandbox." Their misgivings were compounded by a long and costly fight against native Arab insurgents unwilling to exchange their Turkish masters for Italian ones. As for Italy's land-hungry peasants and unemployed workers, it was to America, not to Italy's Empire, that they looked for a more promising future. But the restrictive American immigration laws of the 1920s and the Depression later on revived interest in the colonization of an officially "pacified" Libya. As Professor Segrè shows, private capital could not provide either the means or the initiative to promote peasant colonization. Although the Fascist regime had embraced the old Nationalist dream of a Fourth Shore in North Africa, it discovered through the experiences of Volpi and De Bono that costly infrastructures would have to be built before peasant families could be settled in Libya. Thus, the actual building of the Fourth Shore was delayed until the years immediately preceding the Second World War. The advocates of colonization then found an ideal propagandist and organizer in Italo Balbo, whom Mussolini wished to keep as far away from Rome as possible. Professor Segrè's portrait of Balbo is perceptive and informative, if perhaps a bit too laudatory. He too seems to have succumbed to the charm and enthusiasm that made Balbo one of the most popular (and dangerous) figures of the Fascist regime.

In three very interesting essays, Professor Segrè examines the rise and decline of the Fourth Shore from the perspective of the colonists, of the technical experts involved in the project, and of the Libyans themselves. Together with his pages on Balbo, these essays document his main thesis. Although inspired by economic and demographic pressures, Italy's colonization of Libya was above all a political enterprise, designed to enhance the demestic and international position of the Fascist regime. It could not have materialized without a mas-

sive commitment of state resources which previous Italian governments had been unwilling to provide. Despite the hard work and sacrifices of many Italians, it could not survive the downfall of the regime.

CLARA M. LOVETT

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The Americas in a Changing World. Selected Papers. By Kalman H. Silvert et al. (New York: Quadrangle/The New York Times Book Co., 1975. Pp. 248. \$3.95, paper.)

This book includes a collection of scholarly essays by specialists in Latin American affairs and the lengthy Report of the Commission on United States-Latin American Relations. Both the report and the essays are unified by a common theme which argues that the last few years have witnessed fundamental changes throughout Latin America and that the United States must thoroughly reconstruct its foreign policy in response to these changes.

The Report of the Commission enumerates thirty-three specific policy recommendations. These recommendations are directed at achieving five policy objectives, which include strengthening Latin American cooperation in the global context, heightening our sensitivities to Latin American interests, eliminating paternalistic and discriminating programs, preserving human rights, and promoting synergistic economic relations between Latin American nations and the United States. None of the recommendations is particularly novel or striking to the student of Latin American politics. The value of the Report is not in its freshness but in its excellent integration of these previously articulated recommendations into a unified policy statement based on carefully conceived and explicitly articulated assumptions about the present state of the world.

In its factual thoroughness and theoretical breadth, the Report previews much of the substantive material which follows in the essays. Although the close relationship between the materials in the Report and the essays contributes to the unity of the volume, this unity is not achieved without some cost of repetition to be born inevitably by the reader. Taken individually, however, the essays are quite readable and offer a limited range of approaches and suggestions for restructuring U.S.-Latin American policy.

The opening essay by Kalman Silvert argues that the United States must carefully tailor its foreign policy to respond to the different levels of political development of each of the Latin

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American nations. Silvert outlines a developmental typology of Latin American polities to be used by policy makers in matching policy to levels of development and determining the appropriate form of U.S. intervention for individual Latin American nations.

Like Silvert, Riordan Roett notes the importance of political differences among Latin American nations and cautions against formulating a comprehensive regional policy for the area. Unlike Silvert, however, he focuses on national differences in geopolitical influence rather than developmental regime form. Roett convincingly argues for a pragmatic U.S. policy which would recognize the emerging international pecking order in Latin America and which would channel most of our foreign policy efforts into those nations among the upper echelons of this order.

A more global perspective is offered by Stanley Hoffmann, who describes a world that is still dominated by superpowers restrained by nuclear considerations but vulnerable to countless opportunities for both international terrorism and localized interstate chaos. Since the U.S. cannot preserve world order by itself, argues Hoffmann, it must enlist other nations by democratizing world politics and encouraging their participation in the management of international affairs. Hoffman therefore concludes that U.S. policy toward Latin America should facilitate a more responsible role for Latin American nations in managing the international community.

Jorge Dominguez analyzes Cuban/U.S. relations as a series of games alternatively involving various actors including the superpowers, multinational corporations, other Latin American governments, and domestic U.S. interest groups. After describing a range of conceivable U.S. policy options toward Cuba and discussing their comparative costs in terms of relations with the aforementioned actors, he finds that a policy of sufficiency involving little change is preferable. In his conclusions, Dominguez is somewhat at variance with the prescriptions of the Commission's Report.

Robert Cox presents a sobering analysis of policy options and their attendant costs in the Canal Zone. For the United States, reveals Cox, the Canal is a military liability and yields little economic benefit; for Panama, however, it is the most vital and salient issue on which President Torrijos has staked his political life. Cox stresses the certainty of bloodshed and damage to the U.S. hemispheric image absent an immediate accommodation which will minimize the U.S. presence in the Zone.

Changes in Latin American security condi-

tions, notes David Ronfeldt, have prompted military leaders to shift their concern from problems of domestic violence to the deterrence of external aggression. Like the Commission Report, Ronfeldt sees the role of future military assistance to the region as largely diplomatic. He argues that such assistance should strive to minimize Balkanization, facilitate military technology transfers, and maintain the necessary contacts for mobilizing military joint ventures.

C. Fred Bergsten assumes that economic issues will provide the impetus for U.S.-Latin Relations in the future. He argues that to achieve its domestic economic objectives, the U.S. must retreat from a policy which treats Latin America as a special regional bloc. Alternatively, he suggests that the U.S. should conduct multilateral trade negotiations with Latin American nations as members of worldwide producer groups or specialized cartels where appropriate. Given the reality of transnational economic interdependence, the U.S. should take the initiative in imposing control on multinational enterprises and must promote global programs for the benefit of both the United States and Latin America.

The concluding essay by Roger Hansen further describes the magnitude and breadth of the changes in Latin America noted by the Commission and other authors. Hansen is pessimistic about the ability and willingness of the U.S. to respond completely to Latin America's requests for revisions in foreign policy. His essay rejects the soundness of alternative policy orientations grounded exclusively on either bilateralism or a special relationship with Latin America. Rather Hansen recommends a globally oriented foreign policy with a tilt toward Latin America which recognizes the historically significant institutional and economic linkages between the U.S. and its Latin neighbors.

The recommendations of the Commission and the individual authors are generally liberal in the context of American foreign policy, but only moderately so when viewed from a more global perspective. Third World nations and their advocates will find the recommendations unresponsive; and for different reasons, powerful North American pressure groups will find them inconsistent with their interests. This provides grounds for the political cynics among us to question not only the practicability of these recommendations, but also the weight of their potential impact in prompting changes in U.S. foreign policy.

WILLIAM M. BERENSON

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The Strained Alliance: Peking, P'yongyang, Moscow, and the Politics of the Korean Civil War. By Robert R. Simmons. (New York: The Free Press, 1975. Pp. 287. \$10.95.)*

This is an ambitious book. For the author sets out to demolish the conventional wisdom about the origins, conduct, and consequences of the Korean War and replace it with a set of new hypotheses. Nevertheless, as Professor Simmons admits at the outset, it is "not a definitive work." What is disappointing is that his hypotheses are so precariously grounded that their plausibility remains exceedingly low.

The thrust of his arguments is aptly summed up by the title and subtitle of the book: that the alliance between the three Communist nations during the Korean War was by no means harmonious but rather was strained; that the Korean War was primarily a civil war whose causes must be sought in the internal situation in the Korean Peninsula rather than in external factors; and that the dynamics of that "civil war" were closely linked to the strains among Peking, P'yongyang, and Moscow.

In order to bolster his arguments, the author examines a wide array of materials, with particular emphasis on transcripts (for the most part, in English translation) of the Chinese, Russian, and North Korean radio broadcasts. His principal methodology is Kremlinclogy—that delicate art of scrutinizing the esoteric Communist communication in an attempt to decipher its hidden meanings and messages. Although, given the paucity of usable data, Kremlinology is widely used in Communist studies, its practitioner is nonetheless constrained by the canons of scholarship and the rules of logic.

Even if one keeps in mind the author's explicit disclaimer that his are nothing more than hypotheses, one is still entitled to ask, of a scholarly work, whether the hypotheses have a reasonable basis — whether the evidence the author presents does in fact support or, at the least, can be reasonably construed as supporting, his conclusions or inferences. When Simmons's study is subjected to the preceding test, it does not fare too well. Due to the limits of available space, only a few of the problems will be discussed below.

As noted, one of the author's major contentions is that the Korean War was a civil war — that, in particular, North Korea played a larger role in it than has thus far been assumed. The boundary line between a civil war and other (presumably international) wars, however, re-

mains hazy. Setting aside its origins for the moment, no one, including the author, denies that there was massive external intervention in the war. The number of belligerents alone amounted to nineteen nations. When the countries which provided various types of assistance are added, the magnitude of international intervention in the war becomes staggering. Besides, the *principal* belligerents included two major non-Korean powers — the United States and the People's Republic of China.

Simmons underlines the fact that North Koreans themselves referred to the conflict as "the civil war" (pp. 128-129). The text of the article in Nodong sinmun (North Korean party newspaper) which he cites at length, however, is two steps removed from the original version: it is an English translation (by the Current Digest of the Soviet Press) of a Russian translation (by Pravda) of the Korean-language article. Since the Korean language makes no rigorous distinction between civil and other types of wars (the generic term, chonjaeng [war] is used in most cases), one may question the utility of Kremlinological exercise here. In fact, North Koreans have used two expressions most frequently in reference to the war - tongjok sangiaeng ui naeran [fractricidal rebellion] and choguk haebang chonjaeng [fatherland liberation war]. According to P'yongyang, South Korea started the former, and North Korea responded with the latter.

More basic to the author's emphasis on the civil nature of the war is his hypothesis that the timing of the war was determined by North Korea. Among other things, he notes that "weapons for the invasion were still coming through the Soviet 'pipeline' in June, which would indicate an invasion plan for sometime later than June 25, when all of the military preparations would have been completed" (p. 120). First of all, it is doubtful that anyone can authoritatively differentiate "weapons for invasion" from weapons intended for use in the postinvasion phase of the war. Second, is one to assume that an invasion begins only after the inflow of weapons from their principal supplier ceases? Is it not a continuing process?

As "a 'harder' indication that June 25 was not the prearranged date," Simmons cites a statement allegedly made "at a conference of the communist Korean League in Japan" in April, 1950, that "[o]ur operations are scheduled, until further notice, for August" (p. 118). It simply strains one's imagination to believe that the North Korean leadership would either engage in or tolerate the public revelation of its invasion plans.

Since relations among states involve a clash

of, or, at least, interaction between, perceived self-interests of the states concerned, they usually entail some measure of rivalry, tensions, and strains. That the relations among the three Communist states on the eve of, during, and after the Korean War were consistent with this general pattern therefore does not come as a surprise. It is when the author tries to underscore the magnitude of such strains that problems arise. In order to demonstrate "China's disappointment with the Soviet Union's U.N. stance" (p. 138), for example, Simmons cites various Chinese statements praising the Soviet delegates' return to the Security Council after a costly boycott (pp. 128–140).

He then writes: "The Chinese media made almost every comment on the situation but the direct one: 'Why wasn't the Soviet Union in the Security Council during June and July blocking the successful American pursuit of legitimacy' (p. 140)? In short, he would have us believe that what passed as a commendation was really a rebuke - a rebuke which was so carefully veiled that only the hindsight of subsequent Sino-Soviet relations helped to uncover it. Sino-Soviet relations have since deteriorated to the point where Peking no longer shrinks from using explicitly denunciatory language vis-a-vis Moscow, and vice versa. One wonders why Peking has not articulated its alleged displeasure over Moscow's U.N. stance. In November, 1975, Peking even distorted Soviet Ambassador Malik's U.N. speech on the Korean question in order to accuse Moscow of "hypocrisy" - of allegedly siding with South Korea while pretending to support North Korea (see Peking Review, November 7, 1975, p. 23 and compare it with the text of Malik's speech in U.N. Document, A/C.1/PV.2061, 21 October 1975, 53-55). If Peking goes so far as to distort the truth, why doesn't it set the record straight with respect to its perception of Moscow's role in the U.N. in 1950?

One of the symptoms of the strains in P'yongyang-Moscow relations which Simmons cites is a passage in Kim Il-song's message to the Soviet Union in August, 1952 that "our people proudly call Comrade Stalin 'our father and savior." This Simmons construes as "an indirect rebuke to the Soviet Union," because, according to a Pravda dispatch, "[i]n Korea they have a saying that a 'good friend is better than a relative'" (p. 214). First of all, it is hard to believe that the North Korean leader, in the midst of a war that was being fought on his side with predominantly Soviet weapons, would be so rash and foolish as to include a rebuke, no matter how indirect, in a message expressing his gratitude for Soviet help in the liberation of Korea. Secondly, and more importantly, regardless of the authenticity of the alleged Korean saying, "father" is not a disrespectful term in Korea. In fact, Kim Il-song himself, the object of an unequalled personality cult, is referred to as oboi suryong [father(ly) supreme leader].

These are some of the numerous instances of doubtful evidence and questionable inference one encounters in the book. Before closing this review, however, I am compelled to mention another seemingly minor but potentially serious problem. To my considerable surprise, I found striking similarities between several passages in the book and those in my own work:

This third group had the unique advantage of being both politically organized and militarily trained (p. 23).

"This group had the unique advantage of being both politically organized and militarily trained..." (B. C. Koh, The Foreign Policy of North Korea [New York: Praeger, 1969], p. 3).

A fourth group vying for power in postwar North Korea was the "domestic" Communist faction (p. 23).

"Still another group vying for power in post-Liberation North Korea was the domestic Communist faction" (Koh, p. 4).

The Yenan faction had meanwhile organized itself into a separate New People's Party on March 30, 1946. The latter attracted a wide following among the petitiourgeoisie and intellectuals; its growing popularity was viewed with alarm and jealousy by the Soviet-Korean and Kapsan-sponsored Korean Communist Party... (p. 26).

"The Yenan faction ... organized itself into the New People's Party ... on March 30, 1946. The party attracted mainly petty bourgeois citizens and intellectuals. Its wide popularity ... was viewed with a keen sense of alarm and jealousy by the North Korean Workers' Party..." (Koh, p. 9).

The first two passages in the Simmons book are not accompanied by any footnotes, but the third passage is attributed to a Korean source (Kim Ch'ang-sun, Pukhan sibonyon-sa, pp. 97-99). it so happens that this is precisely the source I cited for my own passage. However, since I did not quote directly from Kim Ch'ang-sun, it is unlikely that reference to the same source accounts for the similarity. More important, whereas Simmons implies in an earlier footnote (note 5, p. 35) that he is using the English translation of the Kim book, the page numbers given above belong to the Korean version, which I used.

It should be noted that none of the passages in question contains any novel ideas; they simply describe aspects of North Korean politics in the pre-Korean War stage which are well known to students of North Korea. What is being questioned here, then, is the apparent lack of integrity on the part of the author. Together with the weaknesses in evidence and reasoning illustrated above, this serves seriously to undercut the author's credibility.

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Unequal Partners: Philippine and Thai Relations with the United States 1965-75. By W. Scott Thompson. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 183. \$13.50.)

It is probably fair to say, as the author does at one point, that between unequal partners "the greater the disparity in size and resources, the less overt pressure the greater power will have to exert. The smaller power is only too aware of how much the greater power can do for him and conversely, of how much can be withdrawn if he does not go along."

Both the Philippines and Thailand were closely bound to the United States at the end of World War II, each with different historical antecedents. Their separate (but often similar) perceptions of survival needs, together with internal quests for power and riches, constituted one side of the ledger. America's newfound world role — Asian department — and an abundance of resources formed the other, and unions of sorts were forged. Professor Thompson provides abundant information on the varied phases of the relationships among these nations.

Dependency for a small power may be necessary at times, but it can be exceedingly hazardous, and small powers should eschew it whenever they are able. But the small power, as Thompson shows, is not defenseless or wholly devoid of bargaining power. It can help or hinder the large state in various ways, among them by allowing or withholding the use of strategic sites. Such locations, in the cases of the Philippines and Thailand, have been significant, but it is abundantly clear now that the long-range value to the United States of strategic locations in Thailand is far less important than that of strategic locations in the Philippines. Thailand (especially because of location) has had to relearn how to bend like the bamboo, and indeed both have been making their peace with erstwhile enemies.

Professor Thompson poses early on the interesting question of whether the adaptive mechanisms of a small state are better managed

by democratic or by autocratic systems. He partly answers this by stating that "the circumstances determine which is more pertinent." In a sense, this view is sustained by his evidence which indicates that under some circumstances authoritarian regimes have the advantage, while under others the reverse obtains.

This book is disquieting, not because it postulates something terribly new, but rather because it documents some uncomfortable notions that have been around for some time. Does the largesse of democratic America flow more easily to democratic polities? Apparently not. Indeed, the scuttling by Marcos of the Philippines' long and rather distinguished democratic tradition seems to have resulted in closer ties, while Thailand's new experiment with democracy appeared to cool the American-Thai association. Thompson suggests that American confidence is attracted by stability, not democracy. But he also notes that although the small sovereignty in a buyers' market seems best served by internal centralized authority, in cases where the larger nation is pressuring the smaller nation, democratic institutions are observed to strengthen the latter's hand.

The primary issue, however, is not democracy as such but rather stability and order. The reaction of both American business and government to Philippine martial law seemed almost euphoric, but in all fairness, conditions of urban order, security, and internal politics in the country seemed to be at a low ebb. Thompson wrote that while democratic institutions in the Philippines were annihilated (by Marcos), "genuine social and economic reforms began to be undertaken which left open the possibility of a fuller and wider democracy in the future." But as time goes on, that hope seems less viable.

This volume suggests another line of reflection. In a world often characterized as interdependent, one nevertheless wonders whether interdependence is always the ultimate reality, or whether it is often a mere shortsighted excuse for following a line of least resistance. When a small nation becomes dependent upon a great power, it may well be on the way toward compromising its most transcendent interests in the guise of protecting them. This is not to argue that great states particularly like to cast their lesser allies aside with impunity, but as Thompson observes, that they simply do so when their vital interests are at stake. Perhaps this is as it should be, for is not enlightened self-interest supposed to be the rational rule of national behavior in the international arena? If so, then the enlightened self-interest of the small polity should also be its rule. Clearly

self-reliance remains more than just a minor virtue.

It may be, as Thompson seems to suggest, that internal concern with democratic values must be divorced from parallel concerns with the internal affairs of other states, a version no doubt of the "democracy is not for export" thesis. Trade, economic ties, and bakshish may be among the ties that bind, but it remains to be suggested that in a great state's dealings with others, not only its material interests are at stake, but clearly its reputation is as well. In such settings if the United States compromises democracy, as it does when it clearly favors authoritarian regimes, it cannot escape diminishing its own integrity thereby.

Professor Thompson's book is complex and consequently it does not always flow easily in the reading, but it is intensely interesting. His detailing of the course of conflict and cooperation between the United States and Thailand and the Philippines is full of information and insight. On the whole it is well written and balanced, and constitutes a valuable contribution.

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Intercountry Income Distribution and Transnational Enterprises. By Constantine V. Vaitsos. (New York: Oxford University Press, 1974. Pp. 198. \$16.00.)

Despite the recent resurgence of interest in political economy, political scientists and economists still have difficulty communicating with one another. This lucid and admirably organized book was written by an economist for, one suspects, other economists. Nevertheless it contains a great deal of information that should be of interest to political scientists. Furthermore most of the technical economic calculations have been relegated to the appendices and anyone with a basic understanding of economics should have little difficulty following the main argument.

Unlike so many of the writers on the still highly controversial subject of multinational corporations, the author eschews dogma and polemic. He apparently has no ox to gore, saying that investment of multinational firms can be "mutually beneficial" (p. 119) to both the firm and the host country. But the inescapable conclusion, based on an analysis of the effects of foreign investment in Colombia and several other South American countries, is that in practice most of the benefits have accrued to the multinational firms. With great

lucidity the author discusses the variety of techniques and practices multinational firms have employed to ensure the maximum profitability of their subsidiaries in foreign lands. Among the most important are the use of tie-in arrangements; the underdeclaring of profits; the resort to tax havens; the manipulation of royalties, patents, and interest payments on interaffiliate loans; and the utilization of transfer pricing on interaffiliate sale of goods. The author makes it clear that in resorting to these devices, most multinational firms are doing nothing illegal. Indeed, they are for the most part operating within the terms of the contracts negotiated with their host countries.

Undoubtedly the part of the book that will most interest political scientists is that on the bargaining process. Particularly worthwhile is the analysis of the Comité de Regalías, a committee established by the Colombian government in 1967 to negotiate and approve all contracts of technology transfer. Despite "significant achievements" (p. 130) the problems faced by the committee prevented it from being totally successful in deriving maximum benefit from foreign investment for the host country. Among these problems were lack of information on alternatives, lack of expertise to match that at the command of the multinationals, and susceptibility to corruption. In perhaps the most valuable section of this book, Dr. Vaitsos advances suggestions for improving the bargaining power of less developed countries.

These suggestions are most welcome at the present time. Developing nations, at recent international meetings such as the UNIDO conference in Lima and the Seventh Special Session of the United Nations General Assembly, have vigorously emphasized their determination to increase the extent and rate of their industrialization. But in order to achieve the anticipated benefits of industrialization, a new bargaining framework is necessary. The work of the United Nations in developing codes of acceptable behavior for multinational firms and in encouraging the elimination of restrictive business practices is a hopeful sign. But in the final analysis each country must look after itself. This book should be invaluable to those national officials charged with the responsibility of negotiating with multinational corporations and to political scientists interested in analyzing the negotiating process.

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Nonintervention and International Order. By R. J. Vincent. (Princeton, N.J.: Princeton University Press, 1974. Pp. viii, 457. \$13.50.)

This is an intelligent, well-written book designed to state a case for upholding the principle of nonintervention. Part One contrives an "approximate" definition of intervention as "...that activity undertaken by a state, a group within a state, a group of states or an international organization which interferes coercively in the domestic affairs of another state ... it is not necessarily lawful or unlawful..." (p. 13). It is not particularly helpful as a definition except perhaps to indicate the area of problems chosen by Professor Vincent for study.

Part Two outlines the history of nonintervention, in three chapters dealing successively with principle, theory and practice. The pre-1945 material is, of course, interesting and its treatment is valid in so far as the author regards the principle of nonintervention as a principle which long antedated the U.N. Charter and survives it. There is, however, an inevitable difficulty in transposing practice, or conclusions drawn from practice, from one era to another when the rules of law have undergone radical transformation from one era to another.

Part Three is a study of the principle in contemporary world politics. It contains a very good survey of Soviet practice, including episodes such as Hungary and Czechoslovakia, and explains Soviet adherence to the principle of nonintervention on the hypothesis that Soviet theory admits only of intervention by capitalist states. This is probably correct. Certainly I have never found a persuasive account of the problem which reconciles Soviet practice with any general principle of nonintervention, although the theory is patently absurd.

A separate chapter on U.S. practice begins to expose some of the author's weaknesses. Professor Vincent seems to be a political scientist rather than a lawyer, for although he tackles the law with courage backed by wide reading, he is least perceptive here. For example, he sees the crucial legal point over Vietnam as being whether the conflict was an international conflict or a civil war (p. 224). In fact, granting the U.S. view that it was an international conflict, there remained the most crucial question: "What right does the U.S. have to intervene in such an international conflict?" In essence, the problem therefore becomes one of defining what right is conferred by the right of collective self-defense: the point escapes the author. Admittedly, he has from the outset decided to exclude all but intervention in domestic affairs: but this raises a doubt about his judgment in so doing, for the most difficult and potentially destructive (to world peace) interventions are those in an international conflict. To take a further example, there is the transition from a discussion of intervention by states to a discussion of intervention by the United Nations in Chapter 8, without any real attempt to inquire whether the rules governing intervention by states and by the U.N. are, or even should be, the same. Nor does one find any real discussion of the vexed question whether regional groups of states have, as such, rights of intervention denied by law to states acting alone. Even the highly problematical "exception" to the pro-hibition of intervention claimed by the Afro-Asian states in the guise of a freedom to act to promote self-determination, or eradicate colonialism, is scarcely mentioned.

Perhaps the mistake is to expect too much of one book, or one author. Some of the chapters are highly informative and rewarding, especially to lawyers, perhaps because the author brings to them skills which most lawyers lack. For example, the sketch of the debates in the U.N. leading up to the 1965 and 1970 Declarations is highly perceptive and sensitive to policy implications in a way which few lawyers could emulate. The difficulty, by no means uncommon, is that the author who writes in the field of international relations will rarely satisfy all the specialists whose areas of study come under his scrutiny.

With the author's overall conclusion I would certainly have no quarrel. He sees the principle of nonintervention as a "neutral norm of abstention" (p. 386) which in the present system of independent and often antagonistic states is the essential prerequisite to the maintenance of peace and order. This seems very true. If one examines the many cases of intervention, by both socialist and capitalist states, by East and West, the overriding conclusion is that they have brought little profit to anyone and usually posed very considerable threats to world peace.

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International Law and Psychology; Two Studies: The Intrusion of Order and Conscience and Society. By Ranyard West. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1974. Pp. 418. \$17.50.)

This volume contains two studies, Conscience and Society (1942) and The Intrusion

of Order (1973). Both studies analyze international politics and legal theory from the interdisciplinary perspectives of psychology, political philosophy, and jurisprudence.

The earlier study develops the political and social theories of Hobbes, Rousseau, and Locke as functions of their social and political times and their personal psychological and social situations. A further elaboration of political and social theory follows, with summaries of Hegel's, Bosanquet's, Hobhouse's and others' views of law, the state, and the relationship between the individual and the state. Dr. West, holder of both the M.D. and D.Phil. degrees and a trained analyst, argues that to construct a relevant theory of government one must account for the psychological component of human behavior. Since each theory of government contains assumptions about human motivation, the task is to identify what human nature is about and then choose the appropriate theory.

The author contends that since the purpose of law is to control innate human aggressiveness, law without sanctions, either internal or external to the individual, is impossible. From here it is not hard to see where the argument flows about international law. Without the external sanction to compel sovereigns to obey, international law is a flop. The internal sanction which often restricts human aggressiveness is unavailable in international relations since the blinders of nationalism, prejudice, and ideological self-justifications arouse popular support for action calculated for short-run national interest. The proposed solution is to move from the nation-state as the fundamental unit of international politics and substitute a world state or global confederation of states. The argument should be familiar to students of international politics and international law.

The later study, The Intrusion of Order (1973), continues themes developed thirty years earlier. In a short introduction to Freudian social theory, Dr. West identifies power and prejudice as the governing processes of international politics. Extending the psychoanalyst's technique to recent events, the author presents four episodes to complete the argument: the Abyssinian crisis of 1935; appeasement in Europe, 1937–39; Soviet-American relations, 1945–50; and Korea, 1950.

These cases identify alternative policies which, though not followed by the political leaders of the time, might have altered events. Had Great Britain followed the results of the Peace Ballot, Dr. West argues, war might not have been avoided, but it would have occurred for a "good cause." Had Neville Chamberlain

followed a collective policy to include both France and the Soviet Union to confront Germany, or, had he taken the lead in defusing Hitler's appeal to the German people by offering colonial redress and treaty revision, war might have been averted. In both cases the political leadership reverted to nationalist thinking and anti-Soviet prejudices, making the outcome probable — war with Germany.

In discussing the postwar era, Dr. West turns to the United States and its attachment to nationalist thinking and anti-Communist prejudices. Here too he outlines the theme of missed opportunities: an aggressive anti-Communist policy, a Communist reaction which triggered further anti-Communist policy, a Communist reaction which triggered further anti-Communist action, and the descent into a thirtyyear old cold war with its off-again, on-again crisis antagonisms bringing the world to the brink of "hot" war. Dr. West concludes that if the nation-state continues to represent the power interests of human communities the chances for calamity will increase. "This must continue until and unless the institutions of cooperative law and order are set up upon a world-wide scale" (p. 92).

What then is the remedy Dr. West prescribes to deal with this dangerous and potentially fatal affliction? Following the analogy of how individuals reorient their perspectives when confronted with insecurity or resistance to unpleasant facts, Dr. West asks us to "...reexamine the Russian and the Chinese Communists, their behavior, their nature, their doctrines and your own response to their presence in your world during the past half century of doubts and misunderstandings!" (p. 119). In Moscow and Peking a similar reinterpretation should occur with respect to United States doctrine and action. To conduct this all-around examination "massive ... factual knowledge" and "enlightenment as a flash or as a glimmering that increases" will help us identify a "common humanity we share with our 'enemies' . . . " (p. 120).

Dr. West assumes throughout that the rethinking will lead to the more ordered peaceful relations which could have arisen had alternative policies been followed. Just the opposite may occur: increased examination of action and motive may lead to a more nationalist policy than the present admits. Also, politicians' capacity to rethink old beliefs (and therefore minimize the impact of resistance to new ideas) is most likely to reveal itself when there is some political advantage to be gained. Recent United States policy toward the People's Republic of China indicates that poli-

ticians can restructure their most deeply rooted beliefs when the international and domestic environment will provide the incentives for change. Dr. West implicitly recognizes this mechanism, but unfortunately minimizes its operation.

In Conscience and Society Dr. West argues a psychological theory of law and social order which includes an all-too-brief examination of contract. Since law and promise-keeping are fundamental elements in social order within society, the author argues, the absence of kept promises, or the belief that promises will not be kept, prevents a stable international society. Recognizing this as a doubtful premise, nonetheless can we suppose or hope that promises now made within the international arena have a greater chance to be kept as the interconnectedness among states increases? The four episodes in The Intrusion of Order are national security or diplomatic issues, but will energy, development, ecology, and trade issues effectively alter promise-keeping among states and encourage reliance on international institutions? Maybe

International Law and Psychology most usefully presents the traditional problems of human motivation, political organization and world (dis)order. For political scientists concerned about international relations and law most of Conscience and Society will be restatement. When the section on the "Psychological Theory of Law" is read as a prelude to The Intrusion of Order however, speculative possibilities arise. As an empirical examination of cases leading to general conclusions about the inadequacy of the current era, Dr. West's volume resembles the psychologist's daily appointment record: who would condemn him if he sees the world motivated solely by prejudice, nationalism, and recurrent crises if he examines only Abyssinia, appeasement, the roots of the cold war, or Korea? Dr. West holds up a psychological mirror to our era - what he cannot do is give us an insight into the instrumental forces of change. For that we rely on circumstance, politics, and social science research (?).

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The Politics of Foreign Aid. By John White. (New York: St. Martin's Press, 1974. Pp. 316. \$13.95.)

The study of political aspects of foreign aid has generally focused on the policies of a particular donor country or on case studies of particular donor-recipient relations on a specified project or over a defined time period. The field of general analysis of foreign aid has usually been left to the economist or to the quasi-scholarly study aimed at predetermined policy ends such as the Pearson report. This study by John White is an important and largely successful attempt to pull together the fruits of more particularistic research and to begin a systematic analysis of the role of foreign aid in domestic and international politics.

White, who is on the staff of the Institute of Development Studies, University of Sussex, consciously seizes a middle ground between what he calls the cynical ideologue who sees aid as nothing more or less than an attempt by the donor to generate and perpetuate the economic and political dependence of the recipient and the credulous moralist who sees aid as the altruistic acceptance by the rich donor of an obligation to help the less fortunate poor recipient. White claims the role of a skeptic who avoids automatic acceptance or rejection of stated explanations of aid policy but examines each on its merits.

The only general criticism of aid policy he finds meritorious is that sometimes the wrong kind of aid has been given because of foolishness rather than malevolence and sometimes it has done more harm than good. He remains a believer in aid, primarily on moral grounds, but it is difficult to see why. His chronicle of aid activity and his survey of the literature suggest that the wrong kind of aid has been given more often than not and that there seems to be no little malevolence in some aid decisions. Moreover and more importantly, the political impediments to administrative and policy reform are great, and, in spite of his avowed faith in aid, White is not sanguine that the requisite political will is available to make the kinds of changes his study so clearly recommends.

Two major contributions of this study are rare in the literature. First, White's approach is genuinely comparative. He is interested in the motives and administrative practices which characterize various national and multinational aid programs and analyzes not only the major donors but the smaller newcomers as well. He also uses a comparative framework in analyzing the different uses to which aid has been put in different settings and the different motives for and costs of seeking and receiving aid.

White rightly points out that assessing motives for aid-giving and -receiving is a very slippery and hazardous undertaking because the many agencies and political institutions involved bring divergent motives to their aid

decisions. He concludes that most aid programs originate in political crises which are almost historically accidental. Thereafter, aid programs simply continue with incremental changes because, short of another political crisis in the relations of donor and recipient, both lack the political interest or the political will to make major changes.

The second major contribution, often neglected in the literature, is to break down the monolithic image of the state, both donor and recipient. It is not uncommon to analyze the influence of bureaucratic bargaining and infighting, of political competition and public opinion on the aid policies of donor states. White, however, also deals with the effect of aid policies on bureaucratic power and of bureaucratic and political considerations on aid utilization in recipient states. While generally ignoring the much better documented impact of aid on the power and prominence of the military in developing societies, he emphasizes the impact of aid on nonmilitary segments of recipient societies and bureaucracies, most notably planning commissions and finance ministries, and the struggles within recipient governments for aid as a resource in internecine battles.

White also provides a useful description and analysis of foreign aid theories and the evolution of aid, as well as an assessment of the costs and benefits - both economic and political of aid which is especially insightful on the subject of technical assistance.

Though White's definition of aid as "any transfer of resources from rich countries to poor countries which the former choose to call 'aid'" (p. 23), clearly includes both military and developmental assistance, he elects to focus almost exclusively on developmental aspects. This is unfortunate for, purely military aid apart, it is exceedingly difficult, especially in the case of the major donors, to disentangle developmental and security facets of aid programs. The developmental focus directs our attention to one type of aid impact but it largely ignores another whole range of impacts which are at least of equal importance.

This is an important book. It provides a systematic and perceptive inventory of what we know and, equally important, what we do not know about the purposes and impacts which characterize the foreign aid game. It is a major contribution to an overall understanding of the role of foreign aid in development strategies and in international and domestic politics.

M. GLEN JOHNSON

Technology in Comecon: Acceleration of Technological Progress Through Economic Planning and the Market. By J. Wilczynski. (New York: Praeger Publishers, 1975. Pp. xvii, 379. \$22.50.)

The author, an Associate Professor of Economics at the Royal Military College in Australia, presents in this book a description of the progress of technological innovation in the European Comecon countries. It will be a disappointment to anyone who expects a systematic examination of the subtitle's three variables and of their interrelationship. Nevertheless, political scientists may find it useful for the information it contains, if not for the analysis.

Following two introductory chapters on today's technological revolution and on manpower and investment in Comecon, both of which contain familiar material, the author deals in turn with various sectors of the civilian economies in Eastern Europe and the USSR. He begins with energy, in which we see as elsewhere that Comecon has made great strides in production in the 1950-1970 period, but at the same time that its levels of technology are "well below" those of the West. It is also characteristic of Comecon that its leading member, the Soviet Union, is "ahead of the West" in certain advanced forms of energy technology, but not yet uniformly so.

The chapters on computerization and quality in production seem of greater than average interest. The discussion of obstacles to computerization is particularly good. Also of interest is the author's statement that even the degree of mechanization, let alone automation, is low in Comecon. This suggests that a proper account of technology in the European Communist states should begin with mechanization rather than the more sophisticated elements of the "scientific and technical revolution," as Wilczynski has done.

The chapter on technical cooperation would have had more interest if certain questions had been broached. We do learn that equalization of economic levels of member countries is a Comecon policy. But what policy, one wonders, does the organization have on technology; what are its goals? What politics are involved in securing cooperation on R and D or on specialization? There are indications, Wilczynski says, of the primitivization of the Czechoslovak, East German, and Hungarian economies in these cooperative arrangements. If this is true, is it because the flow of technology is generally undirectional? Unfortunately, the answer cannot be found here because no general theory of the diffusion of technology is utilized as a framework. Indeed, there appears to be no framework at all; even the author's concept of technology is not defined.

Only in the penultimate chapter do we learn of the priority assigned to the military sector in Comecon. In fact, this is a self-sufficient, high-technology sector; the civilian one is residual, enjoys little technological "spinoff" from the military, and acquires technology unevenly, erratically. In view of this, it is curious that Wilczynski devotes so little space to the military economy, and so much to the civilian.

In the final chapter the author at last turns his attention to "theoretical" questions. Actually, however, these comprise neither theory in the social scientific sense nor philosophy. They are ideological. And instead of explanation we are given speculation. Once again it is "socialism" versus "capitalism." "Which system" the author asks rhetorically (p. 361), "has a greater capacity for technological progress?" How much "capitalist" technology can the Comecon countries absorb before they turn completely away from "communism"? A tempting response is, who cares? More seriously, the problem is that these considerations, even if they were theoretically refined (which they are not) are introduced far too late for the author to answer them on the basis of the foregoing

Despite its theoretical shortcomings, this is a rather stimulating book because it suggests implicitly but strongly the need for a policy study of technological progress in the Soviet Union and Eastern Europe, a challenge that should be undertaken by some political scientists in the near future. As it stands, it should help area specialists update their knowledge of the Comecon economies, and would be of value for students in interdisciplinary courses. It contains numerous tables, several diagrams ranking the Comecon countries against the rest of the world, and an index. Despite its recent publication, however, most of the data do not go beyond 1970.

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Case Studies on Human Rights and Fundamental Freedoms; A World Survey. 2 vols. published for the Foundation for the Study of Plural Societies. Edited by Willem A. Veenhoven and Winifred Crum Ewing. (The Hague: Martinus Nijhoff, 1975. Pp. 1137. Price unknown.)

This is a reference work containing some thirty-six separate essays and studies dealing with the enjoyment of fundamental rights and freedoms on the part of ethnic groups, though some of the authors also treat the extent of such rights of all kinds of groups and others deal with subcultures which are not exactly ethnic. The following countries and regions are covered in the survey: Soviet Union, China, United States, West Germany, Spain, Peru, Middle East, Pakistan, Africa, Japan, Eastern Europe, Australia, Britain, Belgium, Israel, India, Sri Lanka, Fiji, West Irian, South Africa, Mexico. In addition some of the essays deal with groups and issues which are not discussable in terms of a single country or region, e.g., essays on anti-Semitism, the plight of Amerindians in Latin America, the Kurds, the Kabyls, women in Islamic societies, and the evils of communism in general.

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Plato's Republic. Translated by G. M. A. Grube. (Indianapolis, Ind.: Hackett Publishing Co., 1974. Pp. vii, 263. \$10.00, cloth; \$1.50, paper.)

This new translation recommends itself to teachers and students because of its price: the paperback version will be the cheapest available, and the publishers are to be commended for their economy. Moreover, the translation is quite literal. It almost goes without saying that it is superior to the Cornford translation. Anyone who knew Greek, who was concerned with faithfully conveying what the Greek meant, and who was not under the delusion that his intellectual superiority allowed him to edit out large portions of Plato's original would produce something better than Cornford. The continued widespread use in schools of the Cornford edition is an unhappy testimony to the ignorance or carelessness of those who assign it. But to recommend a translation because it is preferable to Cornford is faint praise, and Grube deserves more than this. His effort is more careful and accurate than (in ascending order of merit) Jowett, Spens, Chambry's French, Lee, Lindsay, and Shorey; in point of clarity if not precision Grube also improves on Davis. The translation is inferior, however, both in clarity and accuracy, to that of Bloom.

For the most part Grube reproduces what Plato wrote, and in down-to-earth English. Still, Grube is not always painstaking and hence cannot be relied upon in details. This is because he does not believe the *Republic* merits philosophical attention to its details. As he says in

the Preface, the Republic "is not an esoteric treatise for students of philosophy, it is addressed to the ordinarily educated man." Grube has arrived at this remarkable judgment on the basis of the fact that in his own reading he has found the "difficult and highly controversial passages" to be "few." To this I am tempted to respond that Grube's reading has been "blessed" — in the Socratic usage of the term. At any rate, since Grube has not himself experienced a sense of perplexity and bafflement over very much of the Republic, he is not inclined to conceive of his work as aimed at helping readers who have had such difficulties. Above all, he does not strive to render important Greek words consistently.

I will confine myself to stating what I hope will be helpful cautions pertaining to Grube's translation of some key terms. First as regards "justice" and "injustive," the theme of the work. The Greek word and its relatives are not always translated into words related to the English "justice" or "right": in some crucial passages (e.g. 330e-331a), "injustice" is rendered as "sin" or "sinning," without even a footnote warning against religious - not to say Christian - overtones. What Paul Shorey remarked of the word "faith" ("Always avoid in translating Plato" - note to Loeb ed., 534) seems to apply even more to "sin." Second, the word politeia. This is the title (traditionally written "Republic" from the Latin) and is one of the basic theoretical terms in Plato's political theory altogether. It is among the most important words in the dialogue, and the reader ought to be able to follow its usage, observing how its meaning unfolds and develops as the conversation proceeds. He cannot do so with the present translation. Instead of always trying to translate politeia in the same way or at least noting each time it appears, Grube translates it variously as "community" (397d), "city" (424a), "administration" (427a), "form of government" (455c-d), and "constitution" (497a). Only the last two are really accurate, and the reader should be alerted to the danger

in importing Montesquieu's familiar terminology ("forms of government") into Plato. Some additional brief warnings: "dictator" is not a harsh enough equivalent for the Greek tyrannos ("tyrant" is preferable); the "noble lie" is a lie, not a "noble fiction" ("fictions" are not necessarily meant to deceive; the noble lie is a deliberate deception — contrast p. 82 with p. 48n.); the diagram Grube uses to illustrate the famous "Divided Line" is wrong — Grube has failed to discern that the carefully stated ratios and the theorems of Euclidean geometry require the two central line segments to be equal (p. 164n.).

One is also forced to note a certain lack of scholarly professionalism in this edition. There are not sufficient footnotes to explain many important allusions. The book lacks an index. The bibliography is out of date and makes no reference to the very important scholarly advances that have been made in the past ten years in our understanding of Plato's political philosophy. Most important, Grube never indicates what edition of the Greek text he is using. The only edition in the bibliography is Adam, but it is quite clear that Grube usually follows Burnet (although not at 533e, 568d, or 574d). In particular, Grube seems to accept from Burnet (against Adam) the authority of the manuscript Vindobonesis F. This is intelligible, but some note of the controversy seems fitting, especially when there are important points where this choice - as well as other differences with Adam - transforms the meaning (e.g. 362d, 365e, 515b, 585c). Indeed Grube never lets the reader know about manuscript difficulties or possible alternate readings that would seriously affect the content of what is said (see especially 533e, 574d).

All in all, this is a competent and helpful translation, useful for the classroom but not quite accurate enough for the serious student intent on understanding Plato's political philosophy.

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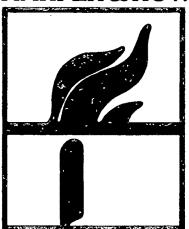
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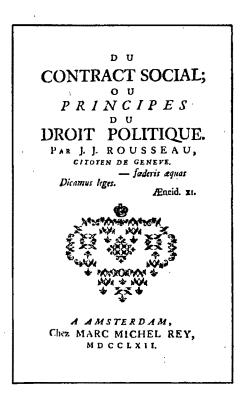
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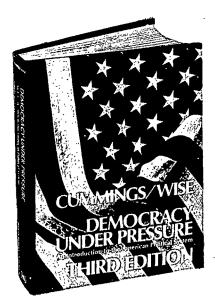
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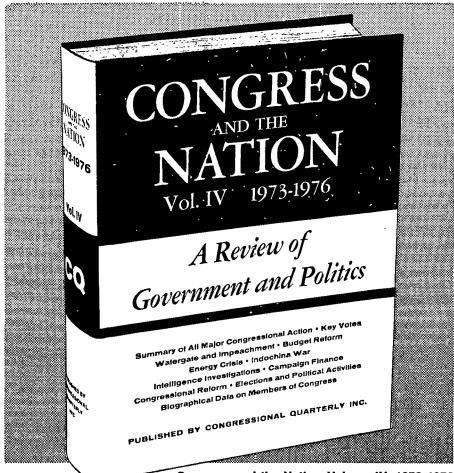
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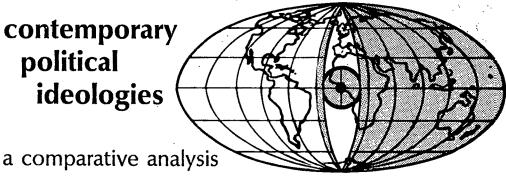
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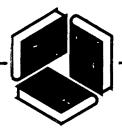
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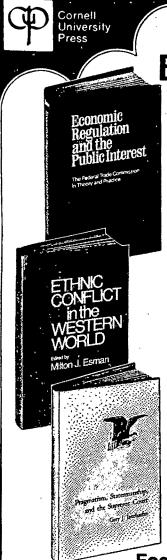
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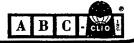
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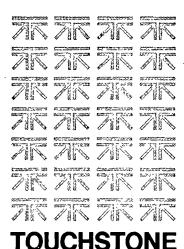
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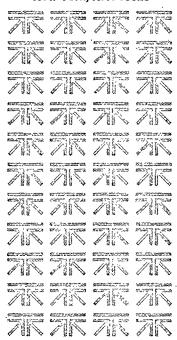
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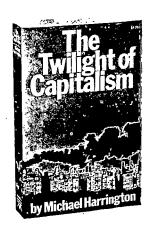
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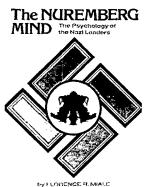
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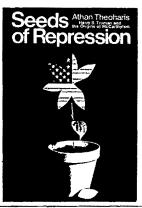
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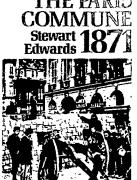
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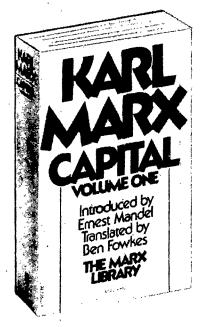








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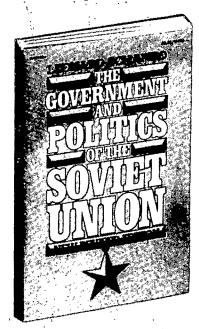


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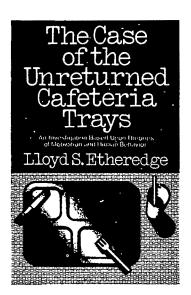
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The Costs of Major Wars: The Phoenix Factor*

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A major unexplored area in the field of international politics is the consequences of major wars for members of the international system in terms of power lost or gained. This paper explores these shifts of power among neutrals, winners, and losers as a result of these wars, using a sample of 32 cases and time series analysis. The findings register unexpected but systematic patterns after major conflicts; while winners and neutrals are affected marginally by the conflict, losers' powers are at first eroded. Over the long run (15–20 years), though, the effects of the loss dissipate; losers accelerate their recovery and soon resume antebellum status. It is this phenomenon that the authors call the phoenix factor.

Introduction

There is no end of books and articles about war, and yet we know little of the subject that is of use. Of the three major aspects of interest in war—its causes, its outcomes, and its consequences—our concern in this paper is with consequences. These, of course, are intimately related both to beginnings and outcomes. The relationship between studies undertaken in the past and the effort in hand is useful briefly to elucidate.

Specialists and laymen alike have traditionally assumed the existence of a tie between the outbreak of major wars and the distribution of power among the major actors in an international system. In a general way, this premise postulates which power distributions went with war and which with peace. But there has been no interest in viewing the problem from the opposite angle of vision: what effects did outcomes of the conflicts have on the power distribution of the international system?

Can an inferior power reduce a superior one by means of war? Can the superior turn back the inferior by winning such a war? In short, what is "gained" and what is "lost," in power terms, by war? The overriding answer to such questions, the one accepted in the past, is that major wars and their outcomes make a tremendous difference in the international system. The future of a belligerent nation depends on victory or defeat. Regardless of other rationales

*This paper is a portion of a much larger work on the beginnings, outcomes, consequences, and prevention of wars. We wish to acknowledge our gratitude to the Earhart Foundation for its financial support in connection with this project. advanced, this is the "logical" basis for going to war. It seems a powerful reason, and it is widely accepted. The justification of wars is rooted in such convictions.

However, we suspect that such reasoning is invalid. Hence, this investigation. A problem confronting the researcher in this area is how to ascertain the validity of the propositions that the outcomes of war either do or do not affect the power distribution in the international system. Several separable tasks are involved:

- 1. Considerable attention must be devoted to the method by which power is measured.
- 2. Attention must be accorded to the selection of conflicts to be studied.
- 3. It must be determined whose performance, in any given conflict, is to be evaluated. In other words, who are the principal actors?
- 4. One's philosophical concern must initially be translated into theoretical propositions and then into operational hypotheses.
- 5. One should test and report one's findings. These five points represent the table of contents of this paper.

The Measurement of Power Resources

Power has long been considered to be the capacity of an individual, group, or nation to control the behavior of others in accordance with its own ends.¹ It is an element of every

¹A. F. K. Organski, World Politics, 2nd ed. (New York: Knopf, 1968), p. 104; Karl Deutsch, The Analysis of International Relations (Englewood Cliffs, N.J.: Prentice-Hall, 1968), p. 70; Hans J. Morgenthau, Politics Among Nations, 4th ed. (New York: Knopf, 1966), p. 26.

relationship, when each party has at its disposal resources, tangible or intangible, likely to alter the conduct of the other. Power becomes apparent only when a disagreement arises between the parties, in which event, the will of the more powerful usually prevails. The measurement of power, therefore, is vital to the prediction and explanation of joint behavior.

There have been attempts to study the actual degree of control which one nation has exercised over another. For the most part, however, specialists in international politics have retreated to measuring the resources that generate power.² This procedure presents problems. Power resources do not necessarily reflect the exercise of their potentialities, nor do reliable estimates include certain essential components (e.g., diplomatic skills, charismatic leadership, and internationally appealing belief systems) not susceptible to easy or dependable measurement. Moreover, hard estimates do not indicate the power a particular nation may ostensibly possess simply because other nations may mistakenly assume that it is either more or less powerful than is actually the case. The semblance of power often passes for its reality.3

This problem may be set aside for this study. A discrepancy between the possession of real power and the external perception of it is more likely to occur in time of peace than in wartime and is most likely to occur just before nations actually commit themselves to conflict. During and after wars, however, the two views tend to merge. Thus the perception and the reality of American military power were far apart until this country's gradual commitment in Vietnam when war brought the two together again. The same occurred in Korea more than a decade earlier.

Procedure for the measurement of objective power comprises two steps: (1) one must compose a list of all the indicators that can influence the exercise of national power and select the indicators considered most important; (2) one must determine a way of

²This distinction between national capabilities and national power is widely used in international politics. Throughout this paper we are concerned solely with national capabilities and power resources; however, to relieve the tedium of repeating over and over again "national capabilities" we shall use three terms—national capabilities, power resources, and national power—interchangeably.

³Ole Holsti, "The Belief System and National Images: A Case Study," Journal of Conflict Resolution, 6 (1962), pp. 245-51; cf. also K. J. Holsti, International Politics: A Framework for Analysis (Englewood Cliffs, N.J.: Prentice-Hall, 1967), Ch. 7.

aggregating these components to obtain a single measure of national capability.

The Selection of Indicators. Among scholars interested in the construction of empirical estimates of national power, there is agreement that measures of economic, political, military, and demographic capabilities suffice to give a reasonably accurate overall indication of power.4 Quantitative indicators are generally available. The economic capacity of a nation, for example, is suggested by per capita, total, or disposable output. Demographic capabilities are grossly reflected by total population or, more accurately, by the fraction in working or fighting age groups. Military preparedness may be inferred from expenditures on arms and the size of military forces. Only political capabilities are still unmeasured. The necessary data are usually not available and, if available, are not easy to interpret.

This problem of satisfactorily measuring political capability has important implications for our study and requires comment. It has been long assumed that economic development and the capacity of a political system to penetrate and mobilize the human resources of a nation go hand in hand. When a country evinced high degrees of social and economic development, it was taken for granted that it would exhibit a high estimate of the extent of mass-elite political linkages, that is, the ability of government to organize and motivate its population for national purposes and to muster national resources through political channels. This is only partially true.

Discrimination is required. High degrees of political mobilization do not necessarily accompany a comparable level of economic development. China, for instance, is highly mobilized politically, but not economically. The same statement is applicable to North Korea and North Vietnam. The outcomes of direct military confrontations between the United States and China, North Korea, and North Vietnam

⁴See, for example, Raymond Aron, *Peace and War*, Trans. Robert Howard and Annette Fox (New York: Doubleday, 1966); cf. also, William Coplin, *Introduction to International Politics* (Chicago: Markham, 1970); Organski, Ch. 6–8; J. David Singer, Stuart Bremer, and John Stuckey, "Capability Distribution, Uncertainty and Major Power War, 1820–1965," in *Peace, War and Numbers*, ed. Bruce Russett (Beverly Hills: Sage Publications, 1972), pp. 21–27; Nazli Choucri and Dennis Meadows, "International Implications of Technological Development and Population Growth: A Simulated Model of International Conflict" (Center for International Studies, MIT, mimeographed, 1971), pp. 23–24. This presentation considers technology an independent variable.

clearly indicate that if a nation is economically underdeveloped, estimates of its power which are based mainly on socioeconomic and military indicators may be totally unreliable.⁵

We include in our survey nations ruled by various forms of government, for there is no suggestion that economic development and democracy are uniquely harmonious. Often, the reverse is true. The United States and nationalsocialist Germany were, in the third and fourth decades of this century, economically developed, and both had political systems which could effectively mobilize human and material resources. Yet one nation was a democracy, the other a totalitarian dictatorship. We cannot directly measure the political resources which are a central feature of national power, and because without such data measurements of national power in underdeveloped countries are unreliable, we consider in this article only economically developed countries.

The Problem of Aggregation. The selection of critical indicators and a method of combining them to form a single measure is the first step. Until recently, no aggregation was attempted. Frequently, values of all the indicators were presented; the reader was left to bring them into some sort of focus. An intuitive or impressionistic measure could be derived if a nation scored equally high on all elements measured. However, if a country scored high in some areas and low in others, impressionistic estimates became fanciful.

Our choice of an overall yardstick of national capabilities, in preference to all others considered, is gross national product.⁶ This decision may appear unwise. Might not a single economic indicator prove inadequate? Would not a more comprehensive index, composed of a number of other indicators, perform better? Unfortunately, more complicated indices do not perform so well as does this single one, and data for them are not as reliable for the period

covered by this research.7

The utility of measurements of total output should not be surprising. Estimates of gross national product closely reflect the movement of the underlying variables crucial to the generation of national power resources: the fraction of the population of working and fighting ages and the level of productivity.8

Because total output is the result of the interaction between the size of the productive population and its level of productivity, the national equation can be expressed in the following fashion:

Power = Population
$$x \frac{GNP}{Population} = GNP$$

In this formulation, Population implies the size of the fraction of members of working age, and per capita product implies their productivity level. The interaction of components implies a weighting system. Fluctuations in productivity affect the importance of population upwards or downwards. One population twice as productive as another implies that two individual workers in the less productive economy are required to perform the labor of one in the more productive. This weighting system, while arbitrary, seems theoretically justifiable. More importantly, it reflects the realities of international politics.

Estimating Consequences of War. As critical as the problem of power aggregation is the decision about the procedure to follow in estimating outcomes of war. This can be accomplished in one of three ways. First, one can simply establish suitable points before and after a conflict and compare them, with the differences regarded as the costs of war. This

⁷We compared the performance of GNP over time with the measure developed by Singer, Bremer, and Stuckey, the only other multidimensional measure available for the same period. Both measures are highly correlated and the similarity increases as the reliability of data improves. (R² of .95 was obtained for the sample of major powers for 1870–1970.) See Jacek Kugler, "The Consequences of War" (Ph.D. Dissertation, University of Michigan, 1973), pp. 82–96.

8See Steven Rosen, "War, Power, and the Willingness to Suffer," in Peace, War and Numbers, p. 171; and Charles Hitch and Roland McKean, The Economics of Defense in the Nuclear Age, 2nd ed. (Cambridge, Mass.: Harvard University Press, 1967) Pt. 1; Norman Alcock and Alan Newcombe, "Perceptions of National Power," Journal of Conflict Resolution, 14 (1970), pp. 335-43. For opposite views see Klaus P. Heiss, Klaus Knorr, and Oskar Morgenstern, Long Term Projections of Political and Military Power (Princeton: Mathematica Inc., 1973).

⁵Attempts to measure "political development" are reviewed in Irma Adelman and Cynthia Morris, Economic Growth and Social Equity in Developing Countries (Stanford, Calif.: Stanford University Press, 1973).

⁶Alternate explicit attempts to estimate a single measure of national capabilities are: Wilhelm Fucks, Formeln Zur Macht (Stuttgart: Deutsche Verlags-Anstalt, 1965); Clifford German, "A Tentative Evaluation of World Power," Journal of Conflict Resolution, 4 (March 1960), pp. 138–44; Organski, pp. 207–14; Singer, Bremer, and Stuckey, pp. 19–49; Klaus Knorr, Military Power and Potential (Lexington, Mass.: Heath, 1970); Ray Cline, World Power Assessment (Washington, D.C.: Georgetown University, 1975).

method is often applied, but is vulnerable to major error.

A second technique consists of calculating losses from a conflict by taking into account those changes that one estimates would have occurred had no conflict taken place. This procedure controls for normal growth, but is still based on the difference between two points. The rates of change are stipulated to be the same as those before the onset of the conflict. This procedure is clearly superior to the first, but it is weak because any comparison of the last year before and the first year after a conflict, work with times that are plainly abnormal, thus rendering difficult, if not impossible, the estimation of war costs. On the other hand, choosing points more widely separated distorts even more the measurement of war's effect. Moreover, the method produces misleading results because it maximizes the effects of different growth rates across countries and usually imputes high losses to rapidly growing nations.9

A third method represents a significant improvement over the others. Three steps are involved: (1) a base period prior to the war is selected, and a trend during this period is established; (2) the trend is extrapolated and serves as a moving base for comparison with actual performance; (3) differences between real behavior and the extrapolated lines are estimated. Figure 1 should facilitate an understanding of the mechanics of the model. The procedure we suggest permits the avoidance of distortions resulting from others proposed.

Forecasting. Our efforts to extrapolate require additional comment. The attempt to estimate changes over time by extrapolating trends from the base period is obviously a crucial factor since the quality of the projections is of paramount importance. The difference between anticipated and actual behavior rests on where we place the extrapolated line. If the selected series is well behaved with only minor fluctuations from year to year, regression techniques can be used to extrapolate from the pattern established in the base period. These techniques are particularly well suited for time series analysis, provided a variety of tests are performed guarding against

⁹See Frank Notestein, Irene Taeuber, Dudley Kirk, Ansley Coale, and Louise Kiser, The Future Population of Europe and the Soviet Union; Population Projection 1940–1970 (Geneva: League of Nations, 1944), Ch. 3; also Gregory Frumkin, Population Changes in Europe Since 1939 (New York: Augustus M. Kelley, 1951).

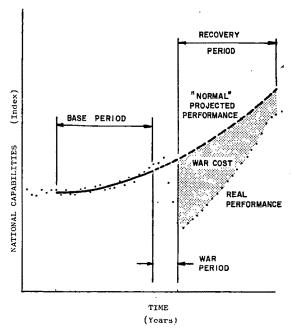


Figure 1. The Costs of Major Wars

violations of assumptions and controls are introduced reducing undesirable effects of fluctuations. However, a number of interrelated questions must be answered: (1) Of what durations should the base period and the projections be? (2) How is one to deal with the abnormalities in periods serving as bases for forecasts? (3) What models should be employed in making projections?

Let us begin with the question of the lengths of base and extrapolation periods. Where a base period is said to end and a recovery period to begin are critical moments to choose. For example, Germany began to arm and mobilize long before the declaration of hostilities in World War II. Italy quit that war long before peace was declared. Such contingencies abound in wars, and adjustments become increasingly arbitrary. To avoid bias, the starting point of conflict for all contenders was judged to be the first full year of active confrontation between two sides. The beginning of the recovery period was judged to be the first full year after belligerence came to a halt.

From these two points, the official beginning of the war and the official termination, we selected a base period of 19 years and extrapolated onward for 20. Our decision was not arbitrary. We were aware that the longer the time segment available to fit the regression line, the more reliable the prediction. On the other hand, the longer the period, the less

characteristic were the growth trends. ¹⁰ Our own selections were made by trial and error. Similar regressions were run for periods of 15, 20, and 25 years prior to the two conflicts in order to determine the stability of results. Those obtained for regressions using a period of 15 years were considerably less stable than those of 20, and regressions using a period of 25 years did not produce improved results. A period of 19 years seemed most suitable, because it provided as much separation as possible between the two world wars and a sample of years large enough to permit the use of at least 16 points in all estimates.

We extrapolated for 20 years. Clearly, a recovery period is difficult to isolate. Some effects of war linger. French resentment simmered for decades after their defeat by Germany in the Franco-Prussian War. German anger and revanchism, after their defeat by the Allies in World War I, produced World II. We are primarily interested in the period when the effects of war are immediate and dominate the behavior of the international system.

Much of the analysis that follows rests on our choice of a forecasting model. We decided that linear models would not serve; they imply that yearly additions to total output are constant. They assume, therefore, a decrease in growth rates as the base expands. Such assumptions seemed invalid; the problem of underestimation is reduced with logarithmic transformations, because constant growth rates can be estimated with an ever-extending base.

To check our choice, we tested the performance of logarithmic adjustments with data unaffected by war from 1870 to 1913. In all cases, the nonlogarithmic projections consistently underestimated the real performance of the nations considered, and outliers systematically increased in the latter part of the series. In all instances better fits resulted from data which had been logarithmically transformed.¹¹

One caveat, however, should be made: there is no assurance that the patterns of growth for the period of 1900-1970 which we used for this study are identical to those of 1870-1913 used to test our model. The log model excludes

all acceleration of growth rates over time. We know that national growth sometimes undergoes such acceleration.

An additional matter should be raised. Normal growth is essential if one is seeking to forecast the future through extrapolation. Periods between wars are regarded as times when normal growth takes place. This is not the case. Wars are but one kind of disturbance; depressions and revolutions can also occasion havoc with patterns of total output, as did the Great Depression of the 1930s. This economic downturn overlapped the period of recovery from the effects of World War I and the end of the era which served as the base for our projections from 1946 to 1965. No analysis could be carried out unless the effects of the depression were eliminated or at least substantially reduced. The simplest and most efficient way was to omit some outlying points to obtain estimates for nations affected by the cycle. It was impossible and theoretically unacceptable, however, to eliminate all of the effects of the depression, and some of the results show the distortions which originate in this disturbance in the base period.

Nor was the depression the only disturbance in the trends of our base period: two revolutions and accompanying civil wars also created major problems. The disastrous effects of the Spanish revolution and civil war on Spain's economic performance caused us to drop that country from our sample altogether, in spite of our wish not to reduce further the already small sample. The Russian revolution and civil war also had extreme effects on the economic performance of the country. Moreover, data were available only for 1895, 1899, 1913, 1920, and yearly after 1928. Nevertheless, we retained the USSR for compelling reasons: Russia was a critical actor in World War I.

Making the best of the situation, we used the data we had and maintained our procedure of estimation of consequences. Reliability measures were quite good, but it should be clear that results in the Russian case cannot be considered as stable or reliable as other projections.

Projections are not predictions. The fitting of data statistically is not equivalent to predicting how much real output there would have been in the absence of war. Moreover, even the correspondence between estimates and reality is not a final test of the validity of the estimates themselves. As Frumkin reminds us, absurd and unscientific estimates occasionally hit the mark.¹²

¹⁰ Scholars have justifiably cautioned that given the deficiencies in data and models in the social sciences, projections should not exceed 20 years; see Heiss, p. 107; Simon Kuznets demonstrates that growth rates of developed countries remained constant or increased in the last century; see his *Economic Growth of Nations* (Cambridge, Mass.: Harvard University Press, Belknap Press, 1971), Ch. 1.

¹¹Kugler, pp. 116-17.

¹²Frumkin, p. 17.

Standardization. Consequences of war as they affect power distribution cannot be estimated merely by the calculation of comparative national losses. Differences in size and rates of growth render meaningless comparisons of absolute losses. Total national capabilities of a country the size of Denmark are smaller than losses of a major power involved in the same great war. Again, identical absolute losses for a fast- vs. a slow-growing nation imply greater absolute losses for the faster growing nation because of its expectation of greater growth. Standardization is obviously required.

We have used three methods. First we constructed an index. Theoretically, the optimal choice of a base year would have been 1930, for it stands at the halfway point in the period under analysis. This was, however, a year of general depression; thus, 1913, the base year established by Angus Maddison who provided us with the original data, was a more useful point of departure, for it was the last year of a very long period of peaceful growth.

We also held constant the size of all nations in our sample to avoid the distortions produced by any changes in boundaries rather than by human or material losses incurred during the war. We dealt with the problem by using throughout the boundaries of 1970. Maddison, whose national product data we used, had followed the same procedure. In his adjustments of his gross national product series Maddison assumed territorial changes to influence output in direct proportion to population changes. ¹³

And we chose years as our unit of measure in order to cope with differences in size and growth rates among nations. Our estimation however was complicated by the fact that our original index was logarithmically transformed and prevented simple evaluations of losses in time units as deviations from the original regression, because algebraic manipulations could not be applied to them. This difficulty was resolved by reversing the dependent and independent variables in the regression estimates and reducing the resulting residuals to time units.¹⁴ For time is obviously a commodity equally available to all and one that is

not affected by growth rates. Thus, when we say that one country has lost or gained 15 years, we mean only that its power resources diminish or increase relative to the performance of that nation in the base period. Because we thought it important to establish negative signs corresponding to losses and positive signs corresponding to gains, the residuals are multiplied by -1.

Independent estimates of normal performance were run for all relevant nations and included the study of each of the wars considered. In all cases, the model was:

Time = $a + b \log of$ the index of total output of each nation + error, where Time = $0, 1, 2, \ldots, n$, where n is the length of the base period.

In this model, the slope and intercept have no significance; only the residuals are important. Two-variables are used in the estimate, and goodness-of-fit measures are not affected when we once again reverse the model to the more familiar function, with total output as the dependent and time as the independent variables.

The Reliability of the Projections. As we have noted earlier, estimates of normal growth after a war depend on stable data for trends in the prewar period. A variety of interrelated statistical techniques was used to determine the reliability of the regression estimates, given the assumptions of that model. The coefficient of determination, R² (showing the proportion of total variance explained), was used to estimate the reliability of the trend in the base period. Average values were utilized to establish levels of tolerance for residuals central to the evaluation of costs of war.

Table 1 displays results of major tests conducted. It is evident from a consideration of Table 1 that the coefficient of determination for the period before World War I is consistently in the high nineties, and that only in the case of the United Kingdom and Japan can one obtain an R² figure below that level, .80 and .87 respectively. Such estimates are not unexpected, given the behavior of the series in that period. Thus, one could consider projections of normal trends after World War I to be stable and reliable.

Results of World War II, following similar criteria, are less satisfactory. Only after controls are imposed to minimize the effects of depression does the coefficient of determination for most countries rise to the levels reached for World War I. Without such controls, coefficients run as low as .04 for Austria, .30 for the United States. But in the cases of some

¹³Angus Maddison, Economic Growth in the West (New York: Twentieth Century Fund, 1964), pp. 194-203; see also Kugler, pp. 31-36.

¹⁴Thomas Sanders, Lutz Erbring, Department of Political Science, and J. Landwehr, Statistical Research Laboratory, University of Michigan, generously contributed to the solution of this difficult methodological problem. They are not responsible, of course, for any weaknesses of the final product.

Table 1. Measures of Goodness of Fit and Range of Residuals in Base Period

			World War I					World War II			
	Number		Residus 1895	Residuals for Base Period 1895–1913 (Years)	e Period (ears)	Number	Depression		Residu	Residuals for Base Period 1921–1939	Period
Nation	of Observations	\mathbb{R}^2	Max. Loss	Max. Gain	Last Year (1913)	of Observations	Years Excluded	\mathbb{R}^2	Max. Loss	Max. Gain	Last Year
United States	19	.97	-1.7	2.0	6'-	19 16	1932–1934	.30 .66	-6.2	4.6	2.6
West Germany	19	66	-1.0	1.0	.7	19 16	1931–1933	.76 .87	4.5	2.7	-1.9
United Kingdom	19	œ.	-6.0	5.2	T-	19 16	1931-1933	8.83 48.	-2.9	2.3	-2.3
Japan	19	.87	4.1	5.0	3	19	none	.92	-2.4	2.6	7
ÚSSR	ဗ		Special Estimate	te		19 16	1931–1933	86. 86.	-1.3	1.5	۲.
Italy	19	96.	-2.4	1.9	9	19 16	1931-1933	8.8.	4.2	3.3	œ
France	18	.92	-2.4	2.7	1.2	19 16	1931–1933	.56 55	5.1	5.5	4.5
Belgium			Data Unavailable	je Je		19 16	1931-1933	.65 79.	-5.1	5.5	4.4
Denmark	19	86.	-1.5	1.3	.6	19 16	1931–1933	95 56	-2.8	2.2	ų,
Sweden	19	96.	-1.8	1.5	1.4	19 16	1932-1934	8. 89.	-2.5	2.1	τ:
Switzerland			Data Unavailable	e Ie		19 16	1931–1933	.79 .79	-3.6	8.4	3.0
Canada	19	.97	-1.6	2.3	9.	19 16	1932–1934	.37 .59	-5.9	6.1	3.2
Australia	- 19	96.	-2.4	1.8	4.	19 16	1931-1933	.56 .79	-5.0	6.4	1.1
Netherlands	14	96.	-1.3	1.1	.7	19 16	1932–1934	.75 .78	4.0	4.4	2.4
Norway	19	.95	-2.2	2.3	2.3	19 16	1931-1933	86. 88	-1.2	2.0	
Czechoslovakia			Not a Nation			17 14	1933–1935	.47 .61	9.9	3.4	n.a.
Hungary			Not a Nation			15 12	1931–1933	.65	-3.4	3.7	n.a.
Yugoslavia			Not a Nation	,		19 16	1932–1934	.83	4.9	3.9	٠.

nations considered, even after controls are established, the coefficients remain low: Austria ($R^2 = .17$), France ($R^2 = .56$), Canada ($R^2 = .59$), Czechoslovakia ($R^2 = .61$), the United States ($R^2 = .66$), and Belgium ($R^2 = .67$). Such evaluations require exegesis.

Austria's performance is explained by her troubled history in the postwar period: Austria-Hungary was dismembered after World War I; the economic depression was particularly acute; there was civil war; and finally, Austria was absorbed by the Third Reich. A stable estimate was not possible. Therefore, Austria was dropped from the sample. France also represents a special case. French growth was very slight in the interwar period, and the regression estimate, while producing a good fit, was statistically not significant because it was close to zero. The remaining low correlations show the effects of the depression of the 1930s. Although the impact of the economic catastrophe is minimized when outliers are removed from the calculations, it is plain from Table 1 that the effects of the depression are still visible; residual values of maximum losses reach -12.3 years in the case of the United States when no controls are imposed and are cut almost in half, to -6.2 years, when three outliers are deleted.

The residuals in our table indicate three points: (1) estimated trends are more reliable for the period prior to World War I than for that before World War II; (2) we expect that the performance of nations over time in the sample will not depart more than two or three years from the projected line during the period covered; (3) the sharp improvement in values in the last year of observation suggests that the forces depressing the economies of the nations considered were being overcome.

One more important item should be noted. Because the information contained in the residuals was so crucial to this study, some additional tests were carried out. In the base period for World War II, some residuals were high when previous values were high. These traces of tracking, however, were sharply reduced when the outlying points for the depression years were eliminated. This operation considerably increased our confidence in the accuracy of the regression model designated. 15

Choice of Test Cases

In discussing problems connected with the

measurement of war consequences, the choice of conflicts to be used is vital. If results are to represent what actually happens as a consequence of war, test cases must be representative as well. Since it is not possible to draw a representative sample of conflicts, it is important to choose those which maximize opportunities for the behaviors we wish to study to surface. If, as the results of such extreme tests, consistent patterns do not occur, we could at least assert with confidence that different findings were not likely to be obtained in less extreme cases. ¹⁶

Thus, our choice of wars to consider could not be arbitrary. We sought two conditions: (1) wars whose contestants were totally committed to victory; otherwise, we would not know if the actual victors would have won had the vanquished made a more serious effort; (2) wars whose consequences were not altered by the interference of other powers. We looked for wars in which the nations which appeared to be victors and vanquished were so in reality, to insure that the consequences ascribed to victory and defeat were properly matched.

Some categories of wars had thus to be excluded: wars between small powers and limited wars did not offer conditions suitable for the testing of our propositions. Even assuming that small countries fought as devotedly as possible, one could not be certain that the consequences were not determined by the behavior of nations not directly participating. The Middle East offers a good illustration of this difficulty: Egypt, Syria, Jordan, and Israel have been combatants in several all-out wars over the years. All contestants have fought to the limits of their strength. Yet the losers have always alleged that the outcomes and consequences were chiefly influenced by the interference of outside interests, specifically, the United States and the Soviet Union. Consequently, such conflicts do not provide adequate tests for our hypotheses.

Limited wars are equally if differently perplexing. In such conflicts, one or both sides limit their efforts and their objectives. Outcomes, therefore, demonstrate only what the contestants actually accomplished, not what they might have achieved had they committed themselves fully to the fight. We must select for testing wars sufficiently important and hardfought to eliminate possible suspicion that the

¹⁵See Kugler, Ch. 4 and App. III.

¹⁶This procedure was used by economists in the study of economic depressions. See Arthur Burns, ed., *The Business Cycle in a Changing World*, 2nd ed. (New York: National Bureau of Economic Research, 1969), pp. 3-53.

combatants were not exerting maximum efforts. Limited wars, therefore, do not make good test cases for our propositions.

We have chosen for consideration conflicts¹⁷ in which the contestants on each side included at least one major power as an active participant, for wars involving great powers can be expected to be massive undertakings, and their outcomes are not easily influenced by external forces. We also insured, in two ways, that the great powers were themselves completely committed to the struggles. We stipulated that wars chosen should show a quantum leap in the number of battle casualties, by comparison with the carnage produced by any previous war. We further insured that the commitment of all combatants was absolute by selecting wars on whose outcome depended loss of territory and/or population.

The availability of data is a final and indispensable criterion in the selection of wars suitable for the testing of our proposals. Lack of data has been a problem besetting most rigorous studies of international politics. Only very recently has research made available reliable time series data, in a small number of areas, going back to the end of the nineteenth century. Such series, however, are still very rare; quality and availability drop virtually to the vanishing point when material occurring in the period prior to 1900 is at issue.¹⁸

The stringent theoretical and data requirements reduced to a very small number the sample of utilizable wars. Although the Napoleonic Wars and World Wars I and II meet the theoretical criteria we have established, only for the latter pair have we time series data at frequent enough intervals and of high enough quality to permit the analyses we wished to undertake. Here, therefore, we must raise the

17We accept Quincy Wright's theoretical definition of war as "the legal condition which equally permits two or more hostile groups to carry on conflict by armed force," in A Study of War, abr. ed. (Chicago: University of Chicago Press, 1964), p. 7; and we further accept the operational definition of Lewis Richardson that any conflict resulting in the death of approximately 300 people can be considered a war. Since our concern is with international war we utilized the list of wars between 1815 and 1965 provided by J. David Singer and Melvin Small in their Wages of War, 1816–1965: A Statistical Handbook (New York: Wiley, 1972), pp. 17–19, 30–32, and 58–70.

18The bulk of the capability data for our study comes from Angus Maddison, "Trends in Output and Welfare," unpublished manuscript, 1972. The gratitude of the authors goes to Dr. Maddison, who generously allowed us access to a data set, as yet unpublished, which made this study possible. For a complete data set see Kugler, App. II.

question of whether or not the analysis of two wars can be more generally representative than the examination of simply two case studies, with all the limitations this approach has on the drawing of broad inferences beyond the cases themselves.

We think that the results of this study should be considered sources of significant generalization because, although the number of wars we analyze is extremely small, the sample of 31 cases we observe is adequate. These cases are observed throughout the two most terrible wars in history and include most of the countries of the central international system. Moreover, the two great wars we examine are those which offer the best opportunity for testing our propositions. The reduction of our area of study to two wars undoubtedly has sharply restricted the nature of the inferences that one might wish to make. On the other hand, the process developed allows tests on a much enlarged sample as soon as improved theory, methodology, and data permit it.

Actors

Some would argue that in the study of war one does not learn much of value if sole attention is given to the behavior of nations, and that other levels or kinds of analysis are more appropriate. In some cases, one might justifiably prefer to concentrate on units of analysis other than nations. Were one interested in questions related to causes and outcomes, particularly the former, it might be meet to observe the views and behavior of military leaders, politicians, diplomats, industrial and labor officials, and mass publics. However, if one is interested in the effect of victory and/or defeat in war on the distribution of international power in postwar periods, this is not the case. Here decision making and the behavior of individuals play an inconsequential role; the nations are the only actors.

Casting nations as uniquely relevant actors poses its problems. In order to compare the performance of a country before and after war, one must be certain that one is comparing the behavior of the same entity over time; otherwise, differences in behavior would legitimately be ascribed to the fact that the entity itself is different. The problem inherent in this kind of comparison is that nations are not as stable over time as one could wish. Ideally, one would expect a nation to have as a minimum: (1) a fixed territory and population; (2) governmental sovereignty over both; and (3) the formal diplomatic recognition of most of the international community. Reality is different

from the ideal. Nations shrink and expand, established countries disappear, and new ones appear in their places. Some are administered, at least in part, by international bodies, while still others make a token effort to supervise populations and territories within their legal jurisdiction but in fact are regulated by rebel groups prompted by authorities operating abroad. Increasingly, this reality has been recognized; ¹⁹ the most exhaustive list of nations compiled to date has been established on the premise that not all the elements of sovereignty, territorial continuity, and international recognition need be present all the time. ²⁰

In seeking the cast of nations whose performance we should measure, it was clear that it should incorporate all belligerents in World Wars I and II, with a group of nonbelligerents serving as a control group. We began with the comprehensive list of belligerents compiled by Singer and Small in their classic data-gathering effort, The Wages of War, to which we added, as an initial massing for a control group, all of the nations in existence at the time of the conflict under study which had not participated in it.²¹

The first list had to be pared. We immediately eliminated from our sample all nations that were not developed, ²² since there was no way to estimate their power satisfactorily. A number of combatants in the two wars had to be dropped from our consideration for this reason. This category is unfortunately long, including Mongolia, Bulgaria, Rumania, Ethiopia, Greece, Brazil, and Turkey. On the other hand, South Africa, New Zealand, Poland, and Finland were eliminated from the study because data were inadequate.

We also had to take into account the fact that Australia, Norway, Germany, Japan, and Austria were not totally free and independent for part of the period important to this study. Australia had not achieved dominion status; Norway had not been separated from Sweden. De jure, therefore, they were not free, although they were free in fact and in action. Finally

disregarding their legal status, we treated them as free and independent units. Germany and Japan were not free because of their occupation after World War II; Austria was annexed by Germany before the onset of that war. For our purposes this inhibition could be devastating: loss of freedom of action was not important if it occurred during a war, but a nation under the control of one or more other nations either before or afterwards might lack independence in the allocation of its resources; this, in turn, could be a factor influencing its prewar pattern of growth or its recovery period. We decided that there was no reasonable way to establish control for the possible data distortions due to foreign occupation and determined simply to consider as continuous units all nations which recovered their identities after occupation. Germany and Japan were included without giving weight to their occupation periods. This sample was further reduced when we lost Spain and Austria because reliable estimates could not be generated.

The Analytic Groups: Belligerents/Nonbelligerents; Winners/Losers; Active/Occupied. Having assembled our basic components, we divide our cast into belligerents and nonbelligerents, winners and losers, both active and occupied. First, a distinction must be made between nations which were belligerent and those which were merely hangers-on, always present in major coalitions. This determination is not easy to make. Argentina, for example, declared war on the Axis in the waning days of World War II. She never dispatched a soldier to the war area, and for most of the war her sympathies were clearly with the opposite side. Was Argentina a belligerent? Brazil is another case in point: she sent only a minuscule force to fight, and her principal contribution was permission to use an air base for refueling planes destined for Africa. How is one to separate the belligerents from the countries whose behavior ranged from total neutrality to one of merely symbolic participation?

Much the same kind of difficulty presents itself within the category of winners. The position of France in World War II is an example of this problem: for much of the war France was fully occupied by Germany. Yet France began and ended that war on the side of the winning coalition. Was France a winner, in the same sense as England, the Soviet Union, or the United States can be thought winners? One would be forced to reject such a notion. The same distinction separates losers. Italy began the war on the side of the Axis, but switched

¹⁹ See Deutsch, p. 70; John Herz, International Politics in the Atomic Age (New York: Columbia University Press, 1959), p. 104.

²⁰Singer and Small, pp. 58-90 and Bruce Russett, J. D. Singer, and Melvin Small, "National Political Units in the Twentieth Century: Standard List," American Political Science Review, 62 (September 1968), 932-51.

²¹Ibid.

²²Wherever more than 50 percent of the workingage males were in non-agricultural pursuits, the country was considered developed.

allegiance in the middle, and ended as a member of the Allied forces. Is Italy to be construed a winner or a loser?

Operational distinctions such as those above require definitions which clarify any ambiguities arising from the theoretical conceptions; thus the following definitions of the analytic groups:

- 1. Belligerent nations are those whose participation in the conflict resulted in military losses of 5000 troops.
- 2. Nations whose strict neutrality or symbolic participation resulted in losses of less than 5000 troops were considered nonbelligerents.
- 3. Nations still fighting in the final third of the conflict on the same side as at its beginning were classified as active belligerents.
- 4. Belligerents which in the last third of the conflict were no longer members of the coalition they had joined at the beginning were classified as occupied belligerents.
- 5. Nations that retained all their territories or extended them immediately after a conflict and as a direct result of that conflict were considered winners. The rational support for such a definition is plain: no victor in a major war would tolerate the loss of any territory under its jurisdiction.
- 6. Loss of territory would be construed as the behavior of a loser. Even transfer of territory, with full compensation made for that loss, is considered an overt sign of defeat; for no victor would submit to such terms, and only a loser would have no alternative.²³

The analytical groupings that we use from different combinations of our three fundamental dichotomies—belligerent/nonbelligerent, active/occupied, and winner/loser—do not exhaust all the logical combinations one could explore. They do, however, satisfy the theoretically interesting possibilities for the analysis of war. Such groups can be most simply and elegantly expressed in the theoretical language of set theory:

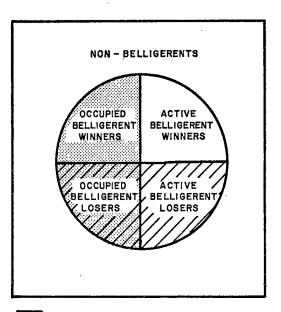
Active Belligerent Winner $= B \cap (O \cup T)^C$ Active Belligerent Loser $= B \cap T \cap O^C$

²³Charles Kindleberger, "International Political Theory from the Outside," in *Theoretical Aspects of International Relations*, ed. William Fox (Notre Dame: Notre Dame University Press, 1959). Our operational indicator of a loss in war is based on the observation that winners in the past have extracted from losers some territory, while never giving up any of their own. We assume, but have no evidence, that losers would have exacted territory from their adversaries had they won the war and cannot rerun the conflicts to see what would have happened had the outcome been reversed.

Occupied Belligerent Winner $= B \cap O \cap T$ Occupied Belligerent Loser $= B \cap O \cap T$ Nonbelligerent $= B^C$

The above can best be represented in a Venn diagram (Figure 2).

We are also interested in an additional distinction that crosses all the analytic groups we have established. We should like to explore the consequences of war for major powers as well as for the entire group of nations; we should also like to analyze the consequences of both wars and then, individually, for each of the two wars. The reason for this attempt to disaggregate is that our sample is so small that it is imperative to see whether or not the patterns of behavior we find are a by-product of our aggregation and, as such, not present when disaggregation occurs. Two points need emphasis, however. First, we do not wish to disaggregate at the level of individual nations. Second, the results of disaggregation are not



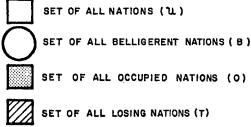


Figure 2.
Venn Representation of Analytical Groups

independent findings for they are based on the same sample.²⁴

The distribution of our total cast of nations into analytic groups discussed above can be seen in Table 2. The sample of 18 nations and 31 cases respectively was the very best we could assemble given our theoretical constraints and the lack of data. It is scant, but sufficient for the research we wished to undertake.

Theoretical and Empirical Propositions

Theoretical Propositions. To many observers, the power position of members of the international system seems obviously influenced by their choice to participate in conflict and by their fortunes in war. Most scholars and practitioners have believed that it made the greatest difference to the power of a country in

24Our results are not simply the effects of the aggregations. The number of nations in each of our major analytic categories is very small; however, the number of points used in our base period in order to make our calculations is quite adequate. Conscious that aggregations inevitably distort results, we inspected the behavior of each of the countries to see if their individual performance deviated widely from the performance of the analytic groups, and we found that this was not the case. Moreover, similar behaviors of the analytic groups is found in partition after partition making clear that aggregation does not cause the results obtained.

the modern age whether it won or lost a war. Very few disputed this view.

In our effort to separate the long-run from the short-run consequences of war and to present, in an approximate way, the distribution of opinion, three important theoretical propositions, with different short- and longterm results, seem to emerge.

The first proposition asserts that in the short run the movement of power between winners and losers creates a pattern much like the gradual opening and then closing of a pair of scissors. J. M. Keynes maintained that the gap which developed between winners and losers as a result of war would continue to increase for a short time in the future and the losers would fall behind at an ever-growing rate. Interestingly enough, Keynes also thought that in the long run, losers laid waste by economic ills would contaminate their partners in trade and bring chaos to the entire system. Thus, the winners would move downward to join the losers, and the gap between them would tend to disappear.25

25 John Keynes, The Economic Consequences of the Peace (New York: Harcourt, 1920). For discussion of the short-term effects of wars on belligerents, see United States Strategic Bombing Survey, The Effects of Strategic Bombing on the German War Economy (Washington, D.C.: Government Printing Office, 1945); cf. also, The Effects of Strategic Bombing on

Table 2. Final Sample of Nations Used in the Analysis

	Active Belligerent Winners	Active Belligerent Losers	Occupied Belligerent Winners	Occupied Belligerent Losers	Nonbelligerents
Australia	I & II ^a				
Belgium			II		
Canada	I & II				
Czechoslovakia				II	
Denmark			11		I
France*	I		II		
Germany (West)*		I & II			
Hungary		II			
Italy*	I	II			
Japan*		II			I
Netherlands			II		I
Norway			H		I
Sweden					I & II
Switzerland					II
Russia (USSR)*	II	I			
United Kingdom*	I & II				
United States*	I & II				
Yugoslavia			II		
Total WWI	6	2		_	5
Total WWII	5	2 4	· 6	1	2

aI = World War I; II = World War II; * = Major Powers.

A second view suggests that as a result of war all nations lose national power, but the winners do not lose as much as losers, and the gap thus created between victors and defeated nations continues for a lengthy period of time. Norman Angell was a supporter of this notion.²⁶

A third proposition, advanced by A. F. K. Organski, asserts that while it is indisputable that losers suffer a good deal more than winners and are in a much worse position immediately after conflicts, levels of power distribution return reasonably soon to the patterns they would have followed had no war taken place. The mechanics of the change work in approximately the following way: After their defeat (and the plummeting of their capabilities) losers accelerate their recovery. Winners, in the wake of victory, show a rate of recovery in capabilities depleted by war which is substantially slower than that of losers. Neutrals are not affected. Within a relatively short period of time, all nations return to the levels of national capabilities they would reasonably expect to hold had there been no war. There is a convergence of winners and losers; the major reason for this seems to be the speed with which the losers recover. They appear almost literally to rise from the ashes of their defeat.27 There is a "phoenix factor" at work.

Empirical Propositions. Let us phrase our empirical propositions in the precise operational language of this study. We have eight hypotheses: The first four represent short-range expectations; the second four represent longrange expectations. Our propositions for short-range effects are:

 \dot{H}_1 —Belligerent countries that emerge as winners of major wars gain in power; nonbelligerent countries retain their antebellum power patterns; belligerent losers suffer substantial power losses.

H₂—In the period immediately after a war, belligerent winners and nonbelligerents retain antebellum power patterns; belligerent losers

suffer substantial power losses.

H₃-After major wars, all belligerent countries suffer major losses in their power capabilities; nonbelligerents retain antebellum power patterns.

Our null hypothesis is stated thus:

H₄—After major wars, the power patterns of belligerents and nonbelligerents are not affected in a systematic manner.

Our propositions for long-range effects are: H_5 -All groups involved in war suffer long-range losses of power and do not regain power patterns at levels established before the onset of major conflicts.

H₆-Belligerent winners and nonbelligerents retain antebellum levels of growth; power cleavages between belligerent winners and nonbelligerents, on the one hand, and belligerent losers, on the other, are rapidly erased by the accelerated recovery rate manifested by losers.

H₇-Differences which result from victories and defeats in major wars are maintained or even slowly increased; thus, the immediate postwar gap between belligerent winners and nonbelligerents, on the one hand, and losers, on the other, is maintained or possibly enlarged.

Our null hypothesis for long-range expectations is:

H₈-The postwar patterns of all groups considered are not affected in a systematic manner as a result of war.

Two final points: it should be clear that in order to test our propositions about long-range effects, the null hypothesis for short-range expectations must be disproved. Second, the two sets of hypotheses, as well as the theoretical propositions that precede them, do not represent all the logical possibilities, but do summarize the views expressed in the literature regarding the outcomes of war and the effects of these outcomes on power distribution.

Now let us turn to what we have found.

Findings

We first wish to examine the behavior of the entire sample; then, in order, that of the subset of great powers taken together; and then that of both for each war taken separately. Had our sample of countries and wars been larger, the performance of all countries taken together would have offered evidence of the kind of behavior we could expect from winners, losers, and neutrals as a consequence of even a major war. However, due to the smallness of the sample, it seemed wise to explore, as deeply and broadly as possible, how important subsets of the total sample behaved. It is important to note that while each of the partitions of the

Japan's War Economy (Washington, D.C.: Government Printing Office, 1946).

²⁶Two works argue at length that there are enormous permanent losses as a result of war. See Norman Angell, *The Great Illusion* (New York: Putnam, 1933), and John Nef, *War and Human Progress* (Cambridge, Mass.: Harvard University Press, 1950).

²⁷For the effects of war on belligerent populations, see United Nations, *The World Population Situation in 1970*, Population Studies No. 49 (New York: United Nations, 1971).

total sample is separately and discretely observed, they are not independent, since the sample is identical for all.

The first partitions permit us to see the behavior of all nations in both wars. Only three of our analytic groups (active belligerent winners, active belligerent losers, and nonbelligerents) are considered. Occupied belligerent winners and losers are excluded because, by our definition, there were no occupied countries during World War I.

The results (Figure 3) show that in the first two postwar years, belligerent winners and nonbelligerents lose from 1.5 to 3.5 years. The deviation of both groups from expected performance is minimal. Active belligerent losers, however, suffer losses in a comparable period of 20.4 to 21.6 years. The difference between the two groups is substantial: 19 years.

In the long run, the nonbelligerent nations retain growth rates characteristic of their prewar performances. Active winners incur an average deviation of about three years from the zero line, indicating that losses suffered after postwar demobilization are maintained. Among

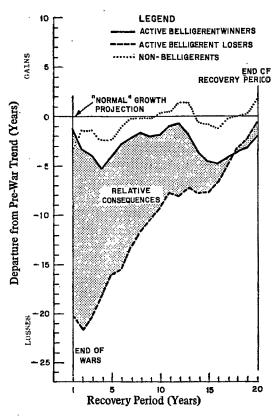


Figure 3. Partition I: General Consequences Sample: All Nations, Both Wars

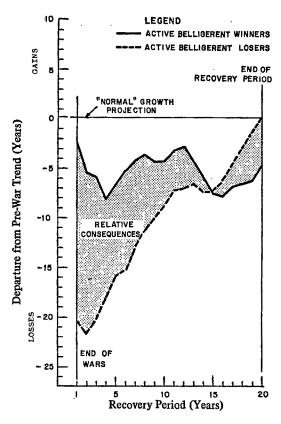


Figure 4. Partition II: Major Power Consequences Sample: Major Powers, Both Wars

active belligerent losers, however, the Phoenix phenomenon manifests itself. Losers begin and maintain a steadily accelerating recovery rate after the war and overtake the winners in the eighteenth year of the postwar period. At the conclusion of that period, differences in power distribution among all groups have been eradicated; levels of power return to points one would have anticipated had no war occurred.

Thus, the results we obtained strongly support hypotheses two and six.

In testing the second subset of actors in our breakdown, the behavior of the great power system over two wars, the results are very much the same (see Figure 4). Only active belligerent winners and losers are considered, for none of the great powers was neutral in World War II. Active winners begin with a two-year loss immediately after the war, then recover briefly, only to slide away once more. Overall, their performance is below expected levels and is heavily influenced by depressions in the interwar period. However, the trajectory as a whole reveals no loss in relation to prewar capabilities.

The active belligerent losers, on the other hand, suffer a 20-year loss immediately after the war, but recover rapidly, overtaking the winners in the fifteenth year. After this point, both groups resume previous patterns.

Hypotheses two and six are confirmed again by a study of the composite data of the great powers for both wars. In our next four operations, we partitioned our sample to show the performance of the entire system and the subsystem of great powers for each world war.

Our third partition permits us to observe the behavior of the entire system after World War I (see Figure 5). In the short run, nonbelligerents appear slightly affected during the first year after the war, but remain within one year of the zero line during the second. The active belligerent winners incur losses of from five to seven years right after the conflict. Active belligerent losers suffer losses of from 21 to 25 years. Winners lose, but losers suffer four times more severely. There can be no doubt that in

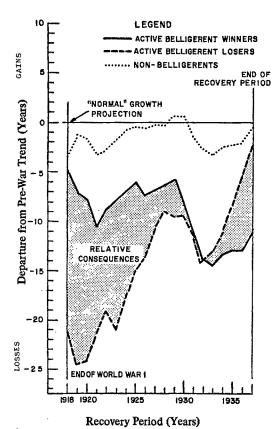


Figure 5. Partition III: World War I Consequences Sample: All Nations, World War I

the short run, there are serious power consequences both to winners and losers of major wars. Again, the evidence in large part supports hypothesis two, although active belligerent winners do suffer markedly.

The evaluation of long-term consequences is intimately related to the effects of economic depression. Consider first the initial 12-year period after World War I. The characteristics described in hypothesis six are supported. Active belligerent winners recover very slowly from war effects; nonbelligerents retain previous growth patterns; active belligerent losers, after the immediate postwar period of heavy loss, display a substantially faster rate of recovery than winners. Then the Great Depression strikes and, within two years, performances of all groups are diminished. Belligerents, however, seem to suffer more than nonbelligerents. After 1933, predepression trends reestablish themselves, but the subsequent period is too short for adequate evaluation.

The great power system during World War I behaves in much the same way described for the sample as a whole (see Figure 6). Active belligerent winners fare slightly worse than the whole sample, but differences are so marginal that we feel secure in concluding again that the results support hypotheses two and six. We draw this conclusion despite the disruption caused by the depression. The graph shows the depression to be a major factor in distorting the recovery patterns of all countries taken together, not merely for World War I but for both conflicts. Moreover, the depression is also a major reason why our projections for the period following World War II are so weak, underestimating egregiously the growth trends (see Figure 7).

In our fifth partition, all major analytic groups are represented for World War II. Some of these representations, however, are so tentative that one needs to take the information they convey with caution.

The consequences of World War II on power distribution, in its immediate aftermath, are much as expected. In the first year, active belligerent winners are slightly ahead, but move toward the zero line in the second year. Nonbelligerents are slightly below the zero line but move to points within tolerance limits (two years for this partition) in the second year. Active belligerent losers suffer substantial losses, between 16 and 17 years, in the first 24 months after the war, and the occupied belligerent winners lose from 11 to 14 years in the same period. The differences of loss between

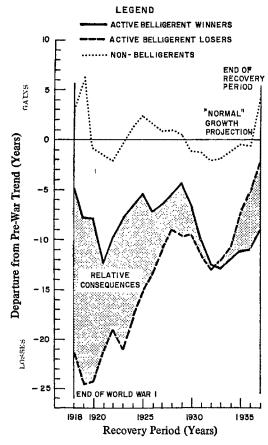


Figure 6. Partition IV: World War I Major Power Consequences Sample: Major Powers, World War I

active winners and active losers range from 21 to 23 years in the initial period.

In the long-run analysis we find, for the first time, evidence to support the proposition that the gap between winners and losers can continue instead of closing.

A number of points should be made. The logarithmic projections seriously underestimate the growth of the system. An indication of this is to be seen in the fact that nonbelligerents make increasing gains over time, indicating that prewar patterns distorted by the depression are not a good indicator of the behavior of the group as a whole in the period after World War II. In some part, however, as Kuznets suggests, economic growth since 1945 is due to a liberalization of trade in the industrial world and is therefore unexpected.²⁸ It is our impression that a more accurate projection of growth

trends would place the zero line approximately where one finds the trajectory for the nonbelligerents. In any event, our main concern is with active winners and losers, and we must first establish that distortions in the pattern do not, in that respect, affect relative calculations. Over the entire period, the active belligerent winners maintain a constant but slight edge over nonbelligerents amounting to about three years. Absolute differences between winners and losers are not distorted by the acceleration of recovery rates.

One should note that here, too, active losers enjoy a sharply accelerated recovery pattern and regain the prewar level of growth within the stipulated period. They do not actually close the gap between themselves and the winners because of: (1) the acceleration of the entire system; (2) a decided deceleration in recovery in the last five years of the period,

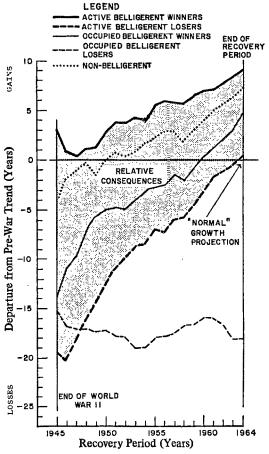


Figure 7. Partition V: World War II Consequences Sample: All Nations, World War II

²⁸Kuznets, p. 43.

which may have caused an absolute loss of from five to eight years.

Nevertheless, a gap of roughly nine years remains at the end of the recovery period, and one might argue that we have, as a result, evidence supporting hypothesis seven, that winners maintain the advantage they gain from victory over the long-run.

The remaining analytic groups behave in interesting ways. The performance of occupied belligerent winners is somewhere between that of active winners and losers and follows closely the performance of the former. Occupied belligerent winners regain prewar rates in 15 years, surpass them, and come close to convergence with winners at the end of the recovery period.

Since only Czechoslovakia falls into the category of occupied belligerent losers, one obviously cannot refer to "findings" in observing the behavior of one country. But one should note that the matter merits investigation when sufficient data become available; for if other occupied belligerent losers behave as does Czechoslovakia, we may have identified the real losers in major wars-the nations which do not recover. It is possible that we have also identified the conditions necessary to support an alternative hypothesis to those advanced here. This may be significant. In all the analyses so far offered, we have discerned only marginal differences in the long-range consequences of victory and defeat on the power distribution of the system. However, if the case of Czechoslovakia is a true indication of what obtains for other occupied belligerent losers, it would then be clear that had the victors insisted on occupation, exploitation, and repression of defeated populations, our findings would be dramatically different. It may be that victors can delay the recovery of the vanquished by occupation and repression. For example, if Hitler, with his plans to depopulate and exploit his victims, had won the war, the vanquished might not have recovered. Had Hitler been victorious, hypothesis five might have been sustained.

Be that as it may, the results of the fifth partition should not be viewed separately, but rather must be compared with those of partition six (see Figure 8) for the deviations disappear when one observes only the subsystem of great powers in World War II, which behaves entirely in consonance with the expectations of hypotheses two and six. In the short term, active belligerent winners suffer no loss, occupied belligerent winners and active belligerent losers, suffer a loss of 20 years of "normal" growth in the first two years after the war's

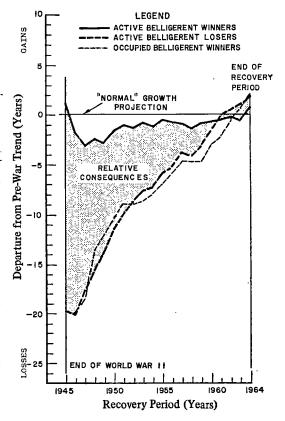


Figure 8. Partition VI: World War II Major Power Consequences Sample: Major Powers, World War II

end. The relative differences between active winners and the latter two categories in this period are 20.9 and 18.1 years respectively. Evidence on long-term effects fully supports hypothesis six. Active winners maintain expected growth patterns, while active losers accelerate recovery rates and overtake winners in the sixteenth postwar year.

How is one to reconcile these results with those obtained when examining the behavior of the total sample? Some explanatory points should be considered: the differences noted across the two partitions are not rooted in the behavior of all our analytic groups. The behavior of the active losers and occupied winners changes only slightly from one partition to the other. It is also plain that the logarithmic model we chose projected accurately the performance of great powers in the period after World War II. Not projected accurately, therefore, are the behaviors of the smaller winners. Their unprecedentedly rapid growth inflates the performance of the entire sample.

The Phoenix Factor

Most unexpected and interesting is the discovery that after wars the active losers catch up with winners in comparatively short order, and that the system of international power begins to behave as one would have anticipated had no war occurred. We cannot explain the phenomenon; we do not know why losers rise from the ashes as they appear to do.

We can, however, make some surmises. It is plausible to believe that structural elements may play a part. For example, favorable occupational distributions may help to accelerate recovery rates, as may the destruction of obsolescent plants and industrial equipment. It is probable that attitudinal factors may also play a significant role in increasing the pace of recovery. A defeated but economically developed population living in the midst of destruction will recall the antebellum status quo and be motivated to rebuild. So motivated a populace would have the technology to make an economic system function well. It is also plausible that the defeated population would exert a greater effort to recover than that of a victorious country-the latter more intent on enjoying the spoils of war. The necessity for work and sacrifice is evident to all members of a vanquished society. Charles Tilly found, for instance, substantially fewer strikes in Italy and Germany for a time after World War II than in England and France.²⁹

These reasons are credible, but we have no assurance that any of them is accurately to be judged responsible for hastening the recovery rates of defeated nations. We do know, however, that one aspect widely believed to be influential had little if any effect. Losers do not rise from the ashes because winners pick them up and help them to their feet. Were this true, it would completely overturn the results we have obtained. For if losers could not recoup their losses without aid, the gap between them and winners would remain if an active winner refused this kind of assistance. This would support the continuing-gap proposition, one of the possibilities previously hypothesized. The point was of major concern, and we tested it with two propositions. First, we were interested to discover whether or not aid was positively associated with the recovery of the recipient and, second, whether or not a large-scale foreign assistance effort does start recovery on its way, as some economists believe.

29 Charles Tilly, personal communication to the senior author.

Our test was simple: since the United States was the major source of aid after World War II, and since such help was dispensed annually between 1948 and 1961, we compared the amounts of aid given by the United States, in totals and per capita, with the relative growth rates of the recipients—Japan, Germany, Italy, the United Kingdom, and France.³⁰

Had there been a direct relationship between aid and recovery, and if one controlled for population, growth rates would show increases as a result of aid. Had aid intensity been a factor one would also expect that growth rates would show strong gains after those years when recipients received particularly large gifts. Because we are dealing with time series data we replicated each evaluation controlling for possible linear influences of time. Table 3 displays the connection between United States aid and the recovery rates of the beneficiaries.

From a consideration of the above figures, we can determine only the weakest association between external aid and recovery; the variance explained by the coefficient of determination is always below .1, indicating that growth and foreign aid, totals or per capita, are almost wholly independent of each other. Such relationship as may exist is negative: the countries that received most of the aid for the longest period performed worst. The United Kingdom received much more aid on a total and per

³⁰One would expect the growth rates of aid recipients to show effects of the foreign help a year after aid had been received. We tested our assumptions of time response by lagging aid and recovery rates for 2 and 3 years and our results remained unchanged. We calculated the recovery rates of each recipient by subtracting the growth rate in the year aid was received from the growth rates posted in the following year. For example, if a nation posted −15 years of growth in 1948 and −13 years of growth in the following year the recovery rate was adjudged 2 years. It should be emphasized that negative numbers do not necessarily indicate a lack of growth but rather a lack of recovery.

Recovery rates were calculated as follows:

Recovery Rate = Relative Growth_{i+1}
- Relative Growth_i

Where i = Recovery Years

Data on aid was obtained from the Agency for International Development, Office of Statistics and Reports, U.S. Economic Assistance Programs Administered by the Agency for International Development and Predecessor Agencies, April 3, 1942-June 30, 1971 (Washington, D.C.: AID, 1972), pp. 46, 68-76; population figures from Arthur Banks, Cross-Polity Time-Series Data (Cambridge, Mass.: MIT Press, 1971), Segment 1, pp. 3-54; our sample included only West Germany, Japan, France, and the United Kingdom as we were only interested in major power behavior. No data on Soviet aid was available.

Table 3. Correlation of U.S. Aid With Recovery of Recipients, 1948-1961^a

	Aid Years	Aid	Aid Partialed on Time	Aid Per Capita	Aid Per Capita Partialed on Time
Recovery Rate	1948–1961	01 (N = 63)	17 (N = 63)	01 (N = 63)	15 (N = 63)
	1948–1953	33 (N = 23)	21 (N = 23)	18 (N = 23)	30 (N = 23)

^aThe correlation coefficient R is used to indicate positive or negative influences, but \mathbb{R}^2 is never above .1.

capita basis than France; France received much more than Italy; Italy much more than Germany; and Germany much more than Japan. ³¹ Yet it was Japan that enjoyed the more rapid rate of recovery, followed by Germany, Italy, and France, with the United Kingdom bringing up the rear. It is, therefore, very hard to credit the conviction that foreign assistance and recovery are closely associated.

These particular findings are not completely unexpected.³² Many economists have questioned the efficacy of this kind of aid.³³ What the figures underscore is that foreign assistance, as a form of investment in the economy of another country intended to incline it toward faster recovery, is not very effective. The variables truly important to recovery lie within the devastated nations themselves. Previous patterns of performance are far more significant than external aid.

Conclusions

We began this inquiry with a number of questions: Does the outcome of a major war reshape the distribution of international power?

Does it make a real difference—in power terms—whether a country wins or loses a major war? How long can winners hold on to their advantages? How long do losers stay behind?

Let us begin with a simple list of what we have found.

- 1. Systematic patterns in the distribution of power (as measured by gross national product) are registered after major conflicts.
- 2. The power levels of winners and neutrals are affected only marginally by the conflict.
- 3. Nations defeated in war suffer intense short-term losses; the outcome makes much difference to them in the short run, especially in terms of power levels.
- 4. In the long run (from 15 to 20 years), the effects of war are dissipated, because losers accelerate their recovery and resume antebellum rates; they may even overtake winners. Soon, the power distribution in the system returns to levels anticipated had the war not occurred. We have evidence that this happens and we can speculate about the explanation, but we have no definitive solution. There is substantial research remaining to be done.

If one wishes to forecast the behavior of a country 15 or 20 years after the end of a war, one should not refer to the outcomes of that war, whether the country in question participated in it or not, whether it was a winner or a loser. The best indicator of the power posture of a nation less than a generation after the conclusion of a war is its performance before that conflict.

One other finding should be mentioned. It is clear that the assistance offered by winners to losers is not a significant factor in the losers' recovery rate.

We are tempted to suggest that the outcomes of war, insofar as international power is concerned, make no difference. We cannot forget, however, that we have found a tracing which indicates that we may have heard only part of the story. While winners cannot help losers to recovery by contributing aid, whatever the quantity, winners may be able to prevent or

³¹Kugler, pp. 196-202.

³²The literature on aid is immense; an excellent review is available in I. M. D. Little and J. M. Clifford, International Aid (London: Allen and Unwin, 1968). The "big push" proposition was derived from Paul Rosenstein-Rodan, "International Aid for Underdeveloped Countries," Review of Economics and Statistics, 43 (May 1961), 107-38, and his classic arguments on the "big push" proposition in "Problems of Industrialization of Eastern and South Eastern Europe," The Economic Journal, 53 (June 1943), 204-07, elaborated in "Notes on the Theory of the Big Push" in Economic Development for Latin America, ed. Howard Willis (New York: St. Martin's Press, 1961), pp. 57-66.

³³For a classic discussion of the uses of foreign aid see Milton Friedman, "Foreign Economic Aid: Means and Objectives," and Charles Wolf, Jr., "Economic Aid Reconsidered," in *The United States and the Developing Economies*, ed. Gustav Ranis, rev. ed. (New York: Norton, 1973), pp. 250–78.

delay the recovery of losers. If the behavior of Czechoslovakia is an accurate indication, the victor may retard the recovery of the loser by occupation and exploitation. Adequate information on this aspect of the question is not available; there are merely hints on which to base speculation.

Such findings are clearly tentative. In this case, however, the tentative nature of the conclusions should be stressed once more because we have been plagued with data problems. If our findings are confirmed through comparable and more exhaustive researches, the implications for study strategies could be substantial.

If the distributions of international power and changes in that distribution are shaped by differential rates of growth across critical sectors of a nation's life and across nations of that system, and in the long term such rates cannot be altered even by the most violent international interactions, such as major wars, then what must be studied are the causes of such alterations and not the interaction of countries. Thus, independent variables in international relations are not found in international relations, but in the growth of the units that comprise the system. Some scholars already study international politics in this fashion, but this is still a very different conception of the field from that traditionally held.

A Stable System of Mutual Nuclear Deterrence in the Arab-Israeli Conflict*

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Many professional observers have come to the conclusion that, despite denials and technical problems whose solutions are not publicly known, Israel already has nuclear weapons or has completed all but the final steps in their fabrication. It is also widely believed that one or more Arab states will come into atomic possession within a 10 to 15 year time frame. The present analysis explores the consequences of the establishment of a regional mini-balance of terror. The central hypothesis is that apocalyptic images and doomsday visions have been accepted too readily and out of proportion to the arguments that are given, and that a stable system of mutual deterrence may be viable in the Middle East and may make a positive contribution to the process of political settlement. Problems of rationality, credibility, second-strike force survivability, escalation, tactical nuclear weapons, accidents, permissive action links, terrorism, preventive war, and the disclosure of nuclear weapons possession are discussed.

Do we stand at the brink of the transformation of the Arab-Israeli conflict to a nuclear stage? Israeli officials have consistently and explicitly denied that Israel possesses the atomic bomb, and they have affirmed repeatedly for over ten years that "Israel will not be the first to introduce nuclear weapons in the Middle East." But at least 16 independent reports published outside Israel have asserted that it either already has nuclear weapons or is going ahead with determination to get them, including statements to this effect by Anwar Sadat, the United States Central Intelligence

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¹ For example, Prime Minister Yitzak Rabin said on Danish television on December 17, 1974, that "Israel is not a nuclear power and Israel has no nuclear weapons." International Herald Tribune, August 1, 1975. Foreign Minister Yigal Allon repeated in October, "We don't have nuclear warheads." Newsweek, October 20, 1975.

²This formula apparently was first used by Levi Eshkol in the Knesset, May 18, 1966. In recent years, a caveat sometimes appears that, while Israel will not be the first, she won't be the second either.

³Sadat told Iranian publisher Farhad Massoudi that he believes Israel already has nuclear weapons. "The Middle East Ready to Explode, Sadat Says," New York Times, December 17, 1974.

Agency,⁴ Der Spiegel,⁵ and the Soviet newspaper Moskovski Komsomoletz.⁶ Mohammed Hussanein Heikal claims to have information that there are 6 to 10 "heavy and primitive... dirty bombs... almost certainly stored on a particular air base outside Eilat;" Yassir Arafat has reliable reports from "our own sources inside Israel" that there are exactly 3 to 5 bombs; and Time Magazine has come into

4"CIA Says Israel Has 10-20 A-Bombs," New York Times, March 16, 1976.

5"Tod aus der Textilfabrik," Der Spiegel, May 5, 1969.

6Cited in "Russians Report on Israeli Arms," New York Times, August 9, 1972. Additional independent press reports appeared in Al Ahram, November 23, 1973, and October 18, 1975; Boston Globe, July 31, 1975; United Press International wire reports published February 14, 1976, and November 25, 1976; New York Times, July 18, 1970; and November 25, 1976; New York Times, July 18, 1970, and October 5, 1971; Washington Post, March 16, 1976; Jane's Aircraft 1972-73, p. 565; SIPRI Yearbook 1972, p. 312; Robert J. Pranger and Dale R. Tahtinen, Nuclear Threat in the Middle East (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1975); and London Sunday Times Insight Team, The Yom Kippur War (Garden City, N.Y.: Doubleday, 1974), p. 282. The leading work on the subject is still an International Institute for Strategic Studies monograph by Fuad Jabber, Israel and Nuclear Weapons (London: Chatto and Windus, 1971) which concluded that "declaratory policy aside, Israel has gone steadily ahead in the development of the capacity to build atomic bombs," p. 123.

7"Cairo Editor Urges Nuclear Arms for Arabs," New York Times, November 24, 1973, and "Israel Has Nuclear Arsenal, Egyptian Journalist Claims," Los Angeles Times, October 19, 1975.

⁸Quoted in New York Times, April 4, 1975.

possession of information that 13 warheads were "hastily assembled at a secret underground tunnel during the 78-hour period at the start of the 1973 October War [and] ... were sent to desert arsenals where they remain today, still ready for use."9

Many of these reports of Israeli nuclear weapons appear to be nothing more than chains of conjecture and unsubstantiated rumors, often drawing upon other reports based on equally slender evidence. One well-informed U.S. government official told this writer that even the Central Intelligence Agency "doesn't really know, but is just guessing like everybody else." The fact that many observers find the reports credible may have more to do with the perceptual frames underlying images of Israel than with "the facts" themselves. People believe that Israel is obsessed by a complex of insecurity, living under an ambient state of siege. Jewish science is resourceful, and Israel has an impressive technological base for a small country. It might be impossible for a Jewish government, some of whose own members are concentration camp survivors, not to take at least the preliminary steps to a "last resort" deterrent, given the widespread belief that the Arabs are bound to get stronger in the future. The secrecy that Israel has maintained around the Dimona reactor naturally has encouraged suspicions and speculation. Even in Israel, according to a March 1976 poll, 62 percent of the people believe that their country already possesses atomic arms while only 4 percent think it does not, and 77 percent think that it should. 10 Moshe Dayan has openly called for developing nuclear weapons because, in the long run, Israel cannot hope to match the quantities of conventional weapons that the Arabs can obtain, 11 and because of America's current weakness and the limitations of its mediating role.12

The widespread belief that Israel has nuclear weapons seems to have spurred consideration on the part of Egypt and other Arab states as to whether they should pursue the nuclear option also. As early as October of 1965, Heikal argued that it might be necessary for Egypt to build a nuclear capability to deter an Israeli atomic threat.¹³ In 1970, Colonel Qadaffi sent the second man in Libya, Major Jalloud, to buy a tactical atomic weapon from Chou En-lai, but he was sent home with the message that every nation must practice self-reliance and that atomic bombs were not for sale.14 Sadat himself said in December of 1974, "We shall also find a way of having atomic weapons,"15 and in August of 1975 he formed a top level council headed by the president and including the minister of war, the foreign minister, and the former head of the air force to oversee nuclear development for "all purposes." 16 In May of 1977, President Assad told a group of British journalists that if Israel had nuclear weapons, the Syrians also would acquire them.17

In terms of capabilities, several Arab states are now on the threshold of a vast expansion of programs for civilian applications of atomic energy, including the acquisition of reactors for power plants and the training of large numbers of personnel in nuclear technologies. Inevitably, civilian programs reduce the lead-time for the development of nuclear explosives. Egypt has announced plans to set up 10 reactors over the coming 20 years to supply an aggregate 10,000 megawatts of power by the year 2000. The first two reactors, rated at 1,200 MW, have already been negotiated with Westinghouse of

^{9&}quot;How Israel Got the Bomb: Special Report," Time Magazine, April 12, 1976. Twelve months later, in a less highly publicized survey of the "Nuclear World," Time quietly demoted Israel to the class of countries which might achieve a "bomb capability within a few years." May 2, 1977, p. 13.

^{10&}quot;Most Israelis Want N-Weapons," Reuters report carried in Egyptian Gazette, March 27, 1976.

¹¹Quoted in Jerusalem Post Weekly, March 30, 1976.

¹²In a press conference in Paris on March 19, 1976, and in a lecture in Tel Aviv on March 29, 1976, quoted in Shlomo Aronson, "Nuclearization of the Middle East: A Dovish View," Jerusalem Quarterly, no. 2 (Winter 1977), p. 36. Dayan was out of the government at the time of these statements, and Prime Minister Rabin specifically disavowed any governmen-

tal responsibility for such statements made by "Knesset members." Quoted in "No Change in Policy," Australian Jewish Times, April 15, 1976.

¹³Al Ahram, October 15, 1965. Quoted in Yair Evron, "The Arab Position in the Nuclear Field: A Study of Policies up to 1967," Cooperation and Conflict, no. 1 (1973), pp. 19-31.

¹⁴Reported in Mohammed Heikal, The Road to Ramadan (London: Collins, 1975), pp. 76-77.

^{15&}quot;Mideast Ready to Explode, Sadat Says," New York Times, December 17, 1974. See also, "Fahmy Warns: Egypt Would Make A-Bomb," Jerusalem Post Weekly, June 18, 1974.

^{16&}quot;Egypt's Nuclear Body," Canberra Times, August 11, 1975.

¹⁷Quoted in "New War Threat," Australian Jewish Times, May 12, 1977.

^{18&}quot;Egypt Pushes A-Power Ambition," Christian Science Monitor, December 22, 1975, and "Middle East Planners Push for Nuclear Energy Development," Middle East Economic Digest, 19 (November 21, 1975), 11, 12.

the U.S.¹⁹ (By way of comparison, Israel's Dimona reactor, which is the subject of most of the speculative reports on atomic weapons, is rated at only 24 MW.) Egypt is also cooperating with Iran on nuclear projects.²⁰ Libya is getting a 10 MW "atomic center" from the USSR,²¹ and a 600 MW nuclear power plant from France.²² India, West Germany, and Sweden have also shown interest in helping Colonel Qadaffi's government with its civilian nuclear program.²³ Iraq is getting a 900 MW reactor from France and technical aid in the nuclear field from Italy.²⁴ Jordanian officials have announced that their country possesses between 200,000 and 300,000 tons of uranium ore in its known phosphate reserves.²⁵

Nevertheless, the Arab civil programs should not necessarily be considered evidence of incipient nuclear weapons development. Most of the reactor agreements announced will not result in completed projects until the mid-1980s, and upon completion all announced facilities will be subject to thorough inspection. For example, the American reactors going to Egypt, according to a statement by Secretary Kissinger, will be "under the most exhaustive safeguards in existence in any country."26 Moreover, even if one or more Arab states came into the possession of fissile materials enriched to weapons grade (and there would be many obstacles to reaching this point), the capacity of such low technology states to fabricate bombs is doubted by some nuclear physicists. While many fabrication technologies can be inferred from open literature,27 actual produc-

19"Westinghouse to Supply Nuclear Reactor," Middle East Economic Digest, 20 (May 21, 1976), 13.

tion from these plans would require sophisticated facilities, equipment, and materials that are presently available from relatively few suppliers in the high technology states and whose export is controlled by a "trigger list" signaling military nuclear intent. It would be much more difficult for a Libya to conceal a clandestine program than for a middle technology state like India or Israel. ²⁸ By way of illustration, one manifestation of the difference can be seen in the fact that Israel had 2,850 publishing scientists in 1975 compared to a total of 1,192 for all 13 major states of the Arab League, ²⁹ and this ratio of 2.4:1 has remained essentially unchanged since 1967. ³⁰

However, Arab nuclear development might not depend on indigenous capabilities. India, which is now running a trade deficit with the Middle East in excess of \$600 million per year. has signed agreements for the transfer of nuclear technology with Egypt, Iraq, Syria, and Libya.31 India is sympathetic to the Arab cause, is seeking closer relations with some moderate Arab regimes, and might look upon military nuclear assistance as redressing an imbalance caused by an already nuclear Israel. The last argument might become more persuasive if Israel were perceived to be rattling nuclear sabres at its neighbors. Taiwan, Korea, and Pakistan may also become potential "supplier" states in the future.

In the net assessment, many professional observers and proliferation scholars have arrived at the conclusion that, despite denials and technical problems whose solutions are not publicly known, Israel has nuclear weapons and the Arabs will also have them within 10 to 15 years.³² If the Arab-Israeli conflict is trans-

^{20&}quot;Cooperation on Nuclear Projects," Middle East Economic Digest, 20 (June 25, 1976), 13.

^{21&}quot;France to Build Nuclear Power Plant," Middle East Economic Digest, 20 (March 26, 1976), 32.

²²"France is to Build Libyan Atom Plant," New York Times, March 23, 1976, 9.

 ²³Christian Science Monitor, December 22, 1975.
 ²⁴"Italy Signs Atomic Energy Protocol," Middle East Economic Digest, 20 (January 23, 1976), 21.

^{25&}quot;Uranium Deposits Believed Important," Middle East Economic Digest, 20 (March 12, 1976), 15.

²⁶Quoted in "Egypt Pushes A-Bomb Ambition," Christian Science Monitor, December 22, 1975. On the general subject of American controls, see U.S. Foreign Policy and the Export of Nuclear Technology to the Middle East, Hearings before Subcommittees of the U.S. House of Representatives, Committee on Foreign Affairs (Washington, D.C.: Government Printing Office, 1974).

²⁷See, for example, Mason Willrich and Theodore B. Taylor, *Nuclear Theft: Risks and Safeguards* (Cambridge, Mass.: Ballinger, 1974).

²⁸I am indebted to Professor Carlo Schaerf of the Department of Physics, University of Rome, for calling these problems to my attention.

²⁹Who is Publishing in Science? (Philadelphia: Institute for Scientific Information, 1976) tallied from indexes.

³⁰Compare A. B. Zahlan, "The Science and Technology Gap in the Arab-Israeli Conflict," *Journal of Palestine Studies*, 1, no. 3 (Spring 1972), 22.

³¹David Shirreff, "India Struggles to Close Trade Gap," *Middle East Economic Digest*, 20 (February 13, 1976), 2-3, 34-35.

³²Fuad Jabber estimated in 1971 that the lead time for a crash Egyptian program would be no more than seven to eight years (p. 141). A team of scientists including Henry W. Kendall, Professor of Physics at MIT, George B. Kistiakowsky, Emeritus Professor of Physical Chemistry at Harvard, Daniel Ford, Executive Director of the Union of Concerned Scientists, and George Rathjens, Professor of Political Science at MIT told the House Committee on Foreign Affairs in 1974 that the minimum time required for Egypt to build

formed to a nuclear stage, we will need a conceptual apparatus to comprehend the first regional "balance of terror" outside the central European theatre. The purpose of the present analysis is to explore the consequences of the introduction of nuclear weapons in the Middle East, should it come about, and particularly to examine the possibility of a regional balance of terror as a path to stabilization. The central hypothesis is that apocalyptic visions and doomsday images have been accepted too readily and out of proportion to the arguments that are given. A stable system of mutual deterrence may be viable in the Middle East and may make a positive contribution to the political deescalation of the Arab-Israeli conflict.

Nuclear Balance, Conventional Balance, and Political Settlement

Stabilization through regional nuclear deterrence is sometimes contrasted to two other approaches to stability that are said to be conceptually distinct: a negotiated politica! settlement or a "negative peace" based on containment by conventional military forces. However, it will be argued here that political settlement, conventional balance, and nuclear balance are not competitive approaches but in fact are synergistic. A stable balance of forces is not an alternative to a political settlement but more often a precondition. Containment leads to settlement when, after a long period during which potentially aggressive states are held at bay by their would-be victims, they eventually moderate their demands and accept a compromise. The Helsinki accords, for example, merely recorded the negative peace that had prevailed for 25 years in Central Eugope. They were based not on a perception in either camp that the final postwar lines of division were equitable or optimal, but simply on a universal belief on both sides that no change in the status quo could be achieved by force of arms at an acceptable cost. Moreover, the need for a stable military balance does not terminate when an agreement is signed. Where conflicting objectives are a permanent condition, as in Central Europe and the Middle East, the continuing viability of the negotiated settlement itself often depends on an environment of military stability.

Even if the need for a military balance is

nuclear weapons would be six to ten years after initiating a large scale civilian nuclear reactor program. See U.S. Foreign Policy and the Export of Nuclear Technology to the Middle East, pp. 182-83.

accepted, the relationship between reliance on conventional forces and resort to nuclear deterrence is controversial. Some have argued that nuclearization in the Middle East would be redundant (i.e., that there is a stable relationship of conventional forces), would introduce new instabilities, and would divert budgetary resources away from conventional arms. However, there are reasons to doubt that a balance can be maintained in the Arab-Israeli region similar to that between India and Pakistan, North and South Korea, Iraq and Iran, or China and Taiwan by conventional means alone. First, conventional balance depends critically on a number of independent variables, each of which is highly volatile, including (1) a finely tuned management of arms flow into the region from many independent sources of supply available unequally to the two sides; (2) harmonizing the impact of multiple technological innovations introduced at a rapid rate; (3) controlling for the shift and flux of regional alliances and interference by the great powers; and (4) managing the effects of highly unequal rates of economic development. A conventional balancer in a region of such fluid and revolutionary patterns has the problems of an acrobat on a high wire in a swirling windstorm: in principle it can be done, but only with supreme skill and exertion.

Moreover, the problem of creating a stable conventional military balance in the Middle East is compounded by the special nature of perceptions in this region. Abba Eban described the perceptual element from Israel's point of view succinctly in 1965, two wars ago: "We want to create doubt, and eventually resignation and despair [in the Arab mind] about the dream of eliminating Israel from the world's map." But Israel has now decisively won four wars, the most recent under worst-case conditions, and yet the Arabs are far from "resignation and despair" and indeed some seem ready to contemplate a fifth, sixth, nth round, rolling indefinitely into the future. 34

³³Jewish Observer and Middle East Review, July 2, 1965. Quoted in Jabber, p. 104.

34For example, Lt. Col. Haytham Al-Ayubi, head of the military research department of the Palestine Research Center (Beirut): "The fifth round will not be the last round. There will be a sixth, seventh, eighth ... until both sides finally realize that the only solution lies in setting up a [secular] democratic state..." "Is War Inevitable?" translated from the Beirut weekly Monday Morning by Maha Samara in Atlas, January 1975, p. 36. Similar though less extreme views were expressed to the present author during visits with officials and scholars in Cairo and Damascus during October/November 1975 and February 1977.

The failure of the containment theory is explained by some Israelis in terms of a kind of Arab madness, an endless jihad in which there is no concept of ends-means rationality nor any calculation of the relationship between actions and their consequences.35 Others explain it as an attempt to externalize the contradictions within Arab societies by demonizing Israel and maintaining a permanent state of hostility. But several "rational actor" explanations deserve attention before resort is made to socio- or psychopathological analysis. First, past defeats are not necessarily indicators of the future military balance. Unless one assumes a racialist theory of Arab inferiority, tiny Israel's past victories against the populous Arab world tend to be perceived as "unnatural" events to be explained by special circumstances. Heikal echoed a widely held view when he said that

a military or political balance between the 100 million Arabs and the 3 million Israelis cannot be kept forever. Perhaps the Soviet Union and China—300 million to 800 million—can preserve a balance of a sort in spite of the disparity of numbers, but a ratio of 100 to 3 is absurd. With modern technology, the population gap becomes more significant. 36

Arab observers explain Israel's past victories in terms of special factors in each case, such as the intervention of great powers in Israel's behalf (1956), Israel's success in catching the Arabs unprepared (1967), the failure of the Arab planners fully to comprehend and exploit their advantages at critical moments (October 6–9, 1973), and above all, the technological gap in equipment and manpower competence that has operated in Israel's favor since 1948. Most Arab strategists believe that, inevitably, the modernization of their countries will close the technological gap, and this will be reflected in a considerable alteration of combat loss ratios, relative manpower competence, and battle outcomes.

Moreover, Arab strategists can look to the example of other revolutionary movements which, after years of reverses and against seemingly hopeless odds, finally achieved their goals: Vietnam, Algeria, Angola, Mozambique, China, etc. In their own case, the revolution in oil prices might seem to signal the beginning of a process that is bound, over time, to alter the regional balance of power. OPEC oil revenues alone now exceed \$100 billion per year, while Israel's GNP is only slightly in excess of \$10 billion. Israel does not have the resources to

continue the present rate of acceleration in military expenditures for long, while the Arab coalition, if it can maintain some degree of cohesion, could, in principle, double and redouble its military expenditures again. Israel is increasingly dependent on the United States, and Washington must weigh its interests in this small country against the strategic importance of the Arab world. Diplomatically, Israel is increasingly isolated and in a corner, and time does not seem to be on her side. It would not be unreasonable for an Arab strategist to conclude, from this ordering of the facts, that the wind of history is behind the Arab sail, and that the trends in the balance of power are bound to favor the Arabs if only they have the patience to let these processes mature.37

The final problem of the conventional balance of power as an approach to stability is its assumption that states only initiate wars when they expect to achieve military successes on the battlefield, and that they do not fight when they expect to lose. But the Arabs have shown in 1973 that the political result of a war need not be a direct reflection of the simple military outcome. The purpose of active warfare, in other words, may not be to achieve movement in combat, but in diplomacy. Moreover, a sophisticated understanding of the diplomatic environment can insure that the battlefield losses remain within tolerable limits. The military moves by both sides are restrained by the superpowers, and if the battle really begins to go badly, a cease-fire can be imposed. Thus, even against a highly unfavorable balance of conventional power, the Arabs may have a viable war option at an acceptable level of cost to achieve defined political objectives.

For all these reasons, it appears that conventional containment alone cannot bring stability to the Middle East, even if the balance of forces can be equilibrated during rapid technological, economic, and political changes. However, the addition of a nuclear level of deterrence has the potential to compensate for the deficiencies of the conventional balance of power. Israel in particular may be drawn to the nuclear option to put a halt to what might otherwise be an

³⁵The issue of Arab rationality is taken up below, 36Heikal, *The Road to Ramadan*, p. 43.

³⁷This perception of the future military balance is presented here to illustrate why some Arab strategists believe the military balance to be tipping. It is not meant to reflect the present author's assessment—indeed, I believe that in spite of the disparity of numbers, the conventional military balance is likely to remain strongly in Israel's favor well into the future. See Rosen, "The Arab-Israeli Military Balance in the 1980's," Jerusalem Journal of International Relations, 4, no. 1 (Fall 1978) forthcoming, and my forthcoming book, Balance of Power: The Arab-Israeli Case.

unending series of small wars. Ironically, the case from Israel's point of view has been put forth most eloquently by an Arab-American scholar, Fuad Jabber:

Where conventional power has failed, weapons of mass destruction would be expected to succeed in convincing Arab populations first and their governments second of the futility of continuing their confrontation with Israel. With the realization that Israel cannot be militarily defeated, the rationale behind the permanent state of war, the economic blockade, and the policy of non-acceptance and non-recognition might be expected to break down. Moreover, whatever tendencies towards recognition and negotiations may already exist in the Arab world would be enormously strengthened. Hitherto, governments willing to negotiate had not dared to act because their position at home would have become untenable. In a nuclear context, the survival imperative might provide enough justification to make such approaches possible.... The psychologically erosive effects of the nuclear logic would be at work on the Arab will, gradually producing that pervasive feeling of "doubt and eventually resignation and despair about the dream of eliminating Israel from the world's map" that Israel's doctrine of deterrence had always sought to create. 38

In this way, a nuclear deterrent could enhance the stability of the balance of power by creating the *perception* of balance that conventional arms alone cannot produce.

Arab Perceptions of Israeli Nuclear Weapons

Empirical evidence on Arab perceptions of Israeli nuclear weapons is fairly thin, but the few indications that are available support the theory of a strong deterrent effect. For example, as early as 1961, Iraqi Colonel Hussan Mustafa wrote in a Baghdad daily that "the atomic weapons would serve as a permanent deterrent to the Arabs and would compel them to avoid provocations." Moreover, "Israel's acquisition of atomic arms will decrease the value of the ordinary weapons in the hands of the Arab armies and the importance of their numerical superiority." In early 1966, Ahmad Samih Khalidi, in "An Appraisal of the Arab-Israeli Military Balance," wrote that,

when Israel gets the atomic bomb, there will be a "stalemate." Egypt will also get atomic weapons "and then neither country will dare attack the other" and "the sands will have run out for the Arabs." Later in 1966, according to Abdullah Schleifer, the Syrian Left-Ba'athists called for immediate struggle against Israel because once Israel got the bomb, "none of the Arab armies, however strong by then, would even consider the idea of liberating Palestine at the risk of nuclear destruction..." 141

Recent Arab commentaries carry forward the same theme. For example, the pro-Arab London monthly, *The Middle East*, in a 1977 assessment asking "Can the Arabs go to War?" concluded that "since [nuclear weapons] were developed [by Israel] at the end of the 1960s, there has never been the realistic possibility of a total Arab victory. The Arab states simply could not afford to win to the extent of overrunning the heavily populated heartland of Israel." Arab military strategists do not seem to doubt that Israel would be driven to the nuclear "last resort" if the conventional shield were broken. For example, Haythem Al-Ayubi believes that

Israel might use nuclear weapons if the Arab armies penetrate beyond the Green Line [i.e., the pre-1967 boundary] with a force capable of inflicting a military defeat on Israel. The Zionists think that if Arab forces drive past the Green Line, this will lead to the annihilation of the Jewish population....43

Similarly, Riad Ashkar, a well-known strategist at the Institute for Palestine Studies, believes that "if Israel finds herself... unable to stop an Arab advance within the territory of Israel proper, and if it feels about to be destroyed, then Israel might think that the only way out for them is to resort to the use of nuclear weapons."44

These and similar statements support Jabber's view that a nuclear deterrent might succeed where conventional containment has failed, to being about the realization that neither the present balance of forces nor any

³⁸Jabber, pp. 146-47 and 133.

³⁹ From a series of articles in January 1961 in the Baghdad daily Al-Ahali written by a retired colonel. Translated in Avraham Ben-Tzur, "The Arabs and the Israeli Atom Reactor," New Outlook, 4 (March-April 1961), 21.

⁴⁰Ahmad Samih Khalidi, "An Appraisal of the Arab-Israeli Military Balance," Middle East Forum, 42, no. 3 (1966), 55, 63.

⁴¹See Abdullah Schleifer, *The Fall of Jerusalem* (New York: Monthly Review Press, 1972), 76.

^{42&}quot;Can the Arabs Go to War?" The Middle East, no. 29 (March 1977), 23.

^{43&}quot;Middle East: The Military Dimension," Journal of Palestine Studies, No. 16, 4, no. 4 (Summer 1975), 24.

⁴⁴Ibid., p. 11.

foreseeable future power alignment will offer a viable military option to destroy Israel. The existence of nuclear weapons in the region will induce moderation and a revolution of declining expectations in the Arab "street," as the end-of-the-world character of atomic war is understood by both mass and elite elements within the Arab world. It is possible that nuclearization will reduce the political appeal of militant factions within the Arab world and that the so-called "rejection front" of regimes and organizations refusing to have anything to do with Israel will lose all credibility. This is a "trickle-down" theory-the new restraint will be evident first among the top leadership, but eventually the moderating effects of the danger of annihilation will percolate through the entire Arab polity. Also, the great powers, and notably the Soviet Union, will have new incentives to restrain their Middle Eastern clients.

While there is no doubt that Israel's highly concentrated population would be vulnerable to massive damage from even a few nuclear weapons, a few observers have raised the objection that the much larger number and dispersion of targets in the Arab countries might reduce the deterrent effectiveness of a small Israeli nuclear force. However, the population of Egypt is concentrated in the upper third of the country, including Cairo, Alexandria, and the towns of the Nile Delta; a third of the Syrians live in Damascus, Aleppo, Homs, Hama, and Latakia; and Jordanian society is centered in Amman. No city of the Arab world would be immune to long-range strikes by the Israeli Air Force. It seems reasonable to assume that an Israeli deterrent capable of hitting five Arab capital cities would be quite sufficient to constitute an "unacceptable" level of damage.

Rationality

Another objection that is often raised in discussions of the nuclearization of the Middle East is the assertion that the politics of this region tend to irrational and spectacular acts. The Arab-Israeli conflict is characterized at times by intense and uncompromising fears and emotions, expressed in extreme utterances and bitter images. The practical issues are overlaid by elements of religious war and transcendent moral imperatives that some claim would justify the sacrifice of millions. The Arab side in particular is widely believed to have a tendency to emotionalism and loss of self-control. One study of the Arab mind, for example, points to "the frequency with which the flood of anger, violence, or other intense emotion sweeps over the dam of self-control and in an astonishingly

short time transforms the entire personality."⁴⁵ Poverty and frustration are so pervasive that there is a great deal of what Morroe Berger calls "free-floating hostility,"⁴⁶ and another observer finds that "conflict is so frequently on the verge of breaking out that personal relations seem to be directed at avoiding or covering up the slightest tendency toward the expression of difference."⁴⁷ Ciro Zoppo relates this directly to the problem of nuclearization:

Should Egypt and Israel travel the nuclear road to military options, the slender reed of rational calculation may not sustain, in a nuclear confrontation... the weight of the Arab quest for justice. Arab policies have in the past, on occasion, appeared suicidal.⁴⁸

Alan Dowty asks, "Can [advocates of nuclear stabilization] explain Nasser's behavior in the 1967 crisis as an exercise in rational policy-making?" 49

No strictly scientific and objective judgment is possible on the question of rationality, but there are some reasons to doubt the most alarming views. Nadav Safran has stated the case precisely:

Israelis and Arabs, like all people, act on the basis of an interplay between emotional considerations and coercive pressures.... A given degree of emotional commitment sets a threshold that coercive pressures must reach in order to induce "rational," that is to say, calculated behavior.... In the Arab-Israeli conflict, the degree of emotional commitment has been high, indeed. But this has not meant that either side was emotionally "incapable" of yielding, but that efforts by one party to affect the behavior of the other necessitated the marshalling of vast means of pressure. Although individuals sometimes "crack up" or commit suicide rather than submit to the pressures of reality, in the case of nations this has seldom if ever happened. 50

Past perceptions of the Arab states in particular may have been clouded by the climate of

⁴⁵Raphael Pitai, The Arab Mind (New York: Charles Scribner's Sons, 1973), p. 160. See also Sania Hamady, Temperament and Character of the Arabs (New York: Twayne, 1960), pp. 205, 211, 222.

⁴⁶Morroe Berger, Conflict in the Middle East (London: Allen and Unwin, 1971), quoted in John Laffin, The Arab Mind Considered: A Need for Understanding (New York: Taplinger, 1975), p. 108.

⁴⁷Ibid.

⁴⁸Ciro Zoppo, "The Nuclear Genie in the Middle East," New Outlook, February 1975, p. 24.

⁴⁹Alan Dowty, "Nuclear Proliferation: The Israeli Case," *International Studies Quarterly* (forthcoming).

⁵⁰ Nadav Safran, From War to War (New York: Pegasus, 1969), p. xvi.

intense hostility and by the lack of normal intercourse with Israel and the West, Comparison with the case of China suggests that an adversary whose behavior once seemed irrational from a distance may look quite restrained after relations are improved and more objective and detailed information becomes available. Future studies of Arab policy may find, retrospectively, a consistent and rational pattern of behavior that was not perceived earlier. Nasser's 1967 miscalculation in Sinai may look no worse than Truman's misassessment of China's willingness to intervene in Korea, or Kennedy's groundless expectation that the Bay of Pigs incursion would trigger a popular uprising against Castro. Casual statements about "crazy states" may come to be seen as simplistic and even racist as applied to the Arabs, whose behavior seems to this observer to have exhibited the same care in assessing the costs and benefits of alternative policies as that of the great powers. And, as the risks of miscalculation escalate, greater restraint will be evident.

If rationality is not assumed, the prospect for the Middle East is dire with or without nuclear weapons. For example, the scarce water systems of the region depend on a highly developed network of reservoirs, pumping stations, and water carriers. Many elements in this fragile net would be vulnerable to conventional explosives. Still more vexing is the possibility of a modern equivalent of the classic tactic of the poisoning of wells, perhaps in the form of chemical or bacteriological warfare through the water supply. Small canisters of advanced toxic agents could be lobbed into reservoirs by a variety of means, and existing filtration systems would not be effective to protect the civilian populations. While each state has taken some precautions to defend its storage and delivery installations, there is no absolute defense in this sphere. Security depends on mutual offensive threats-i.e., on deterrent threats posed by each state against the other-a hydrological version of mutual assured destruction. The fact that rational interest has restrained total warfare in this field should give some encouragement as to the prospect of a rational nuclear balance.

If rationality is assumed, there are still quite a number of problems for the design of a stable system of nuclear deterrence in the Middle East, including: (1) securing second-strike forces against pre-emptive counterforce attacks; (2) controlling escalatory tendencies of tensions, incidents, and even subnuclear wars that may continue to occur; (3) preventing unauthorized use of nuclear weapons by over-

zealous subordinates; (4) preventing accidents; (5) preventing seizure of weapons by terrorist groups; and (6) controlling the disclosure process during which the introduction of atomic weapons into the region becomes publicly known, so as not to upset the delicate balance of forces in the area. Key problems in this list are taken up serially below.

Second-Strike Forces

The protection of a second-strike retaliatory force from destruction by an incoming counterforce surprise attack is a classic design problem for stable deterrence. The potential aggressor must believe that it will suffer retaliation at an unacceptable cost, and this means above all that some portion of the defending state's strike force must survive the initial assault. Of the many approaches to the problem of protecting the retaliatory force, 51 only a few are promising for the Middle East.

1. Launch-Under-Attack. Unprotected aircraft or missiles could be launched as soon as an incoming attack is detected. To avoid the possibility of catastrophic error, this would have to mean that unambiguous signals of an enemy attack have been received-in practice, that their nuclear-capable aircraft or missiles have crossed an explicit "red line." Given the extremely short flight times of Mach 2 bombers and surface-to-surface missiles in the Middle East-generally from 5 to 15 minutes on the most direct routes⁵²—the timing would be such that an almost instantaneous decision would be necessary. This had led some, notably Ciro Zoppo, to conclude that a Middle East balance would be "one of the most unstable that one can imagine ... a hair-trigger situation with strong urges to pre-empt."⁵³ Clearly, a stable balance of terror cannot depend primarily on a launch-under-attack system. However, as noted by John B. Walsh, Deputy Director, Strategic and Space Systems, DDR&E, launch-underattack is

a very sound augmentation to any other survivability scheme, and hence, I think we should

⁵¹Warner Schilling has described nine ways of dealing with the fixed site ICBMs of the superpowers. See Schilling et al., American Aims and a Changing Europe: Dilemmas of Deterrence and Disarmament (New York: Columbia University Press, 1973), pp. 25–26.

⁵²See Steven J. Rosen, *Military Geography and the Military Balance in the Arab-Israeli Conflict* (Jerusalem: Hebrew University, 1977), Jerusalem Papers on Peace Problems, no. 21.

⁵³Zoppo, p. 24.

never deny ourselves this capability. Moreover, we should always make it clear that such a capability is inherent in our systems and we are willing to exercise it. I think it is a strong deterrent capability in conjunction with other systems and can have some capability of its own.⁵⁴

It is not inconceivable that this option will have a deterrent effect in the Middle East as well.

2. Continuous Circulation. This can take the form of either random occupancy of a large area on land, sea, or air or a crisis-oriented system in which the launch vehicles scatter on warning. This system faces the problem of avoiding detection by the increasingly sophisticated surveillance systems operated over the Middle East by the superpowers and, more recently, by the local states themselves. Israel now has the Hawkeye E-2C Airborne Early Warning System with an APS-125 overground radar which can track as many as 300 vehicles at a time out to a radius of 250 miles and can monitor intercepts of up to 30 targets, in a surveillance cylinder from the surface up to 100,000 feet. 55 Continuous circulation will not be a high-confidence survival mode for the Arabs in the future, though it may have some possibilities for Israel until the Arabs get a Hawkeye equivalent or real-time satellite data transmission from a superpower. 56

3. Hardened Silos and Aircraft Shelters. The superpowers protect their land-based ICBMs in reinforced concrete silos hardened from 300 to 900 psi; Sweden keeps some of its Viggen aircraft in underground caverns carved out of rock in remote areas adjacent to highways suitable for takeoff (similar to the Swiss "underground aircraft carriers"); and Egypt has constructed over 450 reinforced concrete shelters up to 10 feet thick to protect its combat aircraft on the ground.⁵⁷ However, a ballistic

or cruise missile or glide bomb with a circular error probability of less than 150 feet and a warhead of 20 kilotons would have a kill probability greater than 97 percent even against a target superhardened to withstand 1,000 psi overpressures. 58 And a wide array of precision guidance technologies are now becoming available with accuracies better than 150 feet, including laser designation, electro-optical, infrared, and radar seekers, radar area correlation, distance measuring equipment, microwave radiometers, and satellite position fixing.⁵⁹ With accuracies down to ten feet, it may be possible to destroy many hard targets even without nuclear warheads by using fuel air explosives.60 While the most advanced precision guidance technologies have not yet been introduced into the Middle East. Israel has reported error distances of only 200 to 300 feet in test firings of its (nuclear-capable) Lance missiles;61 it produces its own air-to-air (Shafrir), antishipping (Gabriel), and air-to-surface (Luz-1) missiles; and it is believed to have the electronics know-how to produce more sophisticated guidance packages in the future. While Egypt's past efforts in the missile field were not successful,62 the new large-scale Arab military industries to be built in Egypt with Saudi aid will have a large element of French participation.63 Moreover, European and American guidance packages may become available to the Arabs as off-the-shelf technology. Overall, terminally guided PGMs are quite likely to become part of the "normal" inventories of Middle

⁵⁴Testimony in Hearings before the Senate Armed Services Committee, March 19, 1976, Fiscal Year 1977 Authorization for Military Procurement ..., Part 2, Research and Development (Washington, D.C.: Government Printing Office, 1976), p. 6257.

^{55&}quot;Airborne Early Warning for the U.S. Navy," International Defense Review, 8, no. 5 (October 1975), 679–82; and Philip J. Klass, "E-2C Radar to Provide New Flexibility," Aviation Week and Space Technology, July 12, 1976, p. 51.

⁵⁶This discussion of ontions is an extensive recon-

Scientific Effect of the October 1973 War: Modification of Planning-Construction and Maintenance and Repair of Military Airports," paper delivered to the International Symposium on the October 1973 War, Cairo, October 1975; and Steven Rosen and Martin Indyk, "The Temptation to Pre-Empt in a Fifth Arab-Israeli War," Orbis, 20 (Summer 1976), 279–80.

⁵⁸Based on calculations from Kosta Tsipis, Offensive Missiles, Stockholm Papers no. 5 (Stockholm: Stockholm International Peace Research Institute, 1974) and other sources; see Rosen, "Nuclearization...," pp. 166–68.

⁵⁹ See Richard Burt, New Weapons Technologies: Debate and Directions, Adelphi Paper no. 126 (London: International Institute for Strategic Studies, 1976), esp. p. 3.

⁶⁰See Steven Rosen and Desmond Ball, "Fuel Air Explosives for Medium Powers," Pacific Defence

Eastern powers within a decade, and they will render obsolete the protection of second-strike forces in hardened sites whose location is known. Hardening will be a viable survivability option only in the near-term.⁶⁴

4. Protective Concealment and Dispersal. One of the most promising approaches to survivability in the Middle East, particularly for Arab states with large open areas, is concealing the location of weapons-bearing aircraft and missiles (possibly including the use of camouflage and decoys). The blast overpressure resistance of an unhardened missile on a gantry is estimated at 2 psi; a combat aircraft on the ground is disabled at 3 to 5 psi. The maximum overpressures generated by a 20-kiloton warhead detonated at optimum burst height would drop below 2 psi at distances greater than 2.3 miles from ground zero; that of a 1-megaton weapon at 7 miles. If we assume a random occupancy pattern for second-strike missiles and aircraft on the ground, and that the incoming first-strike force consists of fewer than 50 warheads, the aggressor would have little chance of achieving a disarming counterforce first-strike provided it did not know which vehicles carried the nuclear strike forces and provided that the area of dispersion was large. Moreover, it would not know if addi-

64However, medium-range surface-to-surface missile systems presently available in the Middle East in 1977 do not possess counterforce-capable CEPs. Israel's Jericho (range 280–300 miles) is said to have guidance problems (Insight Team, p. 283) and has been rated as accurate within one kilometer (3280 feet). See Le Monde, April 25, 1968; and Jabber, Israel and Nuclear Weapons, p. 96. The Arabs' Scud-B (range 180–200 miles) is reported in one study as having an error probability of 1300 feet, and the unguided Frog-7 is known to be inferior to the Scud. Robert E. Harkavy, "The Strategic and Diplomatic Implications of the Israeli Nuclear Weapons Program" (Mimeo, Kalamazoo College, Michigan, 1974), p. 39. A later draft of this paper is to appear as Israel's Nuclear Weapons: Spectre of Holocaust in the Middle East. Monograph Series in World Affairs (Denver: University of Denver Press, 1977). The Pershing (range 460 miles) promised to Israel during the Sinai II negotiations (but not delivered) has accuracy problems so severe that a Pershing II with radar area correlation guidance is reported to be under development to correct this deficiency (Jane's All the World's Weapons Systems 1976, p. 42.) The Soviet Scaleboard SS-12 (range 400–500 miles), reported in the Middle East Economic Digest of June 11, 1976, to be under delivery to Syria, appears to derive from the SS-13 and SS-14, which are regarded as low accuracy missiles. See Doug Richardson, "Soviet Strategic Nuclear Rockets Guide," Flight International, December 11, 1976, p. 1732. Of presently available systems, the Lance is the most accurate but has a limited range (50–75 miles) and a CEP well in excess of 150 feet.

tional second-strike forces were already airborne or at sea at the time of its attack.

A caveat should be added here, on the additional problems of concealment if the adversary possesses the means to detect and track the fissile material in the warheads during peacetime. The unclassified literature on surveillance and verification techniques generally indicates that peacetime detection of warhead movements depends on photographic, infrared, radar, and radio wave means of detection which could be evaded, and that neutron, gamma-ray, and X-ray sensors, which would be more difficult to counter, are effective only after the warheads are detonated.65 However, a few published and unpublished sources speculate that ultrasensitive surveillance equipment may be able to detect the tiny amounts of radiation that normally escape the shielding of atomic weapons in storage. 66 If this is so, a survivability system depending on concealment and dispersal may require proliferated decoys emitting low-intensity radioactivity with signatures comparable to those of atomic warheads, to saturate the enemy target acquisition system with fakes. For states able to develop atomic weapons, the fabrication of such devices should be relatively simple. Overall, concealment and dispersal is the most promising low-cost alternative.

5. Land-mobile Multiple Aim Point Shelter System. The survivability system for land-based ICBMs that the United States seems to be moving to adopt when improvements in Soviet delivery vehicle accuracy render existing silos obsolete is a system in which missiles mounted on transporter-launchers "would move from one relatively hard shelter to another within a complex." It is a deception game, essentially a pea-and-shell game, in which "the attacker would have to target all of the shelters" within

65See Herbert Scoville, Jr., "A Leap Forward in Verification," in Mason Willrich and John Rhinelander, eds., SALT, The Moscow Agreements, and Beyond (New York: Free Press, 1974), pp. 160-82; and U.S. Arms Control and Disarmament Agency Publication 85, Verification: The Critical Element of Arms Control (Washington, D.C.: Government Printing Office, 1976).

66See John W. R. Taylor and David Monday, Spies in the Sky (New York: Scribner's Sons, 1972), p. 117; and Desmond J. Ball and Edwin Coleman, "Alternative Missile Basing: Analysis of a Land-Mobile ICBM System," Survival, 19, no. 4 (July/August 1977). Shlomo Aronson has called attention to reports that American monitoring stations were able to detect the nuclear "radiating freight" of Soviet ships headed for Alexandria during the October War, picking up the "low radiation" by "highly sensitive, secret American equipment" (privately circulated communication).

... ...

every complex, "since he would not know in which shelters the missiles were [actually] deployed."67 With a limited number of attack missiles, they could not hit all the shelters. As described by Desmond Ball and Edwin Coleman in their "Tootle" version, the system consists of a series of semi-hardened shelters (about 125 psi) in concentric circles linked by roads to a central station where the missile transporter is based. The shelters are so spaced that no two of them can be taken out by a single enemy warhead, and the distances are such that the missile can be moved from the central station to a randomly selected shelter when warning of the enemy attack is received and before the incoming warheads impact. It is a time-urgent system, but one without the catastrophic error potential of the launch-on-attack system. The critical questions for adaptation of this system to the Middle East are the shorter warning times and the land requirements and cost of the system. Here, the assumed warning time is as little as five minutes (for the sites closest to the enemy border) but the missile system and its transporter are lighter (relatively short range rather than intercontinental missiles) and therefore can be moved more rapidly and developed at lower cost. If we assume a five-minute warning time, 200 kiloton warheads on the attacking missiles, and the development of a 40 mph transporter, a ten-shelter "Tootle" could be built in a circle with a diameter of six miles. Israel could space a very large number of such systems (possibly including dummies) in the vast undeveloped areas of Negev and northern Sinai, and the areas available to Egypt, Syria, and Jordan are many times as large. Ball and Coleman estimate the cost of a U.S. 1,000 ICBM, 10,000 shelter system at \$15 to \$30 billion, including research and development, shelters, ICBMs, transporter/launchers, roads, and command and control installations. If we make the simplifying assumption that the Middle East unit cost would be identical (in fact, it would probably be lower with smaller missiles and shelters, lighter launchers, and shorter and less heavily surfaced roads), a 25 missile, 250 shelter land-mobile multiple aim point shelter system in this area would cost approximately \$375 to \$750 million. By way of comparison, Israel's first 25 F-15s cost \$675 million, though like the shelter system their cost can be amortized over a decade or more. In summary, a "Tootle" solution is another viable (albeit

67See Secretary James Schlesinger, Annual Defense Department Report FY 1976 and FY 1977 (Washington, D.C.: Government Printing Office, 1975), pp. II-28.

more expensive) option for the Middle East. 68 6. Other Survivability Options. The above five options do not exhaust the possibilities. Other concepts examined by American strategists include off road mobile, inland waterway, rail mobile, hard capsule, revetted rail, pool, pool/canal, canal, covered trench, buried trench, hard rock tunnel, and soil tunnel, 69 in addition to various submarines, surface ships, and air platforms. The purpose here has been to indicate the range of options rather than to provide an exhaustive analysis of each possibility. But the examples developed should be sufficient to establish that the technical problems of a stable second-strike system of mutual assured destruction are solvable in the Middle East.70

Controlling Escalation of Crises

Another design problem for a stable balance of terror in a region of continuing hostility will be to raise the nuclear "firebreak" as high as possible should fighting break out and to control the escalatory tendencies of subnuclear crises. Low-intensity threats, such as border incidents, commando raids, fedayeen infiltration, and Israeli reprisals would not be likely to cross the nuclear threshold, but full-scale conventional wars in the Sinai, Golan, and the West Bank would raise incalculable dangers of uncontrolled escalation. If Israel remains in control of a substantial part of these territories, it is not inconceivable that the Arab states might mount an October-scale assault in defiance of the Israeli nuclear deterrent, on the reasoning that Israel's nuclear weapons are usable only as a

68See Ball and Coleman. The present analysis assumes that the movement of the transporter from its station to the randomly selected shelter will not be tracked by enemy radars or infrared or electro-optical sensors. The use of smoke, decoys, or other protective measures might be required.

69See testimony of Colonel Vigil W. Munsey in Hearings Before the Senate Armed Services Committee, Fiscal Year 1975 Authorization for Military Procurement . . . April 2, 1974 Part 6 (Washington, D.C.: Government Printing Office, 1974), p. 3347.

70Fuad Jabber points out that, even without sophisticated second-strike defenses, no first-strike could be counted on to destroy all the nuclear weapons available to the other side and, given the geographic concentration of Middle East populations, even one or two remaining warheads would cause what may be considered unacceptable damage to the initiator of a nuclear exchange. Jabber, Israel's Nuclear Option and U.S. Arms Control Policies, California Seminar on Arms Control and Foreign Policy research paper no. 9 (Los Angeles: Crescent Publications, 1972), p. 34; and Jabber, Israel and Nuclear Weapons, Chapter 11.

last-resort to guarantee the ultimate survival of the state and not for such secondary objectives as holding Hebron, Sharm el-Sheikh, or Mount Hermon. If so, strategic nuclear weapons in themselves would not halt the endless round of border wars in the peripheral areas. Indeed, the more stable the relationship of strategic nuclear deterrence, the wider the scope for subnuclear provocations without fear of escalation.

Nor would Israeli evacuation of all the territories held since 1967, as advocated in many settlement proposals, eliminate the problem entirely. Most of these proposals anticipate that the relinquished territories would remain demilitarized areas or limited forces zones, so that they would not subsequently become the launching points for future Arab attacks against Israel. It is entirely conceivable, however, that sometime after the agreements establishing such a regime, successor Arab governments in Egypt, Syria, and Jordan, or a PLO-originated government in the West Bank, would choose to reintroduce large-scale forces into the demilitarized areas, whether to resume the war against Israel, or in defense against perceived Israeli threats, or merely to assert the prerogatives of sovereignty over territory. In such a case, Israel might feel compelled to initiate a conventional preventive war in the territories, to stop the buildup of Arab forces in areas proximate to the Israeli heartland and to liberate Israel from what might be seen as another "stranglehold of aggression."

Large-scale conventional wars in the 1967 territories could challenge the restraining codes of nuclear conduct that are essential to a balance of terror. If, for example, an Egyptian force broke through the Sinai passes and hill chains and reached as far as the Abu Aweigila triangle of northeastern Sinai, Israel would not know whether this unrestricted advance of Arab armies flushed with victory would stop at the Green Line or penetrate into the Jewish state itself. Similarly, an Israeli advance into the Damascus plains past the Sassa lava ridges or toward the recapture of Hebron might induce the Arab states to consider the nuclear option. Any scenario involving full-scale mobilization or conventional war must assume that there would be a tense nuclear alert, with unprecedented dangers of miscalculation. The world has not yet seen a full-scale conventional war involving directly opposed armies of two nuclear-armed parties.

The introduction of tactical nuclear weapons could alter this picture substantially, both in relation to the correlation of forces in the affected theatres and in relation to the exten-

sion of atomic deterrence from the heartlands to the territories. Usable theatre nuclear weapons (TNW) would have to be much smaller than the present TNW in Europe (which range up to 400 kilotons). In this tiny area, where distances are measured on a doll-house scale (with the exception of Sinai), battlefield use of multikiloton weapons might be self-deterred by fear of collateral damage to one's own population and army. However, subkiloton mininuclear weapons could be developed with yields as low as .01 kiloton (artillery-delivered nuclear projectiles in Europe are as small as .1 kiloton), for use against a wide variety of battlefield targets. 71 Larger interdiction weapons could also be used, possibly in the form of atomic demolition munitions to destroy bridges, cave in tunnels and defiles, cut roads, and otherwise create barriers to slow enemy movement or induce concentrations of its forces. Battlefield nuclear weapons could be used to defend a border⁷² or to arrest the movement of an adversary who had achieved theatrewide conventional preponderance or a blitzkrieg breakthrough in an area of local superiority.⁷

TNW can also be a signaling device to convey the message that a major threshold on the scale of violence has been entered, reminding the adversary of the linkage between the forces in the field and the strategic forces in reserve.⁷⁴ They add a potentially useful rung on the ladder of escalatory responses. If we infer from the reports cited earlier that Israel is likely to enjoy an atomic monopoly for approximately ten years,⁷⁵ but that during this period the

71Though it should be noted that a mini-charge requires just as much nuclear explosive as a charge of greater force, in order to achieve critical mass. The low force is obtained by designing in a low degree of efficiency, and apparently requires substantial knowledge of nuclear weapons technology. See Anders Thunborg, Evolution of Doctrines and the Economics of Defense (Stockholm: Swedish Institute, 1973), Appendix: "What are mini-nuclear weapons," pp. 60-62.

72John F. Scott, Nuclear Strategy for Defending a Border (U.S. Army War College: Strategic Studies Institute, Military Issues Research Memorandum ACN 75042, 1975).

73 James R. Schlesinger, The Theatre Nuclear Force Posture in Europe: A Report to the United States Congress in Compliance with Public Law 93-365 (Washington, D.C.: Department of Defense, 1975) released July 1, 1975, pp. 1-2, 12-16.

74Colin S. Gray, "Theatre Nuclear Weapons: Doctrines and Postures," World Politics, 28 (January 1976), 300–14. Also, Colonel Stanley D. Fair, The Changing Roles for Theatre Nuclear Forces (U.S. Army War College Strategic Studies Institute, Military Issues Research Memorandum ACN 76044, 1976).

75See footnote 32.

Arabs will use their oil wealth and influence to accumulate ever-larger inventories of conventional weapons, an Israel fully committed to nuclearization might use this period to develop TNW to enhance its war-fighting capabilities within the secondary theatres and, still more important, to create in the minds of its adversaries an escalation linkage between combat in the territories and strategic nuclear weapons. This would increase the risks and costs of subnuclear wars for the Arabs, and it might extend the effect of Israel's atomic deterrent to have at least some application in the West Bank, Sinai, and Golan. Israel might well be attracted to develop capabilities that would discourage Arab efforts to nibble at the edges in contentious areas.

However, we are assuming for the present analysis that eventually the Arabs will match whatever nuclear capability Israel might have including the tactical atomic level. If a war should occur in the territories at a time when both sides possess strategic and tactical weapons, the potential for escalation will be greater yet. Presumably, it will be of mutual interest to avoid the use of TNW, and if they are used, to see that an intrawar limiting process continues to operate to protect civilian population sanctuaries. But historical analogies suggest that rational interest alone can fail to arrest the escalation from limited to total war, even when there is a highly developed process of signaling and tacit bargaining. There are bound to be some factions who will call for TNW use to limit casualties, to turn the tide of battle, to bring a more decisive victory, to end the war earlier, or to break the morale of the enemy. The advocates of escalation will play upon the belief that the other side eventually is found to escalate anyway, or conversely, that it has only a limited ability to respond. Accidental violations of limiting principles by the enemy may be misread as intentional acts signaling the end of restraint. As the distance between "allowed" and "disallowed" weapons and targets narrows, as both sides try to bring within their "disallowed" sanctuaries an increasing number of targets regarded as legitimate military objectives by the enemy, and as the "fog of war" thickens, the boundaries between permissible and impermissible actions will be eroded. There may be a sequence of small escalatory steps whose end result the leaders are unable to foresee, but which will lead incrementally to unlimited warfare. And, in the heat of battle, restraint may fail under the pressure of anger demanding expression. Once the hounds of war are unleashed, the decision-makers may be

overwhelmed by the social forces they have activated. 76 No one could be certain that a full-scale war in the territories would not bring out tactical and eventually strategic nuclear weapons.

This principle of uncertainty-as to the conditions under which nuclear weapons might be used, the numbers, kinds, and locations of atomic weapons available to the parties, and the problem of controlling escalation of subnuclear wars-is at once the most dangerous element of a Middle Eastern mini-balance of terror, and yet the most restraining influence. Nothing so clarifies the mind of a statesman as the knowledge that his miscalculation tonight could lead to annihilation in the morning. Strategies of flexible response, graduated deterrence, and calculated escalation in Europe seem to have contributed to an environment of such generalized uncertainty that even low-level confrontations are eschewed for the incalculable destabilizing effects that they might have. It is possible that TNW could have a similar restraining influence in the Middle East. With or without TNW, the probability of full-scale conventional offensives in a nuclear Middle East would be quite low.

Accidents, Unauthorized Use, and Terrorism

If intentional use of nuclear weapons can be controlled, a stable system of deterrence also depends on preventing accidental detonations, controlling unauthorized use of nuclear weapons by subordinates, and preventing the seizure and use of atomic devices by terrorist organizations. The United States and the Soviet Union use sophisticated organizational arrangements and technological devices to guard against these dangers. In the American case, as described by Lyndon Johnson:

the release of nuclear weapons would come by Presidential decision alone. Complex codes and electronic devices prevent any unauthorized action. Every further step along the way from decision to destruction is governed by the two-man rule. Two or more men must act independently and must decide the order has been given... An elaborate system of checks and counter checks, procedural and mechanical, guard against any unauthorized nuclear bursts. In addition, since 1961 we have placed permissive-action links on several of our weapons.

76On the failure of efforts to limit strategic bombing of cities in Europe during World War II, see Frederick M. Sallagar, *The Road to Total War* (New York: Van Nostrand Reinhold, 1969).

These are electromechanical locks which must be opened by secret combination before action at all is possible, and we are extending this system.... We have taken every step man can devise to insure that neither a madman nor a malfunction could ever trigger a nuclear war.⁷⁷

James Schlesinger described another set of more recent technological devices that add to overall weapons security by preventing the use of a nuclear weapon "in the most unlikely event that one is captured," including "improved internal protective hardware which would selectively disable the weapon either permanently or temporarily." 78

Once a state comes into possession of nuclear weapons, it is in the interest of the entire international community (including and perhaps especially its adversaries) that it also possess the means to manage them safely. 79 Yet the most sophisticated permissive action links, disabling devices, and other security systems may be beyond the technological or economic means of small powers to develop for themselves. If this proves to be the case, it may be in the higher interest of the great powers, however opposed they may be to proliferation itself, to pass along weapons security technologies that will reduce the chance of accidents and unauthorized detonations as well as disable weapons that fall into the hands of terrorists or fanatical insurgent groups.

It should be noted that the possibility of theft of a complete nuclear device from a national arsenal, which appears to be a manageable security problem, is not the only means by which terrorist organizations or insurgent movements could come into possession of atomic weapons. Recent studies of terrorism focus on the prospect of the theft of nuclear fuels from various points within the reactor fuel cycle, and then the use of various methods of processing to enrich these materials to fissionable grade. 80

77Address by President Johnson at Seattle, "The Direction and Control of Nuclear Weapons," September 16, 1964, in U.S. Arms Control and Disarmament Agency, Documents on Disarmament 1964 (Washington, D.C.: Government Printing Office, 1965), p. 431.
78Schlesinger, pp. 24-25.

79 See Joel Larus, Nuclear Weapons, Safety, and the Common Defense (Columbus: Ohio State University Press, 1967).

80 See Willrich and Taylor; also Augustus R. Norton, "Nuclear Terrorism and the Middle East," U.S. Army Command, Military Review, 56 (April 1976), 3-4; and Brian Jenkins, Will Terrorists Go Nuclear?, California Seminar on Arms Control and Foreign Policy, discussion paper no. 64 (Los Angeles: Crescent Publications, 1975).

However, these scenarios are not dependent on the existence of nuclear weapons in the region, but rather focus primarily on the security problems of civilian nuclear power programs. The security or insecurity of civilian nuclear activities will exist whether or not atomic weapons programs are initiated, and therefore the main terrorist threats are a separate issue from the design of a stable nuclear balance of terror.

Disclosure

Another problem in the design of a system of mutual deterrence is the potentially traumatic transitional period during which the existence of atomic weapons is disclosed and before the new relationships settle into routine patterns. The degree of instability caused by the transition to nuclear weapons will depend partly on the circumstances of their introduction and disclosure. If disclosure is incremental, and takes the form of a series of rumors, followed by reliable reports, followed by official pronouncements or demonstrations, with adequate time periods between stages, there may be a process of "weaning" or assimilation during which the parties will have time to adjust to the new idea without undue alarm. Conversely, if atomic weapons possession were suddenly disclosed at the height of a crisis, the possibilities of precipitous action and miscalculation would be heightened and the deterrent effect might be lost.

Several Israeli authors have suggested that a policy of calculated *ambiguity* is the optimizing choice of Israel, whether or not Israel has gone ahead with nuclear weapons development.⁸¹ George Quester sees this as a kind of "psychological warfare," and he foresees the day when Arab civilian programs will lead to offsetting "rumors of Egyptian bombs emerging periodically to match rumors of Israeli bombs."⁸² Quester views this as a negative trend, but to the advocate of nuclearization as a path to stability, it is a necessary and constructive process to cushion the shock.

81See Yair Evron, "Israel and the Atom: The Uses and Misuses of Ambiguity, 1957–1967," Orbis, 27 (Winter 1974), 1342, 1332, 1333; Avigdor Haselkorn, "Israel: From an Option to a Bomb in the Basement?" in Robert M. Lawrence and Joel Larus, eds., Nuclear Proliferation: Phase II (Lawrence: University of Kansas Press, 1974), p. 165; and Dowty, forthcoming.

82 Testimony in U.S. Foreign Policy and the Export of Nuclear Technology to the Middle East, p. 189; and George Quester, "Can Proliferation Now Be Stopped?" Foreign Affairs, 53 (October 1974), 83.

From Israel's point of view, perhaps the greatest hazard that might be associated with open disclosure would be the reaction of the United States. The profound antipathy of American policy makers toward proliferation might lead to punitive sanctions in the form of a reduction of economic aid or a restriction in the further supply of conventional arms.⁸³ The Arms Export Control Act of 1976, passed by the 94th Congress and signed into law by President Ford, prohibits economic and military assistance to any country which receives nuclear reprocessing or enrichment equipment, technology, or materials from any other country without thorough provisions for inspection and oversight, unless the president determines that termination of assistance would have "a serious adverse effect on vital United States interests" and unless the president "has received reliable assurances that the country in question will not acquire or develop nuclear weapons."84 It is not clear whether Israel would be in technical violation of this law if it received no further uninspected assistance from abroad (though it would appear that the conditions under which France assisted Israel in the construction of the Dimona reactor in 1958-1962 would constitute a violation if Section 305 applies retroactively). But even if Israel is not in technical violation of existing legislation or policy, an open policy of nuclearization would almost surely disrupt the already fragile relationship with Washington.

If Washington alone restricted the flow of arms and aid—whether on grounds of Israeli self-sufficiency or as a punitive sanction—and the Arabs continued to receive uninterrupted supplies, the effect of nuclearization would be to upset the conventional arms balance which would continue to be of primary importance even after nuclearization. However, the United States might be inclined to adopt the opposite posture and to keep Israel well supplied with conventional weapons to raise the nuclear "firebreak" and reduce the probability that Israel would feel compelled to resort to nuclear response during a conventional war. 85 Perhaps there will be a period of trauma involving a

temporary interruption of aid, after which behavior will return to the normal pattern. However, it is clear from past behavior that any restriction in the flow of American arms of however brief duration will be regarded as a serious handicap in Jerusalem, and this will be a major disincentive to develop nuclear weapons or to disclose them if they exist. On the other side, the possibility that Israel might be driven to nuclearization will act as a restraint on the United States in the degree of pressure it applies against Jerusalem during the hard bargaining ahead on broader diplomatic issues, so the nuclearization threat cuts both ways.

The United States' antipathy to an explicit Israeli disclosure of nuclear weapons possession might be less effective as a deterrent to disclosure if one or more of the following conditions exists at the time: (1) Proliferation becomes epidemic on a world scale, and particularly if there is clear evidence of atomic weapons development or acquisition by one or more Arab states; (2) Washington perceives a significant, large-scale alteration of the conventional military balance against Israel to be taking place but is unable or unwilling for political or technical reasons to provide the huge quantities of countervailing arms required; (3) Continued published rumors of Israeli nuclear weapons lead to a situation in which the world becomes inured to the idea, so that official disclosure is reduced to a fairly minor step; (4) The ambient Arab threat to Israel intensifies considerably, and Washington no longer believes itself to have the leverage to exercise a moderating influence on the Arabs nor does it feel it can expect the Israeli government to resist the pressure of alarmed public opinion inside the country; (5) The Arabs move to the Soviet camp en masse, and the polarization of the region generates a new willingness in the United States to support a hard line; (6) America has already made

⁸³See Fuad Jabber, "Not By War Alone: Curbing the Arab-Israeli Arms Race," Middle East Journal, 28 (Summer 1974), 240; also Aubrey Hodes, "Implications of Israel's Nuclear Capability," Wiener Library Bulletin, 22 (August 1968), 4.

⁸⁴P.L. 94-329, "Arms Export Control Act," Section 305, "Nuclear Transfers." See Congressional Quarterly Almanac 1976, p. 215.

⁸⁵ See Richard Burt, "Proliferation and the Spread of New Conventional Weapons Technology," Inter-

national Security, 1 (Winter 1977), 119-39. Secretary Kissinger declared himself in opposition to "overly severe constraints which would seriously set back, rather than advance, our nonproliferation efforts" including those which would "cast further doubt on the credibility of U.S. supply commitments and the constancy of our policy at precisely the moment when we can least afford such doubts." Statement on U.S. nonproliferation strategy before the Senate Committee on Government Operations, March 9, 1976, reprinted in Department of State Bulletin, 74 (March 29, 1976), 410. On the other hand, Senator Symington has been a vigorous advocate of strong measures. See also Donald Brennan et al., The Implications of Precision Weapons for American Strategic Interests (Harmon-on-Hudson, N.Y.: Hudson Institute, 1975), Report 2264-RR, pp. 25-33.

excessive use of negative sanctions against a wide variety of other allies for various reasons, and it faces a general loss of credibility as an ally if it continues coercing friends; (7) Washington is already applying severe sanctions against Jerusalem to force concessions on other issues, so that the incremental cost in American displeasure following nuclearization is reduced to a tolerable level (indeed, America might have to choose between using its leverage for diplomatic goals or to halt proliferation, not being able to accomplish both); (8) One or more Western allies in other regions have recently fallen to Soviet-sponsored attacks, and the general mood in the United States shifts to a harder line. It is entirely possible that one or more of these conditions will materialize, and that the problem of American reaction to Israeli disclosure will be less severe at the time than one would assume today.

Preventive War

Another danger in the disclosure process that has impressed some observers is the possibility of preventive war by one side to arrest the atomic development of the adversary. President Nasser warned in 1960 that, if it were certain that Israel was constructing a bomb, "it would mean the beginning of war between us and Israel, because we cannot permit Israel to manufacture an atomic bomb. It is inevitable that we should attack the base of aggression."86 Major General Ariel Sharon, a leading Israeli hawk, echoed this theme in 1975: "If we discover that the Egyptians are working on a nuclear weapon of their own ... we'll have no choice but to wipe them out."87 Others have discussed the possibility of a Soviet disarming attack.⁸⁸ But these prophecies of "surgical strikes" to excise an adversary's "nuclear cancer" have a long and dubious history. Harrison Salisbury and others adumbrated Soviet strikes against the Chinese atomic facilities in Lanchow, Paotow, and Lop Nor;89 some "revisionist" historians of the origins of the Cold War

86Quoted in Jewish Observer and Middle East Review, December 23, 1960; cited in William B. Bader, The United States and the Spread of Nuclear Weapons (New York: Pegasus, 1968), p. 96. have claimed that several key U.S. military officials, reportedly including Secretary of the Navy Francis P. Matthews and Commandant of the Air War College General Orville Anderson, favored preventive war against the Soviet Union in the late 1940s in order to smash its nascent atomic technology;90 and a popular American magazine found it necessary to warn readers in 1948 against the simplistic idea of "using the atomic bomb against the Russians before they crack the formula and use it against us."91 These scenarios of disarming surgical strikes seem to excite the imagination of the general public and some alarmed scholars, but they do not seem to have the same credibility for responsible commanders-in-chief.

In the Middle East, one scenario that has attracted wide interest is the prospect of a Soviet strike against Israel; but Moscow would have to consider the possibility of an American response and even Israeli counterthreats to the Baku industrial region and the major cities of south-central Russia, including Odessa and Tbilis, or to the Black Sea/Mediterranean fleets.⁹² The arresting idea of direct Israeli deterrence cannot ignore the fact that Soviet strategic air defense is by far the most massive and extensive in the world, consisting of more than 5,000 radars, 2,600 fighter-interceptors, and almost 12,000 surface-to-air missiles. However, its low altitude intercept capability is limited⁹³-though it is being corrected to a degree by the introduction of the Su-15 Flagon-E-and a desperate Israel might overfly weakly defended areas at treetop levels to execute a limited strike on southern Georgian cities. Even a small possibility that unrestricted escalation could result in destruction of a

90See David Horowitz, From Yalta to Vietnam (London: Penguin, 1967), p. 263; and I. F. Stone, The Hidden History of the Korean War (New York: Monthly Review Press, 1952), pp. 92–94. Preventive war options were assessed officially by the National Security Council in 1950 and, according to all reports, rejected categorically on philosophical, political, and military grounds. See NSC-68, A Report to the National Security Council by the Executive Secretary, on United States Objectives and Programs for Security (Washington, D.C.: Government Printing Office, April 14, 1950), pp. 11, 53, and 37.

91"Is War Inevitable?" Woman's Home Companion, 75 (March 1948), 43, excerpted in Peter G. Filene, American Views of Soviet Russia, 1917–1965 (Homewood, Ill.: Dorsey Press, 1968), p. 235.

⁹²See Haselkorn, p. 165 ff; and J. Bowyer Bell, "Israel's Nuclear Option," *Middle East Journal*, 26 (Autumn 1972), 386-87.

93"Soviet Air Defenses," Air Force Magazine, March 1976.

⁸⁷ Interview in New York Times, February 8, 1975.
88 For example see Haselkorn, pp. 168-69.

⁸⁹Harrison Salisbury, War Between Russia and China (New York: Bantam Books, 1970), pp. 126–27. The United States reportedly expressed its concern about this possibility to the Soviet Union. See John Newhouse, Cold Dawn (New York: Holt, Rinehart, & Winston, 1973), pp. 164–65.

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Soviet city could be expected to act as a moderating influence on Russian policy.⁹⁴

Conclusion

Overall, the problems of preventive war, accidents, second-strike force survivability, escalation, and unauthorized use and theft of weapons appear to be manageable in the Middle Eastern context. The design of a stable regional system of deterrence will require imagination in adapting the techniques and conventions of superpower atomic diplomacy to a quite different context. But it would be quite unjustified to assume that the introduction of nuclear weapons in the Arab-Israeli conflict inevitably will lead to catastrophe.

On the other hand, even if it is accepted that nuclear weapons may make a contribution to the stabilization of the Arab-Israeli conflict, they are not a panacea for all the problems of the region. They will not resolve the deeply rooted conflict of historical claims and national interests between the Arabs and the Israelis. They will not resolve the issues of the final disposition of the 1967 territories—indeed, the outcome to these questions will depend more on political negotiations than on any balance of military forces whether or not nuclear weapons are introduced in the Middle East. They will not solve the problem of the Palestinian Arabs, nor will they prevent guerrilla actions. They will not bring status and prestige to their owners-indeed, in Israel's case they may reinforce its image as a pariah state. And, contrary to the major argument of one proponent of Israeli nuclearization, they will not decisively reduce the dependence of Israel and other regional states on outside aid from America and other sponsors.95

94The classic expression of the theory of finite deterrence is Pierre Gallois, *The Balance of Terror* (Boston: Houghton Mifflin, 1961), pp. 1–14. See also Geoffrey Kemp, *Nuclear Forces for Medium Powers: Part I: Targets and Weapons Systems*, Adelphi Paper no. 106 (London: International Institute for Strategic Studies, 1974).

95Robert W. Tucker advocates an open Israeli

The one thing that strategic and tactical nuclear weapons may do, and this at considerable risk and expense, is to stabilize the military balance in the region in a manner that conventional arms alone cannot achieve, and thereby create an essential precondition for a durable political settlement.

nuclear deterrent primarily to reduce the dangerous dependence of the Jewish state on American aid and to enable it to depend to the maximum extent possible on its own resources. See his "Israel and the United States: From Dependence to Nuclear Weapons?" Commentary, 60 (November 1975), 29-43. This proposal rests on the highly arguable supposition that conventional military expenditures could be reduced substantially after nuclearization. Moreover, it implies that the sole cause of Israel's dependence is military expenditures, which is demonstrably not the case. Israel's post-1973 reliance on vastly increased American aid is accountable primarily to the widening of her balance of trade deficits from an annual average of approximately \$1 billion before the war to the \$4 billion range following it. However, less than half (46 percent) of the increase in the balance of payments deficit between 1972 and 1975 is accountable to defense imports, the remainder representing the consumer goods import surplus, worsening terms of trade, and other non-defense-related factors. Israel's civilian sector historically imports over one-third more than it exports. The post-1973 global inflation of prices therefore automatically widened the absolute deficit, even though export growth rates have exceeded civilian import growth in recent years. Had there been no defense imports or exports at all, Israel's balance of payments deficit without U.S. aid and other donations payments deficit without U.S. aid and other donations would still have exceeded \$2.2 billion in 1975. See "Balance of Payments" in Bank Leumi Economic Review, no. 85 (October 1976), pp. 2-5. Israel's total imports in 1975 were \$7,430 million, of which defense accounted for approximately \$1,500 million. See David Kochay, "Economics of Defense: Israel," in Military Aspects of the Arab-Israeli Conflict (Tel Aviv. Liviversity, Publishing Projects, 1975), p. 181 Conclusions. University Publishing Projects, 1975), p. 181. Conclusion: if defense imports were reduced to zero, Israel's financing requirement for U.S. aid would be reduced by about half, but would still approximate \$1 billion per year, and the U.S. might not be willing to provide any economic assistance to a nuclear Israel. And of course it is quite unrealistic to assume that conventional defense imports would be discontinued, if indeed they would be reduced at all. A similar case could be made regarding the effects of nuclearization on Egypt's aid requirements, most of which also derive from civilian rather than military demands.

Party Systems and Government Stability in the Indian States*

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This paper contributes to the substantive and methodological discussion of the issues concerning the causes of cabinet instability through analysis of data from Indian state politics. The focus of the analysis is on explaining the duration of Indian state governments in days with variables measuring the degree of fragmentation and cohesion in the party system, the composition of the cabinet, the characteristics of the opposition, and the role of ideological differences. A substantial amount of the variation in the durability of coalition governments is explained with variables that measure the degree of party system institutionalization and the extent of political opportunism, but ideological factors do not explain much of the differences in durability of governments. It is also found that none of the measures used can explain much of the variation in one-party majority governments for which, it is argued, explanations must be sought that focus on leadership skill and on relationships between leaders and factions in a dominant party.

Introduction

The study of the causes of government or, more properly, cabinet instability in political science has a long and hallowed history. Michael Taylor and V. M. Herman in their 1971 article on "Party Systems and Government Stability" used contemporary political science methodology to test hypotheses developed on this question by A. Lawrence Lowell 80 years ago. 1 During the past 80 years, both the methodology for analyzing the causes of cabinet stability and instability and the range of countries and governments brought under analysis have changed dramatically, but the theoretical questions have remained largely unchanged. Whereas Lowell concentrated his attention on detailed descriptive accounts of five political systems, contemporary political scientists tend to present accumulations of data on the governments of the 19 "Western" parlia-

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¹Michael Taylor and V. M. Herman, "Party Systems and Government Stability," American Political Science Review, 65 (March 1971), 28-37; A. Lawrence Lowell, Governments and Parties in Continental Europe, Vol. 1 (Cambridge, Mass.: Harvard University Press, 1896), pp. 70-74.

mentary democracies, sometimes enlarged slightly by the inclusion of Israel, Japan, and/or the United States.² And, whereas Lowell was largely content to present his arguments in the form of axioms supported by the evidence from his descriptive accounts, contemporary political scientists have an array of measures, indexes, and statistical techniques to choose from to test their hypotheses. Even when contemporary political scientists concentrate their attention on a single political system, of which Fourth Republic France has received the lion's share, the methods of analysis nowadays tend to involve sophisticated statistical manipulations of large masses of data.³

Although recent methodological advances have produced some refinements in the ability of political scientists to measure more precisely the structure of legislatures and the composition of governments, answers supplied to the basic questions concerning the effects on government stability of the structure of the legislative party system, the composition of the cabinet, the characteristics of the opposition, and the role of ideological differences have so far fallen short of a general theory of cabinet

²See esp. Taylor and Herman; Jean Blondel, "Party Systems and Patterns of Government in Western Democracies," Canadian Journal of Political Science, 1 (June 1968), 180–203; and Lawrence C. Dodd, "Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis," American Political Science Review, 68 (September 1974), 1093–1117.

³The methodologically most sophisticated analysis of cabinet instability in Fourth Republic France is, of course, that done by Duncan MacRae, Jr., Parliament, Parties, and Society in France: 1946–1958 (New York: St. Martin's Press, 1967).

stability. Taylor and Herman found that most of Lowell's arguments were confirmed by the contemporary evidence, namely, that government instability was associated with fragmented multiparty systems and with coalition governments. They also found that governmental instability in the 19 parliamentary systems they studied was particularly associated with the size of the antisystem (principally the Communist) parties and the fragmentation of the prosystem parties, but otherwise that ideological differences among political parties were not of great significance in explaining cabinet instability.4 Lawrence Dodd, in a recent analysis of data from 17 European and Commonwealth party systems, has modified Lowell's and Taylor and Herman's arguments somewhat by specifying the conditions under which some multiparty systems may produce durable coalition cabinets and has attempted to incorporate his findings into a general theory of government formation and persistence. His argument in brief is that durable coalition cabinets tend to be minimum winning coalitions and that such coalitions are most likely to form in multiparty parliamentary systems in which the degree of party system fractionalization is moderate and the party system relatively stable. Dodd's set of variables, like Taylor and Herman's, explains only approximately 25 to 30 percent of the variance in duration of the governments analyzed.⁵ However, Dodd's treatment of coalition systems separately from one-party systems and his focus on the influence of party system stability in analyzing the causes of cabinet stability are desirable innovations consistent with the approach adopted in this article.⁶

My purpose in writing this article is to contribute to both the substantive and methodological discussion of these issues through analysis of data from India, a country not included in any of the broadly comparative studies so far carried out by contemporary political scientists. In doing so, I have three general points to make. The first is that political scientists ought not to be satisfied with inconclusive answers on questions concerning the causes of government stability and instability and with correlations which explain no more than 25 percent of the variance. When we are dealing with only 100 or 200 cases of govern-

ments, whose every moves have been documented in the press and whose participants often can still be interviewed, we should not be satisfied with correlations that leave most of the variance unexplained. To improve our explanatory capacities, however, political scientists will not be able to rely on only one methodological approach, but will have to combine quantitative analysis and case studies, the former to identify the evident patterns and regularities and to pinpoint the interesting anomalous cases and the latter to explain the anomalies through in-depth analysis of particular cases.

In social science, it is of little value to push aside unexplained variance in hopes that the all-comprehensive theory or the ultimate methodological tool will eventually be discovered. Therefore, my second point is that in analyzing a problem such as the causes of cabinet instability, political scientists ought not to do only "normal science," but must always seek explanations for the anomalous cases as well, by combining methodological approaches. Wherever quantitative analysis leaves a substantial portion of the variance in a dependent variable unexplained, it is incumbent on the researcher to propose an alternative research strategy for explaining the remaining variance through either further quantitative analysis of new kinds of data or through case studies that explore the anomalous cases in particular.

In this article, quantitative analysis of legislative and electoral variables provide an explanation for approximately 40 percent of the variance in the duration of coalition governments, but it proves to be much less useful in explaining the duration of one-party governments. Consequently, some examples, particularly anomalous cases, are extracted from the data file for examination at the end of this article, where the kinds of additional data and case studies that seem most appropriate to carry the explanation further are suggested.

My third point is that the range of observations in the study of cabinet government is still not broad enough, having been confined so far only to the Western and Commonwealth industrialized countries and Japan and, within those countries, only to the central cabinets. If we are looking for a general theory of cabinet instability, the data base ought to be extended to include contemporary governments outside the circle of the 19 or 20 industrialized democracies. In this article, the range of observations will be extended to include postindependence state governments in India.

India as a Research Site. The extension to

⁴Taylor and Herman, pp. 34-37.

⁵Dodd, pp. 1111–12.

⁶However, the approach taken in this article derives exclusively from the Lowell, Blondel, Taylor-Herman tradition rather than from the game theory tradition which Dodd combines with the former in his study.

India of research on cabinet stability and instability adds not just one additional government to the comparison, but, by including observations on the provincial level, a whole new universe of data comparable in breadth and diversity to the entire range of cases previously brought into view in the study of Western parliamentary systems. The collection and interpretation of data on Indian state party systems and governments also offers the potential for opening the study of Indian politics to comparative methods of analysis in a manner similar to the development of the study of comparative state politics in the United States after V. O. Key began his systematic survey of American state politics and party systems.7 And, just as Key pointed out that the description of the United States as a two-party system masked a remarkable degree of diversity of party systems at the state level, the conventional view of India as a one-party dominant system has concealed an even wider range of types at the provincial level. In broad descriptive terms, the range has included multiparty systems with one party dominant, dualized competitive systems (multiparty systems in which the principal conflict is between the two largest parties), segmented pluralist systems in which several institutionalized parties command significant political support in a sociocultural segment of society,8 and fragmented multiparty systems in which no party has a strong and stable base of support in the electorate. The knowledge available so far on such systems in the Indian states has come nearly exclusively from descriptive analyses and case studies of particular states done by regional specialists.9 Very little truly comparative work has been done on party systems and government stability in the Indian states and still less work has been done which sets out to advance theory on this

⁷V. O. Key, Jr., Southern Politics in State and Nation (New York: Knopf, 1949) and American State Politics: An Introduction (New York: Knopf, 1956).

⁸The term "segmented pluralism" comes from Val R. Lorwin, "Segmented Pluralism: Ideological Cleavages and Political Cohesion in the Smaller European Democracies," Comparative Politics, 3 (January 1971), 141–75.

⁹Two important collections of such studies are in Myron Weiner, ed., State Politics in India (Princeton, N.J.: Princeton University Press, 1968), and Myron Weiner and John O. Field, eds., Electoral Politics in the Indian States in four volumes, of which see esp. Vol. 4, Party Systems and Cleavages (Delhi: Manohar Book Service, 1975).

subject by the explicit testing of carefully specified hypotheses.¹⁰

The data to be presented and analyzed below were drawn both from existing provincial units in India and from previously existing units that have been merged into or divided up among the present 21 states as a consequence of India's prolonged process of internal reorganization. The basis for including a provincial unit in the data base for this study was whether or not it had had experience with popularly elected legislatures and cabinet governments. There were 39 provinces in India that had such experience in the period between the First General Election of 1952 and March 27, 1974, the cutoff date for this study, and there were 127 governments in those units during that period. The data base clearly is large enough for the kinds of statistical analyses to be used in this article.

Throughout this article, the dependent variable whose variation is to be explained is the duration of Indian state governments in days. A government has been defined for purposes of this study as any administration whose chief minister has been sworn in by the governor of a state and who remains in office without an interelection resignation. In other words, a government that undergoes a formal resignation and a new swearing-in ceremony after a general election, but under the same chief minister, will be counted as one government. However, in a situation where a chief minister resigns between elections and either reforms the government with a new swearing-in ceremony or is replaced by another chief minister, the two administrations in such cases will be counted as two governments. This definition is not the same as that used both by Taylor and Herman and by Blondel, but it approximates their definition in practice while taking into account the constitutional and political features of Indian state politics. Their definition, which includes, in addition to continuity of leadership, continuity of party support in the legislature, 11 could not be used in India because it is not always clear which parties in fact support the government; this is a determination that is frequently made by the governor of the state rather than by a formal test in the legislature.

The descriptive-analytical portion of this article is divided into five sections. In the first part, some of the hypotheses drawn from the

¹⁰However, see Paul R. Brass, "Political Participation, Institutionalization, and Stability in India," Government and Opposition, 4 (Winter 1969), 23-53.

¹¹ Taylor and Herman, p. 29, and Blondel, p. 190.

Taylor and Herman study have been tested with ly. 13 In addition to these two measures, a third the replicated Indian data. It is demonstrated here that these hypotheses, though confirmed, do not have much explanatory power in the Indian case particularly when Indian state governments are broken up into different types. Different explanations are required for coalition than for one-party majority governments and for majority and minority governments. Consequently, these different types of governments are analyzed separately in the three sections that follow. In these three analytical sections, the principal independent variables are various measures (some derived from other studies, others used here for the first time) of governmental, legislative, and party system fragmentation, cohesion, and institutionalization. 12 In addition, the data are used to test hypotheses concerning the impact of "antisystem" opposition and of ideology and opportunism in determining the relative endurance of governments. In the final analytical section, brief consideration is given to the broader question of the conditions that produce different types of party systems and to the relationship between theories of party system development and theories of cabinet stability.

Legislature and Government

The Legislative Party System. Taylor and Herman used two measures of fragmentation of the parliamentary party system-the number of parties holding seats in parliament (N_p) and Rae's index of parliamentary fractionalization (F_n) . They found that government stability (measured in terms of duration of governments in days) correlated negatively with both measures in the 19 parliamentary systems they studied. Their correlations were r = -.39 and $R^2 = .152$ for N_p with government stability and r = -.448 and $R^2 = .201$ for F_p with government stability, so that N_p accounted for 15 percent and F_p for 20 percent of the variation in governmental stability, respective-

12As in the Taylor and Herman study, all legislative structure variables are held constant during the entire period of a legislature between elections even though more than one government may form and terminate in the life of a single legislature and even though conditions inside the legislature itself may change between elections. In the case of governments that endure through more than one election, the value assigned to each independent variable is "a weighted average of its values in the periods before and after the election, the weights corresponding to the lengths of those periods." Taylor and Herman, p. 29.

measure, the percentage of seats held by the largest party in the legislature (P_p) , has been used here with the Indian data. ¹⁴ Although P_p is highly intercorrelated with F_p (r = -.948)and is, more or less, its obverse, it seemed appropriate to use P_p also because of the importance of one-party dominant systems in Indian state politics. The hypothesis tested with this measure is that government stability is correlated positively with P_p -the reasoning being that the higher the P_p the more likely it is that one party will be able to form a majority government; in addition it is known that one-party majority governments on the average tend to persist longer than coalition or minority governments.

The two hypotheses derived from the Taylor and Herman studies and the third just presented were tested for the Indian state governments, using the data on individual governments for computation of the product-moment correlations. The Pearson correlation coefficients for the three variables with government stability are as follows: -.272 with N_p , -.333 with F_p , and .371 with P_p . Thus, the negative correlation of the state of the stat tions found between government stability and N_p and F_p with the data from the 19 parliamentary systems are confirmed with the data from Indian state governments, but the correlations are smaller and the amount of variation explained by these variables in the Indian states is only 7.39 and 11.08 percent, respectively. F_p explains slightly less of the variance in the Indian data than the more simple measure of. percent seats held by the largest party in the legislature, the R^2 for P_p with government stability being .1378 (13.78 percent).

The Role of "Antisystem" Opposition, Taylor and Herman's leading finding was that cabinet stability correlated negatively with the percentage of seats held in the legislature by antisystem parties, principally the Communist parties. 15 This finding conforms well with what is known about the difficulties posed for cabinet construction and endurance in the two most unstable systems in Europe after World War II, France and Italy, where the Communist parties were excluded from government. India

¹³Ibid., pp. 30-31.

¹⁴All three of these variables were first used systematically as measures of different types of party systems by Douglas W. Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967), Ch. 3.

¹⁵Taylor and Herman, pp. 35-36.

has two strong Communist parties, whose strength varies from state to state, but their "antisystem" character has not been as pronounced or as much feared as in Europe because they have both acted within the rules of the game of Indian politics, and they have not always been excluded from alliances with other parties or from participation in coalition governments. 16 However, because there are some parties in Indian politics whose leaders prefer to avoid coalitions with one or the other or both of the Communist parties, a correlation was run for government duration and the combined Communist party seat shares (CP) in the Indian state legislative assemblies. Once again, the Taylor and Herman hypothesis was supported, but at a much lower level, the correlation coefficient being only -.157 compared to -.450 in their study.

The Composition of the Government. Taylor and Herman looked for the sources of cabinet instability in the structure of the cabinet as well as in the degree of fragmentation in the legislative party system. They found that one-party governments were more stable than coalition governments, that majority governments were more stable than minority governments, and that government stability correlated negatively both with the number of parties in the cabinet and with governmental fragmentation.

16It can be argued that the West European Communist parties, despite their revolutionary rhetoric, have also operated in terms of the rules of the parliamentary game and that they have been kept out of governments because of the real or alleged fears of their opponents concerning the dangers of Communist participation. Whatever the true explanation of the European situation, my point here is that the Indian Communist parties have played the parliamentary game and have not been totally excluded as coalition partners in state governments.

All these relationships are confirmed by the Indian data.

Most noteworthy are the relationships shown in Tables 1 and 2 between the average duration of Indian state governments in days and the type of government. The difference between the mean duration in days for oneparty governments compared with coalition governments is substantial and significant at the .001 level. (The t-statistic is 4.71 with df of 125, and the r, using dummy variables, is .388.) The difference between the mean duration of majority and minority governments is less substantial but still considerable, with a t-statistic of 3.75, which also is significant at the .001 level. The principal line of demarcation, however, is clearly between one-party governments (particularly one-party majority governments) and coalition governments (particularly minority coalition governments). Moreover, this distinction is even sharper with the Indian data than it is with the data from the Western parliamentary systems. Indian one-party governments last longer on the average than Western parliamentary systems (1187.5 days for Indian governments compared to 1107.9 days for Western governments), but Indian coalition governments are far more short-lived than their Western counterparts (378.3 days for Indian governments, 624.5 days for Western governments).¹⁷

The importance of the distinction between one-party governments and coalition governments is demonstrated further in Table 2, where the data on average duration of Indian state governments are broken up into four duration categories and then cross-tabulated with type of government. All governments that lasted longer than five years in India were

17 Taylor and Herman, p. 31.

Table 1. Mean Duration of Indian State Governments in Days by Type of Government

Type of Government	$(N = 127)^{a}$	Mean Duration ^b	Standard Deviation	Range
One-party majority	(76)	1307.9	1022.2	12-4685
Oneeparty minority	(16)	615.3	546.2	19-1750
All one party	(92)	1187.5	990.0	12-4685
Majority coalition	(22)	495.4	400.2	12-1405
Minority coalition	(13)	180.1	139.2	7- 411
All coalition	(35)	378.3	360.1	12-1537
All majority	(98)	1125.5	979.1	12-4685
All minority	(29)	420.2	465.5	7-1750
All governments	(127)	964.5	935.2	7-4685

^aSome of the categories overlap. Consequently, the totals do not add up to 127. However, the Ns for rows 1, 2, 4, and 5, for 3 and 6, and for 7 and 8, respectively, form discrete subsets of the data that do add up to 127. ^bAll the means are unweighted averages.

one-party majority governments. Moreover, there is a monotonic decline in the proportion of one-party majority governments as duration in days declines from one category to another. The relationship is precisely opposite for coalition governments. No coalition government lasted longer than five years, only two lasted longer than three years, and the proportion of coalition governments to other types increases as the category of duration in days declines. Of the governments that lasted less than one year, 56.4 percent were coalition governments and 74.3 percent were either coalition governments or one-party minority governments.

These data clearly establish some evident regularities and general patterns in the data of which the most prominent are the demonstration that cabinet instability is associated with particular types of government and that coalition governments are the most unstable type of government in the Indian states. However, the demonstration of these regularities constitutes only the beginning of the inquiry. The data in Table 1 showing the standard deviation and the range in duration for different types of governments suggest the extent of the problems remaining to be resolved. The standard deviations for all types of governments are high and the range within each type is very broad. What remains to be explained, therefore, are the reasons for the differences in the duration of the different types of governments and for the considerable range in duration within types.

While it is commonly known that one-party governments tend to endure longer than coalition governments, there is far from unanimous agreement in the theoretical literature on why this pattern should hold. Two types of rival explanations have been prominent. One is that

coalition governments tend to arise in political systems where the society is divided by multiple ideological and/or ethnic cleavages which so fragment the support of parties in the electorate that coalition governments are necessary, but also cannot be sustained for very long because of the irreconcilability of the ideological differences among coalition partners. The second type of explanation also assumes that multiparty systems arise in political-social orders divided by multiple cleavages, but argues that it is not the ideological differences among the principal parties in a multiparty system that matter so much as the opportunities given to the nonideological parties, groups, and individuals to exploit the differences among the parties by bargaining for ministerial positions with the highest bidders from competing coalitions. In more homogeneous societies where political culture and social structure sustain either twoparty or one-party dominant systems, it is commonly assumed that neither ideological nor pragmatic problems prevent the formation of stable governments.

The same set of issues arises in interpreting the relative stability of governments within types as between types. Sartori's distinction among moderate, polarized, and atomized multiparty systems, ¹⁸ for example, has been modified by Dodd, who argues that the type of

18 Giovanni Sartori, "European Political Parties: The Case of Polarized Pluralism," in *Political Parties and Political Development*, ed. Joseph La Palombara and Myron Weiner (Princeton, N.J.: Princeton University Press, 1966), pp. 137–76, and "The Typology of Party Systems-Proposals for Improvement," in *Mass Politics: Studies in Political Sociology*, ed. Erik Allardt and Stein Rokkan (New York: Free Press, 1970), pp. 322–52

Table 2. Cross-Tabulation of Duration of Indian State Governments in Days with Type of Government

	Duration in Days							
		826+ = 17)		-1825 = 28)		-1000 = 43)		-365 = 39)
Type of Government ^a	N	%	N	%	N	%	N	%
One-party majority	17	100.0	23	82.1	26	60.5	10	25.6
One-party minority	0	0.0	3	10.7	6	14.0	7	17.9
All one party	17	100.0	26	92.9	32	74.4	17	43.6
Majority coalition	0	0.0	2	7.1	9	20.9	11	28.2
Minority coalition	0	0.0	0	0.0	2	4.7	11	28.2
All coalition	0	0.0	2	7.1	11	25.6	22	56.4
Majority	17	100.0	25	89.3	35	81.4	21	53.8
Minority	0	0.0	3	10.7	8	18.6	18	46.2

^aAs in Table 1, the categories overlap (see footnote a to Table 1). All percentages are based on the total N for each column. The percentages for rows 1, 2, 4, and 5, for rows 3 and 6, and for rows 7 and 8, respectively, add to 100.00 in each column.

cabinet coalition formed in such systems-"oversized," "undersized," or minimum winning coalition-is influenced by the degree of "party system fractionalization, instability and cleavage conflict" and that the durability of the cabinet coalition in turn depends upon its "coalitional status," minimum winning coalitions being the most durable. This explanation takes account of ideological factors, measured in the "cleavage conflict" variable, but places as much emphasis on structural factors in the party system as determinants of coalitional status and more emphasis on structural than ideological differences in the cabinet coalition as determinants of its durability. For, in contrast to Sartori who emphasizes the instability and immobilism of "polarized pluralism," Dodd argues that stable pluralist systems tend to produce minimum winning coalitions, hence more durable coalitions, irrespective of whether or not the party system is "depolarized" or "polarized" ideologically. 19

These alternative explanations, one emphasizing structural and the other ideological factors, will be explored below insofar as possible with the available data. It will be argued that in the Indian cases structural factors, such as the relative dominance of larger and more cohesive political parties in the legislature and the degree of party system institutionalization, have been more important conditions for stable government than the presence or absence of ideological differences between parties. However, it must be conceded at the outset that the way in which the data were distributed provided more satisfactory measures of structural than of ideological variables.

Coalition Governments

Three types of independent variables were used with the Indian data in an attempt to explain the variation in the stability of the 35 coalition governments. One set of variables measured different aspects of the structure and composition of the legislature, a second set measured aspects of the structure and character of the parties and persons in the government. and a third (comprising only one variable) measured the degree of institutionalization of the party system in the electorate. No attempt was made in this article to relate government stability or instability to conditions external to the government, legislature, or party system.

19Dodd, p. 1110.

An analysis of external conditions such as public demonstrations and agitations, riots, and civil disorders would be of relevance and importance, but is better handled through case studies than through the kinds of quantitative methods used here. For purposes of this article, I am concerned principally with ascertaining how much of the variance in the stability of different types of governments can be explained by using measures on which comparable data can fairly easily be obtained or generated for all the Indian states, and which facilitate comparisons with the results obtained in other studies of cabinet instability.

The Structure and Composition of the Legislature. In the previous section, it was shown that government duration correlated positively with P_p and negatively with N_p , F_p , and the combined proportion of seats in the legislature held by the Communist parties (CP). However, these correlations all change markedly when done for only the coalition governments, as shown in Table 3. The negative association between $F_{m p}$ and duration practically disappears, that between P_p and duration is similarly reduced and changes sign, the correlation for CP is reduced and changes sign, and only the correlation for N_p increases and retains its sign and significance level. A comparison of the correlations for these variables for one-party and coalition governments indicates clearly that P_p and F_p are not explanatory variables at all for either type of government, but are merely descriptive indicators of the structure of the legislature. The positive correlation of .371 for P_p and the negative correlation of -.333 for F_p with duration for all governments merely indicated that one-party governments are in general more stable than coalition governments, but the absence of any association between P_p or F_p and duration for one-party or coalition governments considered separately suggests that the proportion of seats held by the governing party and the degree of fragmentation in the legislature are not critical factors in explaining duration for either form of government. Interestingly enough, however, N_p becomes the strongest explanatory variable (of the legislative structure variables) in relation to coalition governments, a fact that requires explanation since, on the face of it, N_p appears to be only a less precise measure of fractionalization than F_p and is intercorrelated with F_p at .606. Nevertheless, it is fairly easy to understand why N_p may, in situations involving coalition formation, be a more important variable than F_p . For example, an F_p of .74 or .75 may reflect a relatively even division of four political parties in a legislature, each holding approximately 25 percent of the seats, or it may reflect a seven-party division, with one party holding 45 percent of the seats, another 15 percent, three others 10 percent, and two others 5 percent. In the second case, there are more coalition possibilities, and a majority coalition without the leading party would be more complex and, therefore, less likely to be durable than any possible coalition that could be formed out of the first situation. Thus, the different correlations for duration and N_p with one-party majority and with coalition governments suggest that the number of parties in the legislature does not matter when one party is dominant, but does matter in multiparty systems where no single party has an overwhelming majority of seats in the legislature.

One other correlation in Table 3 that requires explanation is the positive correlation

between duration and the Communist party seat share for coalition governments. Previously, Taylor and Herman's association of instability with a high Communist seat share was confirmed, though weakly, for all state governments in India. It now appears, however, that the Communist party vote, which is intercorrelated with F_p at .457 and with P_p at -.490, is merely one contribution to multipartyism and fragmentation in the legislature. In other words, a high Communist party representation in the legislature is associated with greater multipartyism, which in turn increases the likelihood that coalition governments will be required. However, the combined Communist seat share in the legislature has no effect on the stability of one-party governments and very little effect on the duration of coalition governments. There are two reasons why Communist representation may affect the durability of coalition governments in India quite differently from those in

Table 3. Correlation Coefficients for Duration with Variables Measuring the Structure of the Legislature, the Composition of the Government, and Party System Institutionalization in the Indian States

Variable ^a	Type of Government							
	All (N = 110, 113, 126, 127)	One-Party Majority (N = 67, 76)	Coalition (N = 18, 32, 34, 35)	One-Party Minority (N = 14, 15, 16)				
F _p N _p CP P _p TPR	333*	010	038	575 *				
N_n	272*	.096	381*	548*				
C₽	157*	.010	.074	.085				
P_n	.371*	.012	007	.433*				
TPR	.208*	.148	NCb	093				
W _p F _O N _{pG} ^c F _G IC	NC	NC	.051	.553*				
F_{o}	NC	NC	NC	430*				
$N_{nG}^{\mathbf{c}}$	219*	NC	221	NC				
$F_G^{ u}$	~.360*	NC	2 19	NC				
IČ	.205*	NC	.237	NC				
IDG	.290*	NC	.277*	NC				
MINMAJCO	.318*	NC	.428*	NC				
CONGNON	.195*	NC ·	.264	163				
PSI	.370*	.228*	.534*	.408*				

^{*}Significance level of .05 or better.

Rae's formula for F_p is: $F_p = 1 - (\sum T_i^2)$ where T_i is any party's decimal share of seats in the legislature.

 $^{^{}a}F_{p}$ is Rae's index of parliamentary fractionalization; N_{p} is the number of parties in the legislature at the time of election; CP is the combined seat share of the Communist parties in the legislature; P_{p} is the seat share of the largest party in the legislature; TPR is the ratio of seats held by the governing party to those held by the next largest party; W_{p} is the seat share of the two largest parties; F_{O} is the fractionalization of seat shares among opposition parties only, using Rae's formula; N_{pG} is the number of parties in the government; F_{G} is the fractionalization of party representation in the government; IC is a dummy variable for ideologically polarized and nonpolarized governments; IDG is a dummy variable for presence or absence of independents or defectors in the government; MINMAJCO is a dummy variable for majority and minority governments; CONGNON is a dummy variable for Congress and non-Congress governments; PSI is a measure of party system institutionalization defined in the text of this article.

bNC = not calculated.

^CThe correlations reported in this row exclude governments that contained independents or defectors because of the ambiguities involved in deciding whether or not to classify each independent and/or defector as one party or certain groupings of independents and/or defectors as one party. Moreover, excluding such governments from this variable avoids overlapping with variable IDG.

Europe. The first reason has already been noted, namely, that, in contrast to the situation in Europe, Communist parties often have been acceptable coalition partners in the Indian states. Consequently, a large Communist party presence does not, as in Europe, ipso facto reduce the coalition possibilities. Secondly, the Communist parties in India have been more effective than most parties in maintaining internal cohesion and discipline in their legislative parties and in preventing defections to other parties. The importance of indiscipline and defections in general in explaining government instability in India will be explored presently. As far as the Communist parties are concerned. however, their internal cohesion means that once the party decides either to participate in or to support a coalition government, its support can be counted upon and is not subject to sudden change because of individual or group defections.

In general, this analysis of the impact of four variables measuring the structure of the legislature on the duration of coalition governments indicates clearly the critical importance of treating one-party and coalition governments entirely separately. Correlations done for the two types of governments combined mask the fact that associations between an independent variable with duration for all governments may reflect only the differences between the two types. A second point that is suggested by this analysis is the uniqueness of the European experience of Communist involvement in the parliamentary system. In India, in contrast to Europe, Communist strength in the legislature is correlated positively with the stability of coalition governments. Third, the multiple correlation for three of the four variables used so far $(N_p, CP, and P_p, eliminating F_p)$ because of its high intercorrelation with P_p) is .416, with an R^2 of .173. Thus, the combined effect of the variables used here measuring the structure of the legislature accounts for only 17 percent of the variance in the stability of coalition governments. Clearly, we must look to other factors to explain further the varying duration of coalition governments in the Indian states.

The Composition of the Government. Six variables were selected to measure various aspects of the composition and character of coalition governments and to test several hypotheses concerning the relationship between government composition and duration. The first variable concerned the majority or minority status of the government. It is evident from Tables 1 and 2 that minority coalition govern-

ments were the most unstable form of government in the Indian states in the period under review and that the mean duration and range of such governments was considerably smaller than the corresponding figures for majority coalition governments. There are two evident reasons why minority coalition governments should be expected to be less durable than majority coalition governments. If the minority government has been formed because ideological disagreements prevent possible coalition partners from joining together, it is likely that controversial issues will soon arise that will precipitate a successful no-confidence vote. If a minority coalition government has been formed because the demands for ministerial payoffs by other potential coalition partners are too high, then it is likely that an alternative coalition will form before long around the disaffected groups. The correlation coefficient for minority and majority coalition governments (MINMAJCO) with duration, using dummy variables, is .428 and the R^2 is .183. Thus, the dichotomous distinction between minority and majority coalition governments explains over 18 percent of the variance in duration.

Two variables used by Taylor and Herman to measure the internal structure of the government-the number of parties in the government (N_{pG}) and governmental fractionalization (F_G) -also were used with the Indian data to test the hypotheses and their findings that duration is negatively correlated with both variables. The correlations reported by Taylor and Herman were r = -.307 for N_{pG} with duration and r = -.302 for F_G with duration. However, these correlations were for all governments, including both one-party and coalition governments and probably reflect in large part the difference between the two types. The more interesting question is whether or not and to what extent the stability of coalition governments is associated with the number of parties in the government and the degree of governmental fractionalization. The correlation coefficients for duration of Indian coalition governments with N_{pG} and F_G respectively, are -.221 and -.219. These correlations hold up also for majority coalition governments treated separately, for which the coefficients are -.247for duration with N_{pG} and -.299 for duration with F_G . All the correlations are in the expected direction.

In an earlier descriptive article on coalition governments in the north Indian states, two other variables were considered in explaining the instability of cabinet coalitions in those states, namely, the extent of ideological differences among the parties in the government and the degree of internal coherence within the parties. It was argued then that, although the cabinet coalitions in north India were ideologically diverse, a lack of coherence within parties was a more important cause of the fall of those governments than interparty ideological differences. Instability threatened most when the balance between opposing coalitions was held by defectors from political parties and nonparty legislators (independents) who shifted their support from one coalition to another.²⁰

In order to test this argument with the larger data base, all the state coalition governments were divided into two sets of dichotomized categories: ideologically nonpolarized and polarized, on the one hand, and coalitions comprised only of parties versus coalitions containing nonparty ministers (independents or defectors), on the other hand. The distinction between polarized and nonpolarized cabinets was a very rough one. Only those cabinets that contained ministers both Left and Right of the Center (that is, on both sides of the Indian National Congress)21 were classified as polarized, whereas all other cabinets were classified as nonpolarized no matter how diverse they were in other respects. This criterion yielded only 8 cabinets that were classified as

²⁰Paul R. Brass, "Coalition Politics in North India," *American Political Science Review*, 62 (December 1968), 1174-91.

²¹For example, the five-party coalition government of Govind Narayan Singh in Madhya Pradesh in 1967–69 was classified as ideologically polarized because it contained Center elements (the BKD and defectors from the Congress) and parties on the Right (Jan Sangh) and on the Left (SSP) as well as a regional party, the KVD of the Rajmata of Gwalior. However, the 13-party coalition government of Ajoy Kumar Mukherjee in West Bengal in 1969–70 was classified as ideologically nonpolarized because all parties in the coalition were Center, Left, or regional parties, but none were parties of the Right.

ideologically polarized and 26 that were classified as ideologically nonpolarized (Table 4). A comparison of the mean duration of the two types of governments showed that the ideologically nonpolarized governments lasted longer on the average (435.8 days) than the ideologically polarized governments (237.6 days). The r with duration, using dummy variables, was .237. Unfortunately, this division of cabinets into polarized and nonpolarized does not provide a satisfactory independent test of the role of ideology because of the fact that seven of the eight polarized governments also contained independents and defectors.

The division of coalition governments into party governments and governments containing independents and defectors yields a more symmetrical division into 18 party governments and 16 governments containing independents and defectors. A government was classified in the latter category no matter how many independents or defectors it contained, the range being from 8 to 100 percent independents and/or defectors in the cabinet. The assumption here is that the presence of even a single independent or defector in a cabinet usually reflects a precarious balance of legislative forces which would allow opportunists to be arbiters of the fates of governments. A comparison of the mean duration of the two types of governments supports the hypothesis that such governments are considerably more unstable than party governments and that a lack of coherence in legislative parties is a major source of instability in coalition governments. The mean duration of coalition party governments is 473.9 days, that of coalition governments containing independents and defectors is only 274.7 days. The correlation coefficient for duration of coalition governments in days, with dummy variables coded 1 for party governments and 0 for governments containing independents and/or defectors, is .277-a slightly stronger correlation than that obtained for

Table 4. Mean Duration of Coalition Governments by Ideological Composition and Party and Nonparty Composition

Type of Government	N	Mean Duration		
Nonpolarized	26	435.8		
Polarized ^a	8	237.6		
Parties only	18	473.9		
Containing independents and/or defectors	16	274.7		

^aPolarized governments are defined as cabinets that contained parties both Left and Right of the Center. A broader definition of polarized governments that included Left-Center coalitions also as polarized reduced the difference of means between polarized governments and nonpolarized governments to 294.7 days and 442.5

polarized and nonpolarized governments.

The importance of opportunism as a source of cabinet instability in India is supported further by a comparison of the data on duration of party governments versus governments containing defectors and/or independents, with minority governments excluded. The results are similar to those obtained with minority governments included. The mean duration for 13 majority coalition governments that did not contain defectors or independents was 560.7 days, that for 8 majority governments that did contain defectors or independents was only 389 days, and the r with duration, using dummy variables, is .265. In other words, opportunism is an important source of cabinet instability in India for majority as well as minority coalition governments.

In considering the role of ideology versus opportunism and the effect of party cohesion or the lack of it in explaining cabinet instability, we are dealing with nontrivial issues on which argument has long raged in comparative politics, particularly with reference to explaining the greater instability of French cabinets in the Third and Fourth Republics as compared with other European multiparty systems.22 While the data from India do not permit any comment on the debate concerning the relative roles of ideology and opportunism in determining the fates of French coalition cabinets, they do lend support to the broad argument that coalition governments face particular difficulties in multiparty systems in which political parties do not monopolize access to power and in which party cohesion is not firmly maintained by all parties. In modern Europe, only Fourth Republic France, whose governments were the least stable on the Continent in the postwar period, operated a parliamentary system with uncohesive parties.23 And, in the Indian states, nonparty legislators and defectors from uncohesive parties play critical roles in the fall of coalition governments.

So far, I have been analyzing government stability and instability in the Indian states as if it were uninfluenced by external forces. In fact, that is far from the case. The central cabinet has the power to terminate any state government in India when the state governor-who is a central appointee-reports that no stable government can be formed. But even more important, the central leaders of the Indian National Congress and of other parties make decisions that influence whether or not a government will be formed and which political parties will participate in it. Moreover, party splits originating in leadership conflict at the national level may precipitate party splits in the states that will in turn lead to a loss of support for particular state governments. The national leadership of the Indian National Congress also often worked actively in a variety of ways to undermine the stability of state governments, both those controlled by opposition parties and those controlled by opposing factions in the Congress itself.

Two noteworthy examples of interference by the central leadership of the Congress in state government affairs occurred in 1963 and then again in the period after the split in the Congress in 1969. In 1963, when the so-called Kamaraj Plan was implemented, all the Congress chief ministers in all the states submitted their resignations to Prime Minister Nehru, who then decided which resignations to accept and which to decline to accept. In this way six state governments were terminated and their chief ministers replaced at one stroke. In the period between 1969 and 1974, Mrs. Gandhi and her political emissaries involved themselves directly and often successfully in the affairs of several states in order to replace hostile chief ministers-whether in Congress or out-with chief ministers favorable to her and her party.

It was commonly charged by opposition party leaders in those years that the national leadership of the Indian National Congress frequently sent emissaries to state capitals when non-Congress governments were in power to persuade, wheedle, cajole, and bribe opposition politicians to defect to the Congress and thus bring down non-Congress regimes. It also was often charged that the state governors acted in a manner prejudicial to the interests of non-Congress governments and recommended their termination at the bidding of the central government when internal events did not justify it. Since external intervention was used to bring down Congress as well as non-Congress governments, it is difficult to devise a measure to convert this charge into a testable hypothesis. The coalition governments were, however, divided into Congress and non-Congress governments to test the hypothesis that non-Congress coalition governments, most of them lacking the support of the central government and

²²On the Fourth Republic, see esp. MacRae, Chs. 4-7, and the interesting critique of MacRae's argument in David M. Wood, "Responsibility for the Fall of Cabinets in the French Fourth Republic, 1951-1955," American Journal of Political Science, 17 (November 1973), 767-80.

²³Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger, 1967), 334-40.

often incurring its hostility, were likely to have been less stable than coalitions in which the Congress was a partner. In addition, coalition governments allied with the Congress were likely to have been more stable because the Congress in all but a few cases was the largest party in the legislatures. Non-Congress coalition governments were, in fact, less stable on the average, with a mean duration of 312.1 days, than Congress coalition governments, which lasted an average of 522.6 days. The correlation coefficient for duration, with dummy variables for Congress and non-Congress governments, is .264. Thus, the hypothesis that non-Congress coalition governments were likely to have been less stable than Congress coalition governments is supported by the data.

Regression equations were run in order to calculate the combined effect, first, of the variables measuring governmental structure and, second, of both sets of legislative and governmental variables. The regression equations established clearly that the governmental composition variables were more important than the legislative structure variables in explaining the variance in duration of coalition governments. In an equation run with only governmental variables, both MINMAJCO and CONG-NON entered the equation at a significance level of better than .05, with an adjusted R^2 of .247. The ideological character of the government contributed only marginally to the R^2 on the third step and IDG (the dummy variable for presence or absence of independents or defectors in the government) had no effect on the equation. In the equation with both sets of variables entered, MINMAJCO, CONGNON, and N_p entered the equation in that order at significance levels better than .05, with an adjusted R^2 of .347. The ideological character of the government again contributed only marginally-this time on the fourth step-to the cumulative R^2 and again IDG had no effect.

These results, however, were influenced strongly by one case in the data, the government of Mohan Lal Sukhadia of Rajasthan which endured for 1537 days from 1967 to 1971. This government began as a Congress minority government, but was later expanded to a majority government by the inclusion of several defectors in it. Because it was classified in the data as a Congress majority coalition government containing independents and defectors, its inclusion enhanced the importance of the variables, MINMAJCO and CONGNON, and decreased that of IDG. In the regression equations run with this case excluded from the data, MINMAJCO and N_p again contributed

significantly to the R^2 , but IDG replaced CONGNON in the equation and contributed more than the other variables to the cumulative adjusted R^2 of .365. Consequently, it would not be correct on the basis of the results of the previous regression equations to dismiss the importance of independents and defectors as precipitants of cabinet instability in coalition governments.

In summary, the following conclusions appear justified by the data from the regressions. Of the two sets of variables, it is clear that the composition of the government is more important than the structure of the legislature at the time of the election. However, the number of parties in the legislature also influences the prospects for cabinet stability and instability such that the greater the number of parties in the legislature the less likely are its governments to endure for long. Of the governmental variables, the majority or minority status of the government is of undoubted importance in explaining duration. The case of the Sukhadia government indicates that where a single party has a near-majority of seats in the legislature, the induction of independents and defectors into the government may contribute to its endurance; but, otherwise, independents and defectors play a critical role in undermining governments. Finally, the limited evidence available also suggests that the ideological composition of the government has played at best only a marginal role in the duration of Indian state coalition governments.

Party System Institutionalization and Stability in Coalition Governments. A suitable indicator was sought for a third type of variable that would measure the degree to which the party system as a whole had become institutionalized. The hypothesis tested here is that cabinet instability results in situations where the parties lack stable bases of support in the electorate-that is, they undergo dramatic fluctuations in support-and do not persist over time to the extent of winning seats in the legislature within a predictable minimum and maximum range, or where new parties arise and make inroads into the established bases of support of the principal parties. The assumption behind this hypothesis is that the more dramatically party membership in the legislature fluctuates the more likely it is either that the parties are dependent upon the support of local notables, political entrepreneurs, and opportunists or that new parties have been created that draw on such support to achieve quick success. Such parties are likely to be less able to maintain internal cohesion in their legislative groups than more institutionalized, stabler parties that return a larger core of persons dependent on the party for their election (rather than vice-versa) and a larger percentage of incumbents.

A rough measure of party system institutionalization that takes into account these features of a state's party system is provided in the following formula:

$$PSI = \frac{W}{C} - \frac{L}{C}$$

In this formula, party system institutionalization (PSI) for each election equals the ratio of seats won by all parties that have won at least one seat in the state's legislature in any election since 1952 (W) to the number of seats contested by such parties (C) minus the ratio of the number of security deposits forfeited by the candidates of those parties (L) to $C.^{24}$ This measure, therefore, ignores independents and completely unsuccessful parties and records only the scores of the successful parties. However, it is sensitive to the rise and fall of parties. For example, a party that was successful in an early election, but is on the decline and loses all seats contested and many of its deposits in a later election will influence PSI in a downward direction for that election. Similarly, a new party that enters an election for the first time and wins one seat, but loses several security deposits also will reduce the size of PSI. If, however, the same parties in a state contest every election and win more seats than they lose deposits, the overall PSI for each election in that state will be positive and relatively high. Therefore, states whose party systems are dominated by parties that persist over time, that regularly win a number of seats, and that know their areas of strength well enough not to field candidates who lose their security deposits will score higher on this measure than states where parties fluctuate in strength, where new parties are rising to challenge older parties, and where such challenges leave few parties with persistent areas of stable support. The maximum score on this index is +1.0 and the minimum is -1.0, but the effective range in genuinely competitive party systems will for the most part move between +.50 and -.50. (Perfect two-party competition where only two parties contest all

24Candidates in Indian elections are required to pay a security deposit which they forfeit if they poll less than one-sixth of the vote in single-member constituencies and less than one-twelfth in doublemember constituencies. seats, where each party wins half the seats, and where neither party fields any candidates who lose their deposits will result in a score of +.50.)

The correlation coefficient for duration of coalition governments with PSI is .534. Thus, although PSI is not a direct measure of the structure of the legislature and tells nothing about the composition of particular governments, it is a better explanatory variable for duration of coalition governments than any of the direct measures used before. A regression equation was run which included all the independent variables not highly intercorrelated (Table 5a). Six of the independent variables entered the equation at a significance level of .274 or better. The amount of variance explained by these variables, adjusted for the number of variables and the number of cases, is 42.7 percent, but it is apparent that PSI makes by far the largest single contribution to the equation and dwarfs the importance of all other independent variables.

In this equation as in the previous equations, the results have been influenced strongly by the single case of the Sukhadia government. With the Sukhadia government removed from the data, IDG enters the equation at the second step and CONGNON drops out of it altogether (Table 5b). Otherwise, the relative importance of the other variables does not change significantly. IC, the variable measuring ideological polarization, could not be entered into either of these equations. The strong correlation between duration and PSI and its importance in the regression equation is supportive of the general arguments that have been advanced above and adds a further element to it so that the argument can now be restated as follows: The stability of coalition governments in India depends principally upon the degree to which the legislature and the government are dominated by the larger, more cohesive, and more institutionalized political parties. Coalition governments are likely to be short-lived (1) either when the existing parties have not institutionalized their support adequately or when new parties arise to challenge the institutionalized support of the existing parties; (2) when the parties in the governing coalition either lack internal cohesion or do not have a majority of seats in the legislature; or (3) when there are a large number of small parties in the legislature. Under these conditions, the uncertainty of support and the weakness of parties provide an atmosphere in which political opportunism and party defections flourish and in which the fates of governments depend upon the shifting support of political entrepreneurs.

Table 5a. Multiple Correlation of Coalition Government Duration with Variables Measuring Legislative Structure, Government Composition, and Party System Institutionalization (N = 29)

Variable Entered	ь	Significance	Multiple R	R ²	R ² Change	Adjusted R^2
PSI	699.8	.002	.547	.300	.300	.300
CONGNON	195.7	.098	.609	.371	.071	.348
MINMAJCO	187.3	.268	.634	.401	.031	.355
N_p	-29.5	.274 -	.657	.431	.030	.363
P_{D}^{P}	-23.2	.095	.705	.498	.066	.414
ĆP	- 6.8	.238	.727	.529	.031	.427

Table 5b. Multiple Correlation of Coalition Government Duration with Variables Measuring Legislative Structure, Government Composition, and Party System Institutionalization, Sukhadia Government Excluded (N = 28)

Variable Entered	b ·	Significance	Multiple R	R^2	R ² Change	Adjusted R^2
PSI	553.4	.002	.554	.307	.307	.307
IDG	188.3	.016	.673	.453	.146	.432
N_p	-20.6	.351	.688	.473	.020	.431
P_n^{ν}	-16.9	.193	.715	.511	.038	.450
P_p CP	- 5.1	.323	.730	.533	.022	.452
MINMAJCO	118.0	.305	.746	.556	.023	.455

One-Party Minority Governments

One-party minority governments operate under quite different constraints from those that affect the stability of coalition governments. Minority governments exist and endure because some parties and individuals in the legislature, though unwilling to join the minority government, prefer to support or at least not oppose it rather than try to replace it with an alternative coalition or force a breakdown and a new election. In contrast to coalition governments, the endurance of one-party minority governments should logically be expected to depend more on the degree of cohesion and fragmentation in the legislature. Whereas a majority coalition government can maintain itself in power as long as the parties in power can retain the loyalty of their following in the legislature, the ability of a one-party minority government to endure depends upon both the loyalty and discipline of the governing party and the loyalty and discipline within the parties supporting the government. A split in a large supporting party may cause a loss of support for the government. A highly fragmented legislature may require the government to depend upon the internal discipline of several smaller parties. The greater the number of such parties, the greater the danger that one or more of them may withdraw its support or may divide internally on the question of support for the existing government.

In fact, the data on one-party minority governments in India show that the stability of this form of government is more dependent on the structure of the legislature than are either coalition governments or one-party majority governments. This adds further support to the argument that the prospects for governmental stability increase the more the legislature is dominated by parties and decrease the more the legislature is composed of fragmented groups and independents. All the correlations for one-party minority governments between duration and measures of party dominance are positive-.296 with percent seats held by the governing party, .433 with P_p , and an even stronger .553 with percent seats held by the two largest parties in the legislature (W_p) . In contrast, three measures of legislative fragmentation $-N_p$, F_p , and F_o (fractionalization of the opposition)—correlated negatively with duration: -.548 for N_p , -.575 for F_p , and -.430for F_o . The multiple correlation for two of these variables, N_p and F_p , was .695, with an adjusted R^2 of .437, explaining 43.7 percent of the variance.

It is also noteworthy that the dominance of large parties in the legislatures is more important for stability than the competitive balance between them. It was hypothesized that one-

party minority governments would endure longer the greater the distance between the percent seats held by the governing party in the legislature as compared with its nearest rival and that, conversely, the more even the balance between the two largest parties in the legislature the quicker a one-party minority government would fall. In fact, these hypotheses were not confirmed. The correlation coefficient for duration in days with the ratio between the percentage of seats held by the governing party and the percentage of seats held by the nearest rival (TPR) was -.093.

One-Party Majority Governments

The hardest part of the task remains, namely, to find some explanation for the great range in duration of one-party majority governments -the largest category of governments (76) in the universe of Indian state governments and the category that contains the broadest range (12 to 4685 days). However, as already noted, the relationships and correlations that seemed to explain the duration of Indian state governments when all governments were taken together do not hold up when one-party majority governments are considered separately. None of the legislative structure variables correlate strongly with duration for one-party majority governments. The governmental composition variables used so far are, of course, not relevant. It would be desirable to construct measures of governmental composition that would take into account the factional composition of one-party governments, but such data are not readily available.

The highest correlation found between duration of one-party majority governments and any of the measures used so far in this article was that with PSI which was .228. It should be remembered, however, that PSI is not a direct measure of the structure of the legislature, but an indirect measure of the degree of party institutionalization in the electorate. It suggests something about the character of the parties that hold seats in the legislature, but it has nothing to do with the degree of fragmentation/cohesion in the legislature as measured by

the percentage of seats held by the parties. Moreover, the correlation for PSI with duration of one-party majority governments is not nearly as high as it was for coalition governments.

. Only three of the variables discussed in this section, PSI, N_p , and TPR entered a regression equation at a significance level of .25 or better (Table 6). Altogether, these variables explain only 7.6 percent of the variance in the duration of one-party majority governments. P_p and CP were eliminated from the regression equation because they did not make any additional contribution to the adjusted R^2 after these three variables were entered. Thus, there are no evident and strong patterns of relationship between the structure of the legislature or the competitive balance among the parties in the legislature that explain much of the variance in the stability of one-party governments.

The absence of any clear relationship between government stability and the competitive relations among parties in the legislature means clearly that we must look elsewhere to explain the relative duration of one-party majority governments. Two broad possibilities may be suggested. One is that factors external to the legislative system may be at work. The other is that the internal relations between factions and between leaders and rank-and-file in the dominant party may be more important than relations between the dominant party and its rivals.

Table 7 provides a breakdown of the oneparty majority governments by duration in days and reasons for termination (eight categories) for those governments where data were available. It is immediately apparent from this table that the principal type of termination of one-party majority governments in India has been an interelection change of the chief minister without a change in party. Such changes, which almost always involve some intraparty factional conflict,²⁵ have accounted

²⁵It would, of course, be desirable to develop a formal indicator of intraparty factionalism in the Congress legislature parties and in Congress governments to correlate with duration, but the fluidity of factional alignments in the Congress is such that an indicator of this sort could be constructed only in

Table 6. Multiple Correlation of Duration with Three Variables for One-Party Majority Governments (N = 76)

Variable Entered	Significance	Multiple R	R ²	R ² Change	Adjusted R ²
PSI	.063	.228	.052	.052	.052
N_n	.150	.287	.083	.030	.071
<i>N_p</i> TPR	.254	.318	.101	.012	.076

for 30 of the 74 terminations or 40.54 percent. The important question for our purposes is whether such changes in the chief ministership have been distributed unevenly. One hypothesis might be that the longer-lived governments have been led by more skillful chief ministers who were better able to prevent factional conflict, whereas the shorter-lived governments have been led by less able leaders whose governments were more readily brought down by internecine conflict. In fact, the available data do not support this hypothesis at all. A higher proportion of the longer-lived governments, 17 out of 40 or 42.50 percent that have lasted more than three years, have been terminated by an internal change of leadership, whereas a smaller proportion of the shorter-lived governments, 13 out of 34 or 38.24 percent, have been so terminated. It should be stressed here, however, that these data do not disconfirm the hypothesis that skillful management of internal factional conflict may provide the principal explanation for the relative endurance of oneparty majority governments. It may still be that internal factional conflicts are endemic in most Indian states and that some chief ministers simply manage to control and balance such conflicts more effectively and for longer periods than others, but that ultimately even the most skillful leaders may be forced out by them.

Before taking up this last point, the question of leadership should be placed in a broader context by considering all the types of government terminations. It makes sense to divide these types into two broad categories: (1) those

approximate form and only after extensive documentary research.

that end in a procedurally regular way (general election) or by direct intervention of the central government (either by dissolving or changing the status of the political unit in question or by inducting the chief minister into the central government) or by accidental circumstances (death of the chief minister); and (2) those that terminate through changes in the legislature leading to resignation of the chief minister and/or a change in the party composition of the government or a complete breakdown in stability and the consequent imposition of President's Rule. The rough distinction being made here is between forces that are more under the control of a state political leader and those that are less so. The hypothesis to be tested is basically the same as that tested for interelection changes in the chief minister, namely, that governments that endure longer will terminate for reasons less under the control of the leader, whereas short-lived governments will terminate for reasons that reflect at least in part upon the leadership skills of the chief minister. In fact, there is not much support for this hypothesis either. Half the governments that lasted for more than three years terminated for "external" reasons, half for "internal" reasons, whereas 47.06 percent of the governments that lasted less than three years terminated for "external" reasons and 52.84 percent for "internal" reasons. Clearly, there are hardly any differences between longer-lived and shorter-lived governments in their forms of termination when the data are broken up in this wav.

A further test of this hypothesis was carried out through correlation analysis by assigning points to each form of termination on a scale of 0.0 to 1.0, with the "external" factors assigned a score of 1.0, a resignation followed by a

Table 7. Cross-Tabulation of Duration of One-Party Majority Governments in Days with Reasons for Termination

	Duration in Days					
Reasons for Termination	1826+ N = 17	1001 - 1825 $N = 23$	366-1000 N = 26	1-365 N = 8	Total N = 74	
General election	3	5	3	0	11	
Dissolution, reconstitution, or change in						
status of state	0	5	6	1	12	
Death of chief minister	4	ĺ	4	1	10	
Transfer of chief minister to union		-				
government	1	1	1	0	3	
Resignation of government followed by	_	_	_	_	_	
reformation	1	0	0	0	1	
Interelection change of chief minister	8	9	10	3	30	
Change of chief minister/party composition	Ö	1	0	2	3	
Imposition of President's Rule	Ŏ	ī	2	1	4	

reformation under the same chief minister a score of 0.50, an interelection change of chief minister a score of 0.25, and a change of both the chief minister and the party composition of the government or a breakdown leading to President's Rule each assigned a score of 0.0.26 The principle behind the assignment of points for each termination is that the higher scores are assigned to terminations less under the control of the chief minister and the lower scores to terminations that reflect more on the chief minister's leadership abilities, with the lowest scores being assigned to situations involving the greatest deterioration in the dominance of both the leader and/or the governing party. Two correlations were run, one for all terminations and one in which terminations of governments caused by dissolutions, reconstitutions, and changes in the status of the states were excluded. The correlation between duration and form of termination for all one-party majority governments was .115 and for governments with status changes in the states excluded was .204. Thus, there is only modest support for the hypothesis that longer-lived one-party majority governments terminate under conditions that reflect more favorably on the leadership capacities of the chief minister, whereas shorter-lived governments terminate under conditions suggesting less skillful political leadership.

Leadership and Factionalism in One-Party Dominant Systems. Although the correlations just noted are weak, they are at least in the hypothesized direction and suggest that it is worth probing deeper into the events surrounding the termination of those governments whose ends were brought about by "internal" causes. A closer look at the events surrounding 38 internally caused terminations suggests some further distinctions of relevance to the leadership question. For example, some chief ministers have resigned for "health reasons" that do not appear to be reasons of political health, others were asked to resign by the central leadership of the Congress even though they continued to command a firm majority in the

²⁶In assigning points to government terminations in this way, I have adapted an idea developed by Leon Hurwitz for classifying types of government formation in "An Index of Democratic Political Stability: A Methodological Note," Comparative Political Studies, 4 (April 1971), 41–68. In the larger study of which this article is a part, I plan to apply Hurwitz's Index of Government Persistence to the Indian states as an alternative measure of government stability to that of duration in days.

state legislative party, some resigned to make way for rivals in the state party after being clearly defeated either in the Congress legislature party or in the organizational wing of the party, the Pradesh Congress Committee (PCC), and some have had to resign because party members defected to the opposition.

Clearly, resignations on legitimate grounds of health do not reflect upon the ability of state leaders to maintain the support of their followers. It is less clear how to assess a forced resignation by the national leadership of the party in comparison with a local defeat of the leader in the legislative party or in the state party organization because of the fact that state and national pressures often reinforce each other. That is, the central leadership of the Congress sometimes intervenes to remove a chief minister who has lost support in state politics. Conversely, a local contest for leadership is sometimes permitted or not opposed by the national leadership when it is hoped that an unpopular leader will be removed in this way without involving the central leadership. Sometimes also, weak and unpopular chief ministers are able to maintain themselves in power solely because the national leadership does not wish them to be removed.

Keeping these reservations in mind, it is nevertheless worth noting that only one-fifth—8 out of the 40 governments that lasted three years or more-terminated in decisive defeats of incumbent chief ministers in either their own party or in the legislature, whereas 11 of the 34 governments or nearly one-third that lasted less than three years terminated in this way. These results again suggest that the question of leadership in the dominant party is worth pursuing further as an explanation for the relative stability of one-party majority governments. However, proper treatment of this question will require qualitative and detailed comparisons of leader-follower and factional relationships in several state parties.

A few examples drawn from the data used in this study will suggest the kinds of questions that need to be pursued in this way. Consider, for example, the case of the second longest consecutive chief ministership on record, the government of Mr. Mohan Lal Sukhadia of Rajasthan that lasted for 4500 days from November 13, 1954, until March 13, 1967. If one counts Mr. Sukhadia's second tenure as chief minister—mentioned above in the discussion of coalition governments—which was separated from the first only by a month of President's Rule in March-April, 1967, Mr. Sukhadia's period in office comprised a total of

8-18-6

6037 days (4500 days between November 13, 1964, and March 13, 1967, and 1537 days between April 24, 1967, and July 9, 1971). Sukhadia's period in office spanned the reconstitution of the state of Rajasthan in 1956 (with the merger of the old state of Rajasthan and the princely state of Ajmer) and three general elections. Sukhadia had to control internal factionalism and had to deal at times with a precarious balance of forces in the legislature. He maintained his position in a fragmented legislature (F_p of .634 for the first tenure and .681 for the second) in which Congress dominance was often not secure. The average Congress majority in Sukhadia's first tenure was 57.36 percent, but the Congress secured only 50.00 percent of the seats in the 1962 legislature. Sukhadia's second span of office began with a minority government, the Congress having secured only 48.37 percent of the seats in the elections of 1967. The average ratio between the Congress vote and that of the next largest party was 4.64 in Sukhadia's first tenure and 1.85 in his second. PSI for the first tenure was .0842 and for the second was .1540. The question raised by Sukhadia's two terms in office is how was he able to maintain power in the face of factional conflicts in the Congress, a fragmented legislature, a not very strongly institutionalized party system, and at times less than a majority for his party in the legislature?

At the other end of the spectrum, similar questions occur in reverse. For example, how can one explain the fact that the very first government in the new state of Haryana, that of Mr. Bhagwat Dayal Sharma, ensconced in power with 59.26 percent of the seats in the legislature and with a TPR of 4.00 was brought down by defections from the ruling party after only 12 days in office? Or, at the other end of the subcontinent at an earlier period of time, how does one explain the fact that the government of Mr. S. R. Kanthi in Mysore, formed after the General Elections of 1962 with a strong Congress majority of 66.35 percent of the seats, was replaced after only 104 days in office by a change in leadership to Mr. S. Nijalingappa? Scholars and journalists familiar with the politics of these states probably have ready answers to these questions, but to be theoretically satisfying, the answers will have to be placed in a comparative context. It will not be sufficient to say merely that one government was brought down by defections, another by internal factional conflict. The questions that need answering must be framed comparatively: for example, what is it about the leadership of Mohan Lal Sukhadia that enabled him to turn minorities into majorities in comparison to the leadership of Bhagwat Dayal Sharma whose majority disintegrated in 12 days? Or, were there differences in the structure and composition of intraparty factions that explain the differences in the fates of these two governments rather than differences in the skills of the two leaders?

Until now, questions concerning leadership and factionalism in Indian politics have been treated only in monographic studies of particular leaders or particular states. Such studies constitute a resource that can be used in looking for theoretical answers to comparative questions concerning the conditions which facilitate or impede long tenure in office and stability or instability in Indian state governments. This article demonstrates not that further case studies of leadership and factionalism in Indian politics are no longer needed, but that the cases must be selected carefully and the questions framed precisely so that the results can be integrated into a general theoretical explanation that specifies the relationships among leadership and factionalism, legislative fragmentation and cohesion, and party system institutionalization in determining the fates of governments.

On the Conditions for Coalition and One-Party Systems in the Indian States

This analysis of the causes of cabinet stability and instability in coalition and one-party systems in the Indian states suggests another larger, but related question concerning the conditions under which different types of party systems arise. It may be, in fact, that the narrower question cannot be answered satisfactorily until the larger question is addressed. The importance of party system institutionalization as an explanatory variable for all types of governments in India suggests as much. For party system institutionalization depends upon the ability of the parties to establish strong, secure, and stable bases of support in the electorate. How successful the parties are in doing so in turn is likely to depend upon the degree of fragmentation and cohesion that exists in the social structure and the ways in which the parties relate to social and cultural cleavages.

Although it is beyond the scope of this article to pursue this question in detail here, some of the problems posed by the Indian data and some hypotheses for further research are suggested by the distribution of coalition and one-party minority governments, as shown in

Table 8. Fifteen of the 39 provincial units that have had some experience with cabinet government in India also have experienced either coalition or one-party minority governments. However, some of those provinces no longer exist and others have had experience with only one or two coalition or minority cabinets. More than 80 percent (42 out of 51) of the coalition and minority governments that have been formed in the Indian states were concentrated in eight states-Bihar (8), Kerala (8), Orissa (6), Uttar Pradesh (6), Punjab (4), West Bengal (4), Madhya Pradesh (3), and Manipur (3). Leaving aside the tiny hill state of Manipur (about whose social structure and political process very little is known), the seven larger states can be grouped broadly into three types-the extremely large (in population or in area), extraordinarily heterogeneous, and socially fragmented states of Bihar, Uttar Pradesh (U.P.), and Madhya Pradesh (M.P.); the smaller states of Kerala, Orissa, and Punjab that are divided socially and culturally by two or three sharp cleavages;²⁷ and West Bengal which is inter-

27For analyses of the relationships between party systems and social structures in these three states, see F. G. Bailey, "Politics and Society in Orissa (India)," Advancement of Science, 19 (May 1962), 25–28; Paul R. Brass, "Ethnic Cleavages and the Punjab Party System, 1952–1972," in Party Systems and Cleavages, pp. 7–69; and Robert L. Hardgrave, Jr., "The Kerala Communists: Contradictions of Power," in Radical Politics in South Asia, ed. Paul R. Brass and Marcus F. Franda (Cambridge: MIT Press, 1973), pp. 134–38.

mediate in size and cultural heterogeneity between these two groups of states. In the first group of states, social and political fragmentation have gone together. Throughout most of their postindependence political history, the Congress has been dominant in these states, but when the Congress failed to win a safe majority of seats, the opposition was too fragmented to provide coherent and stable alternative governments. In two of the smaller states, Kerala and Orissa, the Congress has never been dominant and several political parties have organized successfully and maintained stable support within distinct subcultural segments of the society. Punjab is similar to Kerala and Orissa with regard to the relationship between political parties and social cleavages, but is different from these two states since the Congress has had a stronger position there. West Bengal, like the states in the first group, was a Congressdominated state until 1967. When the Congress lost its majority there after the 1967 election, the opposition was fragmented, but a distinct ideological-political polarization developed in the competition between the Congress and the Communist Party of India (Marxist).

The states that had no experience with coalition or minority governments in the period under review were Assam, Maharashtra, Tamil Nadu, Mysore, and Gujarat. While there are no evident similarities among these states that distinguish them clearly from the states that have experienced coalition and minority governments, they are all smaller than the three

Table 8. Distribution by State and by Duration in Days of Coalition and One-Party Minority Governments

	Duration in Days							
	1001-1825		366-1000		1-365			
State	Coalition N	One-Party Minority N	Coalition N	One-Party Minority N	Coalition N	One-Party Minority N		
Andhra			1					
Bihar					8			
Goa		1						
Haryana		•			1			
Kerala			3	3	1	1		
Madhya Pradesh			1		1	1		
Madras			1					
Manipur			1		2			
Orissa	1	1	2	1		1		
Pepsu	•				1	1		
Pondicherry		1			1			
Punjab			1	1	2			
Rajasthan		1						
Uttar Pradesh				1	2	3		
West Bengal	_		_1	_	_3			
Total (51)	1	4	11	6	22	7		

huge and heterogeneous states of Bihar, U.P., and M.P. None of the states except Assam has developed a pattern of political organization of subcultural cleavages that characterizes the states of Kerala, Orissa, and Punjab. Finally, one of the states, Tamil Nadu, is distinctive in the extent to which it has developed a cohesive political culture based on Tamil nationalism.

While these broad distinctions are only suggestive, they do point the way to two hypotheses concerning the conditions under which coalition and minority regimes are likely to arise and concerning the stability of such regimes. One hypothesis that clearly needs to be explored further is that such systems arise in social orders characterized either by extreme heterogeneity and fragmentation or by a more or less sharp social-political organization of two or three subcultural cleavages. A closer examination of the data in Table 8, which shows that half the coalition and one-party minority governments that have lasted less than a year have been in the two states of Kerala and Orissa and that 13 of the 29 coalition and one-party minority governments that have lasted less than a year have been in the two states of Bihar and Uttar Pradesh, suggests a second hypothesis. The differences between these two groups of states suggests that systems that are characterized by the political organization of a few distinctive subcultures are likely to produce more stable patterns of coalition and minority government than extremely fragmented and heterogeneous systems. This second hypothesis links up with the relationship established in this article between cabinet stability and party system institutionalization. The party systems in Kerala and Orissa are more institutionalized than those in Bihar and Uttar Pradesh in the sense that the principal parties in the two smaller states have persisted longer, have undergone less dramatic fluctuations in popular support, and have resisted more effectively the inroads of new parties. (These differences are reflected in the mean PSI indexes for those states for all elections held between 1952 and 1972, which are as follows: +.066 for five elections in Orissa, +.244 for seven elections in Kerala, -.150 for six elections in Bihar, and -.218 for five elections in U.P.)

It also deserves to be noted that Kerala and Orissa are quite distinguishable from Bihar and U.P. with regard to the roles played by party defectors in government formation and termination. Although defectors played roles in the formation and termination of three governments in Orissa and Kerala, 28 they were not as

significant a force as in Bihar and U.P. between 1967 and 1972 when nearly every government either contained defectors or depended upon the shifting support of defectors for its endurance. Moreover, in the post-1967 period when most governments formed in all four states were either coalition or one-party minority governments, there were numerous defections affecting nearly all parties in Bihar and U.P. but no defections in either Orissa or Kerala.²⁹ These associations between social organization, party institutionalization, opportunism, and government instability suggest a possible causal sequence, namely, that a segmented social structure produces party institutionalization (or vice versa), which in turn reduces opportunism and enhances the prospects for stable government. In contrast, a fragmented society may restrict the ability of parties to become institutionalized, leaving opportunities for political entrepreneurs whose ceaseless maneuverings decrease the prospects for stable government. On the other hand, and by the same logic, it may be hypothesized that social orders with either a dominant social segment-such as a caste or a class-or a homogeneous political culture are more likely to provide the conditions for one-party dominant or competitive two-party systems. These linkages and possible causal relationships cannot be explored further in this article, for they require the development and comparative analysis of a whole new set of data on the internal social and cultural fragmentation and cohesion of the Indian states and on the relationships between social structure and party organization. The extension of the study of cabinet instability into this broader area of social and political structure does, however, appear to be necessary if a more complete theoretical explanation of its causes is to be developed.

Conclusions

This article began with the purpose of replicating, with data drawn from the universe of Indian state governments, hypotheses derived from Western parliamentary experience concerning the causes of cabinet instability. Several of the principal hypotheses from the

²⁸Government of India, Ministry of Home Affairs,

Committee on Defections: Proceedings of the Committee and Papers Circulated to the Members (New Delhi: Government of India Press, 1969), pp. 48-49.

²⁹Ibid., esp. pp. 67-73. See also Subhash C. Kashyap, *The Politics of Defection: A Study of State Politics in India* (Delhi: National Publishing House, 1969).

Taylor and Herman study were tested with the Indian data. All their hypotheses tested here were supported by the data from the entire universe of Indian state governments, but in all cases at lower levels of explanation than in their study. Moreover, the correlations differed markedly when the different types of governments—coalition, one-party minority, and one-party majority—were considered separately. In addition, there remained a great range of variability in the duration of all three types of governments.

Consequently, correlations were run separately for each type of government. It proved easier to arrive at some general conclusions concerning the stability of coalition and one-party minority governments on the basis of available quantitative data than to do so for one-party majority governments. It was clear that much of the variance in the stability of governments in India was explainable first in terms of the generally much longer average duration of one-party majority governments. It also was clear that a part of the variance in the duration of coalition governments was attributable to the distinction between majority and minority coalition governments.

The less evident and more theoretically important findings on coalition and one-party minority governments concerned the relative roles of ideology, opportunism, party cohesion, and party system institutionalization. Coalition governments in general were the most unstable form of government in India in the period under review-more unstable even than oneparty minority governments. Even so, there was a substantial variability in the duration of such governments. Among the factors considered in seeking an explanation for that variability, it was found that ideological polarization as defined in this article did not explain much of the variance, but that a lack of party cohesion in the legislatures and the presence of opportunism were more critical factors. Opportunism and a lack of party discipline in the legislature in turn were related more broadly to a low degree of party system institutionalization in the electorate. Instability promoted by opportunism and party defections was most prominent among coalition and one-party minority governments when a state party system as a whole was undergoing realignment, when old parties were losing support and new parties were rising to challenge the old, or when party dominance itself was challenged by increasing numbers of successful nonparty candidates.

Variability in the duration of one-party majority governments was harder to explain.

None of the quantitative measures alone explained more than 5 percent of the variance in the duration of one-party majority governments, and three variables entered into a regression equation together explained no more than 7.6 percent of the variance. Leadership and the relationship between leadership and factions in the dominant party appear to be the critical factors in the relative stability of these governments, but leadership is difficult to express in quantitative terms. The indirect measure of leadership skill used in this article provided only very weak support to the hypothesis that leadership skill is the critical independent variable in explaining the relative duration of one-party majority governments; further probing of the data, however, suggested that the problem may lie in the absence of good measures rather than in the lack of importance of the leadership variable. Questions raised by consideration of a few concrete examples suggested that careful selection of cases-particularly the apparently anomalous cases of long and short tenures of chief ministers-for documentary and field research is indispensable for the further development of theory on the causes of government instability in one-party dominant systems.

The tentative findings and hypotheses preesented above on the issue of leadership in one-party dominant systems suggest also a methodological argument, namely, that the development of theory in comparative politics depends upon maintaining a close relationship between quantitative data analysis and qualitative case studies. If the indiscriminate proliferation of unsystematically selected case studies in comparative politics, particularly in developing countries, is no longer considered methodologically useful, neither should the indiscriminate proliferation of correlation and regression analyses that leave us with most of the variance still to be explained. What this study suggests is that case studies continue to be necessary in comparative politics, but that they should be selected only after evident regularities have been demonstrated, the amount of unexplained variance specified, and the anomalous cases that hold promise of completing the explanation identified.

A brief review of the types of states in which coalition and minority governments have been most common also suggested a possible link between theories of cabinet stability and theories relating party systems to different types of social structures. A causal relationship was hypothesized in which the relationship between political parties and social and cultural seg-

ments of the society affects party system institutionalization which in turn affects party cohesion in the legislature, the scope available for opportunists, and hence the stability or instability of governments. In this model, the emphasis has been placed on weaknesses of organization and of institutionalization of party systems rather than upon the immobilizing effects of ideological differences between par-

ties as the principal causes of government instability in the Indian states. Although the evidence from the data does not provide a clear demonstration that ideological differences between parties are or are not obstacles to stable government, it does indicate strongly that, in the Indian states, political fragmentation, weak institutionalization, and personal opportunism are more critical factors.

National Elite and Local Politician in Taiwan*

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In Taiwan, a national elite socialized to measure the behavior of others (but not its own behavior) against extremely high standards of morality finds itself offended by the apparently immoral behavior of local electoral politicians. With reference to similar situations in other developing countries, this article describes how the attitudes of Taiwan's national elite toward the political morality of others developed and why the electoral politicians act in a manner offensive to the elite's attitudes. As a postscript, the article discusses why the national elite has checked its desire to do away with Taiwan's offensive fledgling electoral system and expanded its scope instead.

In 1965, Myron Weiner published an article entitled "India: Two Political Cultures." In

*This article summarizes my research findings concerning provincial and local politics in Taiwan and integrates these findings with related findings concerning Taiwan and from the general field of comparative politics. My own findings are based on the reading of materials published in Taiwan and on interviews conducted in Taiwan between the summers of 1965 and 1968. Interviews, taking the form of informal conversations, were guided by a standardized series of questions, in combination with a feel for the most productive line of questioning at a given moment. Interviews ranged from single hour-long sessions in the relatively formal atmosphere of an interviewee's of-fice, to repeated informal contacts in a variety of surrourdings, often including the interviewee's own home. In all, hundreds of hours were spent in Taiwan's various counties and cities, conversing with provincial, county, and city assemblymen; provincial, county, and city assembly staff workers; citizens active in local political campaigns; local newspaper reporters; government officials from the central, provincial, and county-city levels; local and foreign social scientists; and foreign officials stationed in Taiwan. Many hours were also spent in talks with members of Taipei's mainlander community.

Based on long and repeated talks with informants, my findings do not lend themselves to statistical statements. The sources which I use to bolster and expand upon my interview evidence—newspapers, magazines, books, and official documents published on Taiwan and research reports about Taiwan published abroad—are also rarely of a statistical nature. However, in footnotes I do include sources based on statistical research when they apply. Note that my interview evidence stands behind most statements in this article. Footnote citations, therefore, are limited to those sources which bolster and expand upon this evidence.

I am indebted to J. Bruce Jacobs for his careful reading and his criticisms of the original draft of this article. Of course, only I am responsible for all mistakes which remain uncorrected.

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¹Myron Weiner, "India: Two Political Cultures," in Political Culture and Political Development, ed. Luci-

this article, Weiner described an elite political culture centered around India's bureaucracy and national rulers, and a mass political culture centered around India's state and local politicians. According to Weiner, these two political cultures represented very different views of the way politics should be carried on in India, and if political statesmen capable of bridging the gap which separated them did not come forward, a potential conflict between the two was likely to develop. Such a conflict, Weiner feared, would endanger the continuation of democratic politics in India.

Weiner noted that these two political cultures and the gap between them were not unique to India, but at one time represented a general condition throughout much of the developing world. Today this condition still exists in those few countries in which the national elites have not yet completely stifled local level democracy in an effort to rid their polity of the rival political culture.

Taiwan is one of those few developing countries in which the political culture of the national elite and the mass political culture still confront one another. In this article, I will describe how these two political cultures came to exist on Taiwan and the trend of Taiwan's politics under the influence of their conflicting relationship. Unlike the pessimistic prognosis for democracy in most of the developing world, however, my conclusion will be that, given the political isolation of Taiwan's national elite in the world today, there is reason to hope that the trend of Taiwan's politics will be toward an accommodation by the national elite with the mass political culture.

an W. Pye and Sidney Verba (Princeton: Princeton University Press, 1965). See esp. pp. 227-28, 243-44.

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While reading my description, the reader should bear in mind that although the text stays close to the reality of Taiwan, most aspects of the description are congruent with the experience which other societies have had concerning the development of the two political cultures and their subsequent conflict. This congruency is not complete, however. While it does include aspects that are often regarded as uniquely Chinese, the aspects of land reform and the possible amelioration of the conflict between the two political cultures are not to be found in many other societies.

A Note on Comparative Methodology. Since I will be describing the conflict between the behavior of local politicians and the ideals of Taiwan's national elite, the reader may be disturbed by a lack of parallelism. Should not behavior be contrasted with behavior and ideals with ideals? I persist in this lack of parallelism because the relevant conflict on Taiwan—as it is (or was) in many other developing countries—is between the actual behavior of electoral politicians and the ideals of the national elite.

The political behavior of the national elite is relevant to this discussion only as a dependent variable, dependent on the perception by the national elite that its ideals are being violated by the behavior of local politicians. It is this perception which motivates the national elite to behave in such a way as to restrict the behavior of local politicians.

The political ideals of the local politicians are even less relevant to this discussion. If investigated, one will find some of Taiwan's local politicians holding ideals quite inconsistent with their behavior and rather in agreement with the ideals of the national elite. However, as will be noted below, other local politicians are well aware of what their behavior is and hold a set of ideals which supports this behavior. Whatever a local politician's ideals are, however, is not relevant to the main thrust of this paper, since these ideals simply do not enter into the local politician behavior in conflict with national elite ideals leads to elite behavioral reaction equation.

A second comparative problem is that this paper is, strictly speaking, discussing aspects of three, not two, political cultures. In addition to the political culture of Taiwan's national elite (primarily represented in this article by the aspect of elite ideals and secondarily represented by one aspect of elite behavior), the "mass political culture" under discussion is divisible into the political culture of Taiwan's ordinary voters and the political culture of

Taiwan's local politicians (both represented in this article primarily by aspects of political behavior). Although closely related, I have taken pains to treat voters and local politicians as distinct groupings.

Some Definitions. Note that for the pre-1946 period (the year in which local elections were instituted), I use the term "local politician" to refer to landlords, heads of large lineages, and any other Taiwanese who were close enough to ordinary individuals living in the countryside to serve as patrons or otherwise importantly influence their lives. For the post-1946 period, the term "local politician" refers to any candidates for elective office from any election district in Taiwan, as well as to their active supporters. This definition is meant to include candidates for national political bodies (elections for which were instituted in 1969) as well as for lower level offices.

I use the term "traditional" to refer to the totality of those sociopolitical practices on Taiwan which, at any given point in time, have not yet felt the influences of: (a) post-1945 economic change, (b) the 1949-1953 land reform program, and (c) the institution of elections in 1946 and important electoral reforms such as those taking place in 1950 and 1961. Note that because the influences of these phenomena made themselves felt at different points in time, because different regions felt these influences in different ways, and because the residual effects of these influences are still being absorbed by the sociopolitical system, my use of the term "traditional" should not be understood as referring to the totality of Taiwan's society at any given point in time. Instead, it should be understood as referring to only those sociopolitical practices which had not yet felt the influences of these phenomena at the time in question.

The World of Taiwan's National Elite and Its Effect on the National Elite's Political Culture

In order to limit the scope of this article to the main points under discussion and to restrict the text to a manageable length, the following description of the world of Taiwan's national elite and the effect of this world on the national elite's political culture will not be comprehensive. It will be confined to the images the national elite holds in its own mind of (a) what a good political system should be, (b) the benefits such a good political system would bring to society, and (c) the role the national elite should play in such a political system. This description will not deal with the images which individuals outside the Taiwan national elite hold in their own minds of the actual political systems which Taiwan's national elite has been identified with or of the national elite's actual role in these systems. These less "subjective" images of the national elite's politics have been extensively treated in other works and need not concern us here.

Note that I am using the term "Taiwan's national elite" to refer to individuals who were born in mainland China, who came to Taiwan in the years after it was returned to China in 1945, and who now occupy national and lower level positions in the government or in the Kuomintang (the ruling political party). It is my strong impression that a large proportion of the middle class mainland Chinese citizens who accompanied this elite to Taiwan, as well as many of their children, share the elite's political culture. Since the mainland Chinese citizens are so close to the national elite and since they are the national elite's strongest supporters in Taiwan, it is conceivable to expand the definition of the national elite to include them.

"Democracy" as a Crucial Element in the World of the National Elite. As it was for the elite of many of the developing nations, democracy was one of the most important goals of Taiwan's national elite. The meaning of democracy for members of the elite, however, is a basic cause of the gap between them and the politicians rising out of the local level electoral system.

The major problem in the minds of national elite members such as those on Taiwan was how to meet the challenge of the European world. The failure of their nation to meet this challenge was causing them much humiliation. Like their counterparts in similarly challenged nations, members of Taiwan's national elite felt—even while growing up on mainland China—that it was an urgent task to restore "wealth and power" to their nation, thus erasing their humiliation.

In the search for means to the end of wealth and power, many elite members turned away from solutions suggested by their own traditional cultures as having already failed to cope with the European challenge. Instead, they attempted to discover the secrets behind European wealth and power in the hope of adopting those secrets for their own use.

The threatening Europeans encouraged this attempt, missionaries and political ideologists presenting one or another of their own favorite

notions as a means to wealth and power. In China, Western style electoral democracy was at first accepted by many as one important means to wealth and power. But as time went on, those who saw Western style democracy as important became fewer and fewer. It was among that portion of the Chinese national elite which retreated to Taiwan (thus becoming Taiwan's national elite) that a faith in Western style democracy appears to have been retained—or to have been reborn—after 1945.

Given a need to restore prestige to its nation (minimally to that portion of the nation still controlled by the national elite, i.e., Taiwan; ideally, of course, to all of China as a consequence of the elite's long-awaited triumphant return to the mainland), thus restoring its own self-esteem, the national elite felt a deep commitment to democracy. But these needs also shaped the content of the elite's definition of democracy. Among the elements which the elite saw as crucial to the definition of democracy were:

- 1. Liberating the energies of the people and channeling them into public affairs. The major goal of the elite was to restore pride and self-esteem. Pride and self-esteem in the world in which China was being challenged came from the possession of wealth and power. According to the elite's understanding, democracy would liberate the energies of the people which had been repressed by Confucian society. Once liberated, these energies by democracy would be channeled into public affairs. Once in the arena of public affairs, the vast energies of the Chinese people could then be tapped for a national drive to wealth and power.²
- 2. Disciplining the energies of the people. Once the energies of the people are in the arena of public affairs, what is to prevent them from

²For historical background concerning this discussion see Benjamin Schwartz, Chinese Communism and the Rise of Mao (Cambridge, Mass.: Harvard University Press, 1951), p. 9; Benjamin Schwartz, In Search of Wealth and Power (Cambridge, Mass.: Harvard University Press, Belknap Press, 1964), pp. 237–47; and George Paloczi-Horvath, Mao Tse-tung: Emperor of the Blue Ants (Garden City, N.Y.: Doubleday, 1963), pp. 30, 32, 46. For examples of the relationships of "wealth and power" and democracy in the writings of Taiwan's national elite, see Hsueh Jen-yang, "Hsu," in T'ai-wan Sheng Ti-fang Tzu-chih Chih-yao, ed. Hou Ch'ang (T'ai-pai: T'ai-wan Sheng Ti-fang Tzu-chih Chih-yao Pien-chi Wei-yuan-hui and Chung Hua Ta-Tien Pien-yin-hui, 1965), preface, p. 2; "Message of Chiang Kai-shek," in Fred W. Riggs, Formosa Under Chinese Nationalist Rule (New York: Macmillan, 1952), pp. 154–55; and Yang I-feng, Chung-kuo Ti-fang Tzu-chih Hsin-lun (T'ai-pei: Cheng-chung Shu-chu, 1962), pp. 17, 39–40, 57–60.

flowing in the wrong direction? Or worse, what is to prevent them from flowing in many directions at once, producing conflict and chaos? A modern day American liberal might see in a situation of free-flowing energies possibilities for great creativity, as was manifested during such periods of Chinese history as the late Chou, the Six Dynasties, or the Republican period. But for the elites of the developing world, such creativity was always accompanied by great disunity, disorder, and disharmony-three attributes which guaranteed a lack of wealth and power for their nations as a whole. These periods, therefore, do not reflect the workings of effective solutions to their contemporary problems. The energies of the people must be disciplined into a unified flow if wealth and power are to be achieved.³

Concern for unity, order, and harmony became significant to the Chinese much earlier than the appearance of the European challenge. Much traditional Chinese political philosophy was concerned with how to counter the nation's tendencies toward disunity, disorder, and disharmony.

Nor was this concern limited to the traditional elite philosophers. Ordinary Chinese peasants knew well the dangers of social disorder. They had learned of the great misery social disorder can cause, both from listening to stories of social breakdown in other times and places and from their own personal experience.

Recently, a number of scholars (Lucian W. Pye and Richard H. Solomon, for example) have speculated that Chinese-felt needs for unity, order, and harmony may additionally result from the use of such psychological defense mechanisms as repression, projection, and displacement. According to these scholars, the Chinese call on these mechanisms in order to deal with the severe anxiety which arises from immersion in traditional-style Chinese

³For example, the importance of "organized democracy and disciplined freedom" is stressed by Huang Chieh, former chairman of the Taiwan provincial government, in *T'ai-wan Kuang-fu Erh-shihmien*, ed. T'ai-wan Sheng Hsin-wen-ch'u (T'ai-pei: Chung-hua Ta-tien Pien-yin-hui, 1966), preface, p. 6. For other examples bearing on this point, see Ch'en Li-fu, *Kuo-fu-chih Ch'uang-chien yu Chung-hua Wenhua-chih Pi-jan Fu-hsing* (T'ai-pei: Chiao-yu-pu, Wenhua-chu, 1971), pp. 8, 12–14; Fu Ch'i-hsueh, *Chung-kuo Cheng-fu* (T'ai-pei: T'ai-ta Fa-hsueh-yuan Shihwu-tsu and T'ai-pei Shih San Min Shu-chu, 1964), pp. 76–78, 83–84, 108–10; and "Ts'ung 'Luo-pen' Nao-chu Lun Tzu-yu yu Chih-hsu," editorial in *Chung-yang Jih-pao* (Hang-k'ung-pan), March 11, 1974. See Weiner, pp. 230-31, 235-37, for the experience of India's national elite in terms of the need for unity.

interpersonal relations—a style which, they claim, persists in the interpersonal relations of Chinese today.⁴

The necessity to channel the liberated energies of the people in unity, order, and harmony in turn required adding to the definition of democracy a further specification.

3. Orderly discussion in search of a unified general will. Elites such as those now on Taiwan came to believe that democracy, because it originated in the "advanced" European world, offers something akin to a scientifically correct method for discovering the best policies for attaining the public good. According to the elites' understanding of democracy, in order to discover the best policy to deal with a specific problem, the citizens and their representatives hold a marketplace of iceas. Ideas are traded in a calm, considered, rational manner, as all sincerely aid each other in a search for the correct method. Since it is a matter of science, the result will be a method proven best for all to see. Once found, therefore, the citizens will naturally all support the implementation of this manifestation of the general will.5

As with unity, order, and harmony, this Rousseauian concern for orderly discussion in search of a unified general will probably had correlates in Chinese culture long before it came in contact with European political concepts. The specific correlate which I have in mind is the Chinese ideal of social decision making in accordance with a high degree of wisdom, tranquility, moderation, and consensus. Of course, just as with the Rousseauian

⁴In this regard, see especially Lucian W. Pye, *The Spirit of Chinese Politics* (Cambridge, Mass.: MIT Press, 1968); and Richard H. Solomon, *Mao's Revolution and the Chinese Political Culture* (Berkeley: University of California Press, 1971).

⁵Pao Ch'ing-chiu, *T'ai-wan Sheng I-hui* (Mu-shan, T'ai-wan: Kuo-li Cheng-chih Ta-hsueh Kung-kung Hsing-cheng Ch'i-yeh Kuan-li Chung-hsin, 1964). Pao's discussion exemplifies this conception of democracy (p. 98). For the stress on nonconflictive politics in general, see Yang I-feng, *Chung-kuo Ti-fang Tzu-chieh Hsin-lun*, pp. 17, 148–49; Ch'en, *Kuo-fu-chih Ch'uang-chien*, p. 8; and Ho Ming-chung, *Chung-hua Wen-hua Tui Shih-chieh Wen-hua-te Kung-hsien* (T'ai-pei: Chiao-yu-pu Wen-hua-chu, 1971), p. 30. For the attitude of India's national elite in regard to democracy as a rational process, see Weiner, p. 229.

⁶Descriptions of this ideal may be found in such works as: Hsiao Kung-chuan, Rural China: Imperial Control in the Nineteenth Century (Seattle: University of Washington Press, 1960), p. 291; H. G. Creel, Chinese Thought from Confucius to Mao Tse-tung (Chicago: University of Chicago Press, Mentor Books, 1953), p. 197; Francis L. K. Hsu, Americans and Chinese (Garden City, N.Y.: Doubleday Natural History Press, American Museum Science Books, 1970),

ideal of democracy, actual practice was generally far less calm, considered, and rational than the ideal would suggest. In reality, for both Chinese and Western practice, the frequency, content, and enforcement of decisions was usually the reflection of the changing balance of concrete force among society's power holders. But, psychologically, Chinese proclivity for the ideals of wisdom, tranquility, moderation, and consensus in decision making may well have encouraged their receptivity to the Rousseauian ideal of democracy.

"The Movement" as a Crucial Element in the World of the National Elite. Such values as unity, order, and harmony were also fostered by another set of experiences of Taiwan's national elite and its counterparts in other countries. These elites were not only humiliated by the inability of their countries to meet the European challenge, were not only attracted to European ideologies as methods of countering the challenge, but also joined movements to propagate the European ideologies within their own societies.

These movements fostered a complex of values rooted in the necessities of a prolonged and difficult struggle. Unity, order, and harmony were among these values. Closely related were the values of party loyalty, selflessness, and willingness to sacrifice for the general good, and the importance of duty and obligations over rights.

Two points are worthy of note here. First, it is not necessary for believers in these values to actually live up to them. They may consistently violate them. But their image of themselves and the standards by which they judge other people are defined by these values. Secondly, the complex of values fostered by the movement

experience may become prime values in themselves. In addition to meeting the European challenge, participants come to see it as important to be unified, loyal, selfless, steadfast in duty, etc. They will likely also attempt to institutionalize these values and the general inspirational atmosphere of the movement into their society.⁸

A cadre ethic may also develop out of the movement. Holders of this ethic will believe that those who struggled for so long have been tested and proven to be especially qualified in morality and in knowledge to be society's leaders. The ordinary citizen should pay heed to their words and copy their deeds.

In countries like Taiwan, the cadre ethic is held in combination with an "expert ethic." This ethic is not a product of the movement experience. It is a result of the general recognition of the importance of "science" as an additional method for the attaining of wealth and power. According to this ethic, many government officials, given experience and/or special schooling, are considered scientific experts. These experts are thought to be especially qualified to make decisions concerning social policy. 10

A major conclusion is drawn by elites which fits in closely with the cadre-expert ethic. Under the influence of their definition of democracy as involving calm and considered decision making, the elites assume that democratic citizens will be rational and that this rationality will be expressed by the choice of the best people and the best policies with which to run society. And since the elites are convinced that it is the cadres and experts within their own ranks who are the best people, and that the policies of these cadres and experts are also the best policies, they naturally conclude

pp. 369-70, 380-81; and Bernard Gallin, "Political Factionalism and Its Impact on Chinese Village Social Organization in Taiwan," in Local-Level Politics, ed. Marc J. Swartz (Chicago: Aldine, 1968), p. 385. For expressions of this ideal among Taiwan's national elite, see Ho, Chung-hua Wen-hua, pp. 10-14, 33, 40; Ku I-ch'un, Ts'ung Tui Shih chu-te Kuan-ch'a Lai Chient'ao Chung-hua Wen-hua Ching-shen (T'ai-pei: Chiao-yu-pu Wen-hua-chu, 1971), p. 12; and Ts'ao Min, Lun Fu-hsing Chung-hua Wen-hua Chi-ko Szu Shih Erh Fei-te Kuan-nien chi Ch'i Fu-hsing-chih Tao (T'ai-pei: Chiao-yu-pu Wen-hua-chu, 1971), pp. 16-24, 27-28, 30-32.

⁷For descriptions of actual (as opposed to ideal) social decision making in Chinese society, see Gallin, "Political Factionalism," p. 385; Burton Pasternak, Kinship and Community in Two Chinese Villages (Stanford, Calif.: Stanford University Press, 1972), p. 99; and Hsiao, pp. 262-64, 290-92.

⁸On the influence of "the national movement" on such values as unity and sacrifice, see Weiner, pp. 230-33. On Taiwan the importance of "duty and obligations over rights" is stressed by Fu, p. 77.

⁹The importance of defining oneself as an "old revolutionary" (lao ko-ming) on Taiwan, is noted by Kao Chun, in "T'ai-wan Ch'uan-li Mao-tun chi Ch'i Chieh-chueh-chih Tao," *Ming-pao Yueh-k'an*, no. 109 (January 1975), 80.

¹⁰ That such an "expert ethic" exists on Taiwan is attested to by Assemblyman Yeh Ping-huang, quoted in T'ai-wan Sheng I-hui Ti-san-chien Ti-pa-tz'u Ta-hui Chuan-chi (Wu-feng, T'ai-wan: T'ai-wan Sheng I-hui Mi-shu-ch'u, 1967), vol. 2, p. 2574; by Kao, "T'ai-wan Ch'uan-li Mao-tun," p. 80; by Felix Tardio, Mr. Tardio Sees Taiwan (Taiwan: By the Author, 1966), pp. 109, 196; and by Liu Yen-fu, Ti-fang Tzu-chih Lun-chi (T'ai-pei: Min-chien Chih-shih-she, 1964), preface, p. 2.

that the populace will support these people and accept their policies.

The World of the Local Politician and Its Effect on the Local Politician's Political Culture

The politicians who represent the "mass" political culture have been formed in a very different world from that of these national elites. Below, I will describe the world of Taiwan's local politicians-as well as the world of the ordinary individuals to which the local politicians must respond—in terms of the basic goals of local politics, the way these goals have been achieved during Taiwan's traditional period, the way society has been changing, and the way in which these changes have been affecting methods of goal attainment. This description will demonstrate how local politicians such as those in Taiwan are constrained by their environment to behave in ways which directly conflict with the just described political ideals of their respective national elites. (See above section on Comparative Methodology for an explanation of the lack of parallelism implied in opposing the behavior of local politicians to the ideals of Taiwan's national elite.)

As in national politics, the main goals of local politics were physical security, wealth, and prestige; national politicians conceived of themselves as pursuing these goals on behalf of the nation as a whole. Individuals at the local level, however, were much more likely to conceive of themselves as pursuing these goals on behalf of themselves and their immediate relations.

For traditional societies such as Taiwan's, the major political methods for achieving these goals at the local level involved reliance on small family, larger lineage, and in-law relationships. If these proved insufficient, reliance was also placed on extra-familial vertical (patronclient) and horizontal mutual exchange relationships. For Taiwan, these extra-familial relationships were often manifested by village groups encompassing some or all members of a single village. Manifestations involving some or all members of a single village in intervillage relationships were also known. 11

11 For discussions of ordinary individuals and the social structures they have relied upon for goal achievement in traditional Chinese society, see Marion J. Levy, Jr., "Contrasting Factors in the Modernization of China and Japan," Economic Development and Cultural Change, 2 (1953), 165-66; Pye, p. 180; Morton H. Fried, Fabric of Chinese Society (New York: Praeger, 1953), throughout, but esp. pp. 123, 214, 218-22; and C. K. Yang, A Chinese Village in

In Taiwan, as in Chinese society as a whole, the character of the relationships that the individual relied upon during the traditional period was often founded on the ideal model of the family in combination with the actual necessities of bartering for crucial values.12 Underlying these relationships was the necessity to satisfy felt-needs of the individual or projected needs of units that the individual identified with (e.g., a family's need for prestige and perpetuation). Individuals learned through the socialization process that one of the main methods for satisfying needs was by the manipulation of human relationships. But they also learned that coldly to calculate human relationships for personal advantage was wrong. Therefore, in Taiwan, mainland China, and in traditional societies in general, the individual was taught to clothe such manipulations in terminology associated with the trust and warmth of ideal family relationships. 13 Partially for this reason, in traditional societies such as Taiwan's the instrumental coercive elements of protective relationships were heavily overlaid by feelings of voluntarism and affection. Patterns

Early Communist Transition (Cambridge, Mass.: MIT

Press, 1959), pp. 99, 109-18.

The same subject is discussed for traditional Taiwanese society by Gallin, in "Political Factionalism," pp. 380-81, 386-88; Martin M. C. Yang, Socio-Economic Results of Land Reform in Taiwan (Honolulu: East-West Center Press, 1970), pp. 428-44; Pasternak, esp. pp. 144-46; and David K. Jordan, Gods, Ghosts, and Ancestors: Folk Religion in a Taiwanese Village (Berkeley: University of California Press, 1972), esp. pp. 12-26.

For general discussions of ordinary individuals and the social structures they have relied upon for goal achievement in traditional society, see James C. Scott, "Patron-Client Politics and Political Change in Southeast Asia," American Political Science Review, 66 (March 1972), 91–113; and Eric R. Wolf, Peasants (Englewood Cliffs, N.J.: Prentice-Hall, 1966), esp. pp. 77–95.

12That this was often the case in Taiwan is noted by Yang, Socio-Economic Results, pp. 420-21. That this was often, but not always, the case in Chinese society in general is noted by Fried, pp. 136, 142, 151, 218, 220; and by Levy, "Modernization of China and Japan," pp. 165-66.

13Levy, "Modernization of China and Japan," p. 166; Pye, pp. 172, 180–81; and Richard H. Solomon, "Mao's Effort to Reintegrate the Chinese Polity: Problems of Authority and Conflict in Chinese Social Process," in *Chinese Communist Politics in Action*, ed. A. Doak Barnett (Seattle: University of Washington Press, 1969), p. 301. Specifically in relationship to use of kinship terminology on Taiwan, see Yang, Socio-Economic Results, pp. 436–37. For a discussion of the use of kinship terminology as a general phenomenon not limited to Chinese society, see Scott, "Patron-Client Politics," p. 95.

of deference to authority were, therefore, very strong. 14

In these traditional societies, the small family or larger lineage was occasionally strong enough to insure individuals an adequate supply of values, and it was not necessary to go beyond these units for protection. Protective ties that did go beyond the family often displayed the additional characteristics of being "based on individual ties to a leader rather than on shared characteristics or horizontal ties among followers." These leaders based their behavior on traditional moral and normative rules.16 They held power because of a personally controlled local resource base, such as land, lineage position, and/or a locally based government office.¹⁷ Given such resource control, they were generally accepted as the natural representatives of a constituency. Often they monopolized the resources of their area, and persons tied to them as clients had individually little power with which to bargain with them.18

However, during the traditional period patrons were somewhat dependent on the good will of their clients in terms of their individual

14According to James C. Scott, "material, particularistic inducements to cooperation play a minor role" during the traditional period, "except among a limited number of local power-holders." See James C. Scott, "Corruption, Machine Politics, and Political Change," American Political Science Review, 63 (December 1969), 1146. My contention is that even during this period, major inducements were of both the material and nonmaterial kind. Evidence for my contention is available in Levy, "Modernization of China and Japan," pp. 164–66, 171, 172, 176; Marion J. Levy, Jr., The Family Revolution in Modern China (Cambridge, Mass.: Harvard University Press, 1949), pp. 241, 242, 245; and Yang, Socio-Economic Results, pp. 437–44.

15Scott, "Patron-Client Politics," p. 97.16Gallin, "Political Factionalism," p. 386.

17In the Chinese case, nationally appointed government positions were also an important resource through which power came to local families. On this point, see Ch'u T'ung-tsu, Local Government in China Under the Ch'ing (Stanford, Calif.: Stanford University Press, 1962), pp. 168–92. On the general point of local resource bases, see Scott, "Patron-Client Politics," pp. 97–98.

18Scott, "Patron-Client Politics," pp. 93, 96, 99-100. Specifically in terms of Taiwan, see Yang, Socio-Economic Results, pp. 420-22, 437-44, 476-80; Pasternak, Kinship and Community, p. 57; and Gallin, "Political Factionalism," pp. 379-80, 384-85. According to these sources, heads of large lineages and landlords were the most typical patrons during the traditional period on Taiwan. For reasons why the landlord-tenant relationship may be regarded "as the prototype of patron-client ties," see Scott, "Patron-Client Politics," p. 93.

actions, and were virtually forced to give up some of their wealth to clients in return for prestige in their eyes, for fear that clients' collective actions would threaten a patron's physical existence.¹⁹

Given this balance of power and the affective elements noted above the patron-client

19Yang, Socio-Economic Results, pp. 423, 439-44; Scott, "Patron-Client Politics," pp. 93, 108; and Fried, pp. 104-05, 195-97, 219-21, 224, all argue against the thesis, so strongly stated in Communist Chinese publications as well as in Western scholarly works, that in traditional societies "peasants only give and cannot control what they give or demand anything in return and aristocrats only take and have no reason to return anything of what they take.... [They expect] nothing from them and [do] not feel that [they have] rights or duties toward them." [From John H. Kautsky, The Political Consequences of Modernization (New York: Wiley, 1972), pp. 27, 35.]

pp. 27, 35.]
Of course, where patrons concentrated in walled towns or where they were so well endowed with material resources and personal relations that they were able to raise their own military forces, or where modern state power brought police protection into the countryside, the patrons were freed from the threat of their clients' collective physical force. (For example, see Fried, pp. 195–98, 224.)

The appearance of police protection in the country-side—which was often accompanied by support by the police for the patrons as a stabilizing force in the society—was a common occurrence during the colonial period for many colonized societies throughout the world. As a consequence, Scott argues that the weakening of the physical threat which the clients were able to bring against their patrons caused a great strengthening of the patrons' power and a weakening of their motivation to be of service to their clients.

That the position of traditional patrons was in this manner strengthened in Taiwan during the Japanese period (although no reduction in service motivation is indicated) is argued by Bernard Gallin in "Land Reform in Taiwan: Its Effect on Rural Social Organization and Leadership," Human Organization, 22 (Summer 1963), 111.

Evidence that before the Japanese period fear of the collective actions of clients was a factor in convincing patrons to be of some service to their clients on Taiwan is based partially on the general Chinese experience of the possibility of peasant rebellion in areas in which landlords lived in the countryside far from the immediate protection of their own or government police forces. For this evidence, see Hsiao, pp. 426–31, and Fried, pp. 195–96, 219–21, 224. Specifically in terms of Taiwan, evidence that at least some of Taiwan's patrons lived under similar conditions before the Japanese period is found in Lawrence W. Crissman, "Each for His Own: Taiwanese Political Response to KMT Local Administration" (paper prepared for the London-Cornell Project for East and Southeast Asian Studies, June, 1969), pp. 19–20; Johanna Menzel Meskill, "The Lins of Wufeng: The Rise of a Taiwanese Gentry Family," in Taiwan: Studies in Chinese Local History, ed. Leonard H. D. Gordon (New York: Columbia University Press, 1970), p. 7; and Pasternak, Kinship and Community, pp. 116–20.

tie was in essence an "instrumental friendship," based on an unbalanced reciprocal exchange of values. It follows, therefore, that these ties were, in James C. Scott's terms, persistent, face to face, personal relationships dealing in terms of the needs of the whole person in a diffuse, multiplex fashion.²⁰

Social Change and Its Effects on Goal Attainment

A number of forces impinged on Taiwan's traditional society after 1945, causing drastic change. Below, three of these forces—economic development, land reform, and the institution of elections—will be discussed.

Economic Development.²¹ At least since 1945, and increasingly each year thereafter, economic development has had an important influence on Taiwan's society. Other economic changes, especially "very rapid population growth and accompanying scarcity of land,"

²⁰Scott describes the patron-client relationship as an "instrumental friendship" in "Patron-Client Politics," p. 92. He explains his use of this term on pp. 92–95 and 106–07. According to Pye and Solomon, aspects of Chinese political culture which closely fit in with Scott's model of the patron-client relationship as an "instrumental friendship" include: (a) manipulation of human relationships as the chief means of satisfying felt-needs, (b) necessity to disguise such manipulation with feelings of trust and warmth, (c) strongly felt needs for order and security, seen as satisfiable only within hierarchically organized personal relationships, (d) personal relationship hierarchies seen as functioning successfully only when they are headed by a single authority figure. See Solomon, "Mao's Effort to Reintegrate the Chinese Polity," pp. 298–306; Pye, pp. 103, 172, 180–81; and Arthur J. Lerman, "Political, Traditional, and Modern Economic Groups, and the Taiwan Provincial Assembly," (Ph.D. dissertation, Princeton University, 1972), pp. 267–75.

Evidence that the model of patron-client relations as an instrumental friendship is applicable to landlord-tenant relationships in traditional China can be found in Hsiao, pp. 427–28. Evidence that the model is applicable to traditional Taiwan is available in Yang, Socio-Economic Results, pp. 438–42, 460–65.

I recognize that there are scholars who argue in favor of using other terms in place of "instrumental friendship." See Fried, p. 103; and Eric R. Wolf, "Kinship, Friendship, and Patron-Client Relations," in The Social Anthropology of Complex Societies, ed. Michael Banton (New York: Praeger, 1966), pp. 10-13, 16.

²¹In addition to interviews, the major source for this discussion of the effects of economic development on goal attainment in Taiwan is Gallin, "Political Factionalism," esp. pp. 380–81, 399. Also see Yang, Socio-Economic Results, pp. 455, 457. For a discussion of the effects of economic development on goal attainment in other societies, see Scott, "Patron-Client Politics," pp. 103, 105–08.

have also been important.²² The following discussion will be limited to describing economic development on Taiwan as it is related to the rise of new interest groups and to the widening geographical area of the ordinary individual's economic activity.

Here it may be useful to remind the reader that such limitations as these are made to restrict the scope of this article to the main points under discussion and keep the text at a manageable length. It also may be useful to remind the reader that most aspects of the following description of economic development and its effects on goal attainment in Taiwan are congruent with the experience which other societies have had concerning the development of the "mass political culture."

The Rise of New Interest Groups. Economic development began to stimulate the appearance of groups with interests in new areas of activity. With the institution of elections in 1946 these new groups began to seek out or send out politicians, in order to insure their protection amidst the vagaries of both the market and the many decisions being made by the burgeoning bureaucracy, which was expanding along with the growing economy.

Such groups often consisted of one person and all those dependent on that person's enterprises. These people not only sought out politicians or became politicians themselves for protection of their enterprises, but often used their enterprises as a political base for further economic and/or political aggrandizement.²³

Whatever the course of action taken, new political interests and political bases were gradually becoming a part of the polity and competing with or supporting older politicians, patrons, village groups, lineages, and small families.

Competition was for more than economic and political power. The new political entities were also an alternative source of status and deference to the older units. Successful competitors, upon attaining economic and political power, were almost automatically granted status in recognition of their success.²⁴ Moreover,

²²Gallin, "Political Factionalism," p. 380.

²³For a detailed discussion of my characterization of new interest groups which "seek out or send out politicians" see Lerman, pp. 345–80.

²⁴Gallin, "Political Factionalism," p. 386, argues that prestige did not automatically come with the attainment of real power. Prestige still had to be earned through traditional respectable behavior. Crissman, p. 26, Yang, Socio-Economic Results, p. 485, and my own research indicates that the process is more automatic.

during the competition new political entities often vanquished those whose status was based on older arrangements.²⁵ Whether as alternatives or as direct competitors, a threat to traditional patterns of status and deference as well as to traditional patterns of power arrangements thus appeared.

For the ordinary individual, the rise of these new interest groups had an unsettling effect. In the first place, their power was so great that the traditionally protective patron, village group, lineage, and/or small family could not protect the individual from their aggressions.26

Secondly, specifically in regard to the village group and lineage, their power was not only deteriorating as a result of attack from these externally based interest groups; the rise of new interests meant that the interests of the individuals within the village group and lineage were becoming more varied. This loss of solidarity was an internal force which caused a marked deterioration in both the ability and willingness of the village group and lineage to guarantee the security, wealth, and status of the individual.²⁷

Tnirdly, given the increasing number of groups and the increasing amount of competition between them, the original fears of the ordinary Chinese concerning unpredictability and chaos were also increasing.

Widening Area of Economic Activity. A related result of economic development was the widening of the geographic area relevant to each individual's and small family's economic livelihood. No longer was it enough to have good relations with neighbors and kin living within the confines of a small market town and its satellite villages. Products originating from all over Taiwan and even from overseas had now to be purchased. In turn, the individual's own markets were being heavily influenced by far away forces. New "quasi-official" economic organizations were also influencing individuals from beyond their local areas.²⁸ As a result, the individual and small family came to be increasingly involved with many individuals in terms of very specific transactions and decreasingly involved with only a few individuals in terms of comprehensive, whole person relationships.

Under these conditions, the relatively small village group and lineage no longer had sufficient power to protect individuals and their families, even if these units had remained united and unthreatened by powerful new interest groups. Nor could the traditional patrons with their locally based resources be of much aid. Indeed, the possibility of maintaining any "whole person" patron-client relationship, whether based on traditional and/or nontraditional resources, was becoming increasingly difficult.

Moreover, even if the increasing differentiation in interests were not considered, the larger number of people now in contact with each other and the wider geographic area over which they were spread gave rise to a looser, more complex, less controllable situation. Thus, another cause for the deepening of ingrained fears of conflict and unpredictability appeared.

Land Reform.²⁹ Taiwan is one of the only developing countries which has successfully instituted an effective land reform program. Therefore, unlike economic development and the institution of elections which have made comparable contributions to the emergence of the mass political culture in both Taiwan and other developing countries, the contribution of land reform to the emergence of the mass political culture is more specific to Taiwan.

Land reform on Taiwan contributed heavily to the rupturing of traditional power and status arrangements in the countryside. Previously it was the landlord, or those members of the lineage who controlled the lineage lands, who had power and status in the countryside. The individual and small family rented land, borrowed money, and/or in other ways received services from these personages in return for whatever services they themselves could offer.

²⁵Gallin, "Political Factionalism," pp. 386-87; Bernard Gallin, "Rural Development in Taiwan: The Role of the Government," Rural Sociology, 29 (September 1964), 318; and Yang, Socio-Economic Results, pp. 487-500.

²⁶Gallin, "Political Factionalism," pp. 394, 396, 398-99.

²⁷Note that solidarity is still manifested by many village and lineage organizations on Taiwan. What we are discussing is a gradual tendency toward breakdown, not a completed process. For fuller descriptions of this process, see Gallin, "Political Factionalism," pp. 387–88, 392, 394–97; Pasternak, Kinship and Community, pp. 72–78, 86–113, 116, 120–27; and Yang; Socio-Economic Results, pp. 455, 457.

²⁸Gallin, "Political Factionalism," p. 381, and Gallin, "Rural Development in Taiwan," p. 314.

²⁹In addition to interviews, the major sources for this discussion of the effects of land reform on goal attainment in Taiwan are Gallin, "Political Factionalism," pp. 380-87, "Land Reform in Taiwan," pp. 109-12, "Rural Development in Taiwan," pp. 314-18; Yang, Socio-Economic Results, pp. 230-59, 458-66, 480-505; and Pasternak, Kinship and Community, pp. 25, 100, 115 munity, pp. 25, 100, 115.

With the end of large landholding in 1953, landlords and lineage leaders no longer had a strong material base with which to serve clients.

For this same reason, landlords and lineage heads now had much less of a material power base with which to sustain themselves in leading positions in the community. As will be seen below, reliance on their traditional prestige along with the remainder of their original material power base was inadequate for this purpose. Instead, landlords and lineage heads had to work hard to find new material power bases, or they had to accept the role of rank and file citizen.

As for ordinary peasants, in spite of the land that they received as a result of land reform the loss of the services of the landlords and lineage heads was another factor increasing their feelings of insecurity.

Institution of Elections. Perhaps the greatest influence on Taiwan's society has come from the institution of elections for local public offices. In conjunction with the influences already discussed above, as well as independently, elections have caused many changes in the world of the local politician.

The most significant aspects of the elections in terms of political change were: (a) virtually any adult could run for office; (b) virtually every adult could vote; (c) the winners obtained real power at any one of a number of levels of government; ³⁰ (d) along with winning came great prestige; and (e) many important elections were held in districts much larger than the village community.

The Institution of Elections and the Growth of Conflict. It has already been demonstrated that because of larger areas of economic activity and an increase in the kinds of interests among the people, conflict was growing. As in other developing countries which instituted elections where anyone could run and with election districts including large areas containing many more voters than that of the small village community, possibilities for conflict in Taiwan were further increased. Moreover, the power available to the winner of an election

30 That many posts at many levels have real power is testified to by Gallin, "Political Factionalism," p. 395; Crissman, pp. 8, 30, 37–38, 42, 44, 47, 54, 57; Yang, Socio-Economic Results, pp. 408–11, 490–91; "Chueh Pu Jung Chu 'Hsuan' Hai Kung," in Lien-ho Pao (Chia-nan Ti-fang-pan), January 12, 1968, p. 7; and Tai-wan Sheng I-hui Ti-san-chieh Ti-pa-tz'u Ta-hui I-yuan Chih-hsun yu Pen Fu Ta-an Hui-pien (Chung Hsing Hsin Ts'un, T'ai-wan: T'ai-wan Sheng Cheng-fu Mi-shu-ch'u, 1967), p. 179.

encouraged those already involved in conflict to seek after that power as a necessary resource. Even those not yet involved in conflict were motivated to join the competition for a share as a hedge against future needs, if not for immediate advantage.

Since anyone could run and since the material rewards could be great, even individuals without an original economic base could borrow money in the hope of a large return for their investment. Those who began with resources, obtained either by selling the bonds received in land reform or by relying on the economic resources and manpower of their new enterprises, were, of course, in a better position. 31

The Institution of Elections and Taiwan's Former Landlords. Because Taiwan had undergone an effective land reform, Taiwan's landlords were in a different situation from their counterparts in countries which are otherwise comparable to Taiwan. Faced with the developing electoral system, each of Taiwan's former landlords displayed one of two responses. The first response was to withdraw from competition for a local leadership position. Those who chose this response did so for one or more of the following reasons: (a) financial inability to compete, (b) unwillingness to take the necessary financial risks, (c) rejection of the extremely opportunistic behavior increasingly necessary for electoral success, (d) diversion of interests into farming or nonfarm business, and (e) diversion of interests away from the local community.³²

31For the institution of elections and the growth of conflict on Taiwan, see Gallin, "Political Factionalism," pp. 386-88, 391; Ch'en Ming-t'ieh, "Ch'e-ti Hsiao-ch'u Hsuan-chu P'ai-hsi Fen-cheng," Ti-fang Tzu-chih, no. 176 (February 15, 1968), p. 15. Pao, p. 69; Liu, pp. 33-34; T'ai-wan Sheng I-hui Ti-san-chieh Ti-pa-tz'u Ta-hui I-yuan Chih-hsun yu Pen Fu Ta-an Hui-pien, p. 179. For comparable phenomena in India and Southeast Asia, see Weiner, pp. 208-13 and Scott, "Patron-Client Politics," pp. 109-11. Note that material rewards are likely to be great only for higher level elected office. Motivation for lower level office is usually based on the quest for prestige. See Gallin, "Political Factionalism," p. 386, and Crissman, p. 40.

32 Yang, Socio-Economic Results, concludes that "land reform removed most of the large landlords from rural communities and into businesses or politics. Some did stay in rural towns but changed their occupations. Small landlords had, in most cases, become real owner-farmers" (p. 497; also see p. 362). I am not sure that Yang's evidence permits us to make such a definitive statement.

Evidence concerning landlords who withdrew from local leadership competition is found in Yang, Socio-Economic Results, pp. 230-60, and in Gallin, "Political Factionalism," pp. 386-87, and Gallin, "Rural Development in Taiwan," pp. 316-18.

Note that the choice to stay out of local leadership competition was not irrevocable. This was especially true for former landlords who became successful in nonfarm businesses. Nevertheless, both Gallin and Yang offer evidence to suggest that the number of former landlords who choose to stay out of local political competition is growing, not diminishing.³³

Many former landlords, however, chose a second response—doing everything in their power to compete for local leadership positions. And while some went bankrupt for their efforts, evidence indicates that many were successful, especially in the early years of the electoral system.³⁴ Just as, or more, motivated than anyone else to obtain the rewards now being offered by the new electoral system, many former landlords managed to successfully establish a new political base.

The Institution of Elections and Status Arrangements. One important cause of many former landlords staying in politics arose from the rupture of traditional status arrangements and deference patterns noted earlier. Traditional status arrangements, buffeted by economic development and land reform, were receiving their coup de grace from politicians using "nontraditional" forms of political behavior. These politicians, many from humble origins, were using "nontraditional" forms to nudge aside those traditional leaders who were not adapting their behavior to the new political system. 35

33Yang, Socio-Economic Results, pp. 492–94, has figures which indicate a possible slow decline in the rate of landlord participation and/or success in competition for local leadership positions over time. If these indications are correct, the decline may be attributable to the aging of the landlords as a group and not to the other causes noted above. The decline may also be attributable to the weakening of traditional status as an electoral asset as time goes by. On this latter point, see Yang, Socio-Economic Results, pp. 480, 484, 501, and Gallin, "Rural Development in Taiwan," pp. 317–18. On the point of a decline in landlord political participation over time see also Gallin, "Political Factionalism," pp. 386–87, and "Rural Development in Taiwan," pp. 316–18.

³⁴Early landlord success in local leadership competition is also indicated by Yang's figures in Socio-Economic Results, pp. 492–94. A generally high rate of landlord participation and success in politics is indicated by Yang, pp. 483–86, 488, 494. Gallin, in "Political Factionalism" n. 389, and in "Pural Desert

Since other arrangements were in decline, elections, with their clear proof of popular esteem, became one of the few methods available for affirming, or reaffirming, one's reputation. As had happened in many other developing countries during the rupture of old status arrangements, it became difficult to clearly discern who the status elites really were. And since status remained highly valued, both to previous and potential power holders alike, elections were seized upon as the new proof that one was indeed of high status.³⁶

The Institution of Elections and the Plight of the Ordinary Individual. Given the above noted social changes, the situation of the ordinary individual in Taiwan, as in similar developing countries, was becoming increasingly difficult. The individual was faced with unprecedented problems of schooling, employment, markets, and health care, as well as a general need for security from the chaos which an increasingly conflictive environment seemed to portend. At the same time, the traditionally protective lineage and village groups were becoming too weak and/or disunited to be effective. Traditional patrons, depending on what remained of their locally based resources, were also unable to offer meaningful protection.³⁷

The alternative of organizing with peers in horizontal pressure groups also did not offer much hope. A number of reasons may be cited for this:

1. In traditional societies there had been much conflict and suspicion at the local level, especially between ethnic groups, between villages, and between lineages within villages. Intergroup relations in mainland China and in Taiwan, for example, often took the form of international relations rather than relations between civic organizations of the same state.³⁸

p. 318; Yang, Socio-Economic Results, pp. 487-88, 492-98, 503; and my own interviews.

³⁶The importance of elections "when other criteria for status become fuzzy" is stated as a general proposition by Weiner, pp. 208-09. For manifestations of this phenomenon on Taiwan, see Yang, Socio-Economic Results, p. 485; and Crissman, p. 26.

³⁷ Scott, "Patron-Client Politics," pp. 103-04. In terms of Taiwan, see Gallin, "Political Factionalism," pp. 378, 380-81, 391, 394-95, 399; Crissman, pp. 20, 21, 204 Version of Taiwan, Tailor of Taiwan, Tailor

- 2. If the recent speculation of such scholars as Pye and Solomon concerning Chinese personality is sound, a reason for nonreliance on horizontal peer organization specific to Taiwan would be the individual Chinese's lack of faith in cooperation between peers and tendency to see cooperation as possible only in terms of hierarchically organized personal relationships led by social superiors.
- 3. In terms of transitional societies in general, if traditional forms of hierarchical relationships (especially the patron-client form, though with a new breed of patrons) could adequately service the felt-needs of the individual, they would continue to prosper. The "naturalness" and lack of strain felt in continuing to use traditional forms of hierarchical relationships, prejudice in favor of these relationships, prejudice against horizontal peer group relationships, and lack of knowledge about many kinds of horizontal peer group relationships, all would act to keep traditional forms of hierarchical relationships prospering.
- 4. Previously united villages and lineages—two hierarchically organized groups with potential for transformation into more equalitarian organizations—were already breaking up along the lines of newly developing economic interests.
- 5. The individual feared that there were not enough resources to be adequately distributed among potential members of a horizontal organization.
- 6. In spite of the above reasons, even if individuals did attempt to organize their peers into horizontal pressure groups, the national elite on Taiwan-like its counterparts in many other developing countries—would not allow such independent organizations, dedicated to the desires of their members, to become powerful. That would be too threatening to the national elite's own power. Instead, the national elite actively discouraged spontaneous organizing and sponsored its own organizations,

manifested itself on Taiwan, especially before the establishment of firm police control in the country-side. In this regard, see Meskill, pp. 7, 11–12; Jordan, pp. 9, 20–26, 42, 48–51, 86, 135–37, 163; and Burton Pasternak, "The Role of the Frontier in Chinese Lineage Development," The Journal of Asian Studies, 28 (May 1969), 554–55, 559–61. Indications that underlying currents of these old conflicts did not disappear with the imposition of police control are found in Crissman, pp. 20–23; Bernard Gallin, "A Case for Intervention in the Field," Practical Anthropology, 10 (March-April 1963), 58; Jordan, pp. 22, 48–49, 135–37; 163–64; and Pasternak, Kinship and

Community, pp. 103-05.

encouraging individuals to join. But these organizations dedicated to furthering the desires of the elite and not the desires of the ordinary members were not seen as aids in solving the problems of the individual.³⁹

Reliance on formal government institutions was another solution which did not appeal to the ordinary individual. The reason for its lack of appeal is also relevant to the lack of reliance on horizontally organized groups. In Taiwan, as in many developing societies, the individual has been socialized not to trust impersonal institutions. Security, the individual has learned, comes from dependence on personal relations. Therefore, ordinary individuals avoided reliance on impersonal government programs.

Even if individuals were able to bring themselves to depend on impersonal programs, moreover, problems remained. Those who ran these programs, influenced by the same socialization concerning impersonal and personal relationships, acted in such a manner as to undermine their impersonal quality.⁴⁰

The Institution of Elections and Security for the Ordinary Individual. Given this inability to rely on horizontal peer organization or on impersonal governmental programs, individuals in Taiwan, paralleling the response of individuals in comparable situations in other developing countries, increasingly conceived of their needs in terms of their own particular problems unrelated to the problems of those outside their immediate circle. Consequently, individuals began making demands with an eye to causing particular power holders to act on behalf of the individuals themselves, members of their families, or on behalf of others close to them. 41

³⁹For the national elite's attitude toward nongovernmental organizations, see statements in *T'ai-wan Sheng Ti-faug Tzu-chih Chih-yao*, pp. 896, 872–73, 876, 879, 891.

40 See n. 13 and 20 above. Also see Scott, "Patron-Client Politics," pp. 102-04, and Bernard Gallin and Rita S. Gallin, "Sociopolitical Power and Sworn Brother Groups in Chinese Society: A Taiwanese Case," in *The Anthropology of Power*, ed. R. N. Adams and R. D. Fogelson (New York: Academic Press, 1977), p. 95.

Note that the impersonal administration of the land reform program and the strict adherence to a merit system in the treatment of students on all levels of schooling in Taiwan are two important exceptions to this rule.

41"Historically, the expansion of the suffrage together with the rupture of traditional economic and status arrangements have signalled the rise of particularistic, material inducements" (p. 1148). "The sense of community was especially weak, and ... social fragmentation made particularistic ties virtually the

Fortunately for the individual, social change had provided new resources which made these demands more effective. Given the rupture of traditional status arrangements and deference patterns, for example, it was now difficult to maintain the support of an individual by heavy reliance on the feelings of voluntarism and affection which traditionally supported patronclient relations. Greater material reciprocity was necessary. More crucial, however, was that the individual now had something that people of power and potential power needed very badly—a vote.

In earlier times, it was often the distance of the rich from the immediate protection of police power which constrained them to accept the ideology that the wealthy should serve the people in return for a good name. Now it is the value of the vote which motivates politicians to aid as many individuals as possible in expending their resources to win election. Moreover, since there are a large number of competitors for their votes, individuals can often shop around for the politician with access to the economic, social, and political resources most relevant to their needs.

As a result, candidates in Taiwan, like candidates in similar developing countries, are placed under great pressure to establish, either personally or through intermediaries, as many personal relationships as possible with potential voters. Within the context of each relationship, candidates must offer the voter an adequate number of particular favors, from whatever resources-public or private-at their command. At least in Taiwan (if the Pye-Solomon speculation is valid), these favors must serve not only the voter's material needs, but also such nonmaterial needs as the provision of a figure of stability in a world of potential chaos. It is to the candidate who offers the most advantageous mix of needed services that ordinary individuals give their votes.42

only feasible means of cooperation" (p. 1150). In these two statements, Scott, "Corruption, Machine Politics, and Political Change," generalizes on the type of situation facing the ordinary individual in Taiwan. Evidence to indicate that this is the case for Taiwan is found in Lerman, pp. 387–401.

⁴²In terms of the voters' material needs, see Scott, "Corruption, Machine Politics, and Political Change," pp. 1148, 1157–58, and Scott, "Patron-Client Politics," pp. 108–13. My statements concerning the voter's nonmaterial needs are based on the evidence noted in fns. 4 and 20. That politician-voter relationships on Taiwan are actually of this nature is indicated by Gallin, "Political Factionalism," pp. 391, 395; Crissman, pp. 30, 32, 37; and Lerman, pp. 203–13. Note that a number of politicians on Taiwan echoed

The Institution of Elections and the "Unsavory" Appearance of Local Politics. Since politicians must service voters individually in order to establish the necessary personal relationships on which reciprocity is based, their services frequently take the form of bending or breaking general rules on behalf of the voter. But because these general rules are usually legally binding, it follows that a politician's services often involve activities which are corrupt, according to the strict definition of the term. 43

In other cases, while politicians' services may not be legally corrupt, their need to attract votes on a particularistic basis will often cause them to act on behalf of a very small section of the community, disregarding other sections of the community or the community as a whole.⁴⁴

In all cases, given the great competition between candidates and the necessity to obtain votes through personal relationships, candidates begin to gather around them as many voters as possible, and mutually opposed factions form. The growing competition of these factions—a competition which includes the exacerbation of the traditional and modern social divisions already mentioned above—creates the appearance of great divisiveness and lack of consensus at the local level.⁴⁵

the statement made to me by one provincial assemblyman that: "the people recognize money, not candidates." ("Jen-min jen ch'ien, pu jen jen.")

It should be noted, however, that two factors make it appear that traditional values still guide individuals when they shop for a politician. First, individuals are still likely to insist upon establishing "personal" relationships rather than treating their dealings with a politician as a coldly calculated transaction. Second, the individual uses traditional guides to determine which politician is likely to offer the most rewarding relationship. See n. 13 above and Crissman, p. 37.

⁴³Scott, in "Corruption, Machine Politics, and Political Change," pp. 1143–45, states the general case. For specific examples in Taiwan, see "Hsin-ying Ko-chia Yin-hang, Shih-yuan Ch'ao-p'iao Huan-kuang," in *Lien-ho Pao* (Chia-nan Ti-fang-pan), January 21, 1968, p. 7; "Chuch Pu Jung Chu 'Hsuan' Hai King," in *Lien-ho Pao*, p. 7; Crissman, pp. 35–36; and Gallin, "Political Factionalism," p. 386.

44For examples of particularism on Taiwan, see Lerman, pp. 218-25, and Pao, pp. 67, 69. For similar examples in other societies, see Scott, "Corruption, Machine Politics, and Political Change," p. 1144, and Weiner, pp. 209-13, 220.

45Often there is a consensus within which the competition is taking place, but this is not necessarily apparent to the outsider. On this point see Andrew J. Nathan, "A Factionalism Model for CCP Politics," The China Quarterly, 53 (January-March 1973), 46, 50-51, and Lerman, pp. 300, 309-10. Sources for the rise of factionalism are in fn. 31.

Moreover, with the desires of all those in local politics centered on personal security, wealth, and status, causing voters and politicians alike to form and break up relationships solely on the basis of individual interest, lack of principle joins corruption, particularism, and divisiveness on the list of "unsavory" appearances at the local level. 46

The Institution of Elections and the National Elite. Even the national elite is forced to participate in this system. The national elite must insure that a great majority of the candidates who win office are identified with its party. Otherwise, it would lose prestige which means so much in terms of a continuation of legitimacy.

In order to win the national elite must find candidates who, at a minimum, are already enough a part of their local societies so that the voters can conceive of establishing personal relationships with them. Usually, Taiwan's national elite—like the national elites in similar developing countries—is faced with individuals in the various election districts who are so well endowed with personal relationships and other resources that the national elite must work with them. Attempts to impose candidates from the outside, in disregard of the will of these local notables can only lead to defeat for the national political party. 47

Taiwan's national elite is not completely at the mercy of the local notables, however. Firstly, the local notables are disunited, since they are the product of the conflict between factions and between other social divisions discussed above. Secondly, the national elite, through its willingness and ability to manipulate and bend electoral rules, its ability to revalue virtually all electorally meaningful resources—public or private—through governmental action, its control of all non-word-of-mouth information about candidates which the

46 James C. Scott argues that this concentration on individual self-interest may be a blessing in disguise. In a divided polity with few other bases of consensus, self-interest can serve as the one motive with which to bind otherwise opposed individuals together. See Scott, "Corruption, Machine Politics, and Political Change," pp. 1151-54. Weiner, pp. 212, 242, and Lerman, p. 310, make the same point. For an example of lack of principled behavior by Taiwan's local politicians, see "Man T'an Cheng-chih yu Tao-te," in Min-sheng Jih-pao, January 24, 1968, p. 2. For similar behavior in India, see Weiner, p. 212.

47Scott, "Patron-Client Politics," p. 110, and "Corruption, Machine Politics, and Political Change," p. 1154. For examples of the national political party going down to defeat in Taiwan, see articles in Tzu Chih, no. Ko Hsin 181 (May 1, 1968), pp. 4, 5, 10.

public receives, its active discouragement of any efforts to seriously organize an opposition party, and its control of a loyal block of votes in many election districts, can have a great influence on the outcome of elections.

In response to the elite's great influence over elections, Taiwan's local level factions have most of their active members join the elite's political party. Only in the event that the elite's party nomination does not go to one's own faction and no adequate compensation is provided, will the non-nominated faction run a serious campaign in opposition.⁴⁸

It appears that an additional elite tactic in Taiwan is to insure that at least two viable factions remain in competition in each election district. In order to attain this end, the elite parcels out nominations and other resources to competing factions accordingly. What the elite seems to fear is that, if one faction became dominant, the whole area perhaps allying itself with other united areas might begin to make demands on the national elite, instead of expending its energies in local factional competition. Except for an occasional failure, the result of this policy has been a two-faction system in many of Taiwan's election districts.⁴⁹

As a whole, Taiwan's political system appears somewhat coercive, but in other respects resembles a boss machine of the type found in noncoercive electoral systems. The national elite acts as the boss, using its control of resources to induce most potentially threatening vote-getters to join its machine.

This boss machine sits on top of the local factional systems. The boss keeps the bought-off local level politicians divided by fostering a balance of power between rival faction leaders. Each leader is kept satisfied, but no leader is allowed to control resources to the extent of driving rival factions out of existence.

Note that in terms of this nationally based boss-machine system, the familiar distributive requirements still prevail. It is not simply a matter of buying off the faction leader. Faction leaders must be assured of enough resources to

⁴⁸For an example of this strategy on Taiwan, see Chao Chun-wu, "Miao-li Hsien Chang Hsuan-chu, Liu Huang Liang P'ai Ta Chueh-chan," *Liao-wang Chouk'an*, no. 31 (October 14, 1967), p. 26.

49 Crissman, pp. 33, 38, and Douglas Mendel, The Politics of Formosan Nationalism (Berkeley: University of California Press, 1970), p. 95. For a general statement describing the elite's influence over elections and the response to this influence by local level factions on Taiwan, see Gallin and Gallin, "Sociopolitical Power and Sworn Brother Groups," p. 95.

pass down to their lieutenants, who in turn pass them down, until voters receive enough to be convinced that it is in their interest to support the entire system by voting.⁵⁰

Conflict Between the Two Cultures

There are political scientists—as well as local politicians in Taiwan and elsewhere—who argue that the type of political system described above, with all its formal corruption and other unseemly characteristics, actually works well in terms of the needs of ordinary individuals whose society is in a transitional stage of political and economic development. Politicians in control of resources are under extremely heavy pressure to expend these resources in helping ordinary individuals in exactly the manner in which ordinary individuals demand.

With the patience and forbearance of the national elite, the political scientists add, this system will indeed develop into a model of democracy somewhat close to the original intentions of the national elite. The economy will make people feel more secure. People will develop trust in impersonal institutional guarantees. They will begin to see their interests in occupational and class terms, form effective organizations with which to pursue them, and abandon their unilateral attempts to gain security by making personal deals with a patron. At that point they will begin to pursue their interests in terms of support for national policies which affect all individuals in a given category in the same way, 51

But it is difficult for a national elite such as Taiwan's to be patient and forbear. This is true in spite of the national elite's complicity in the system, as outlined above. Perhaps this complicity, forced on the national elite by the nature of the local political system and tarnish-

⁵⁰For general descriptions of the boss machine model, see Scott, "Corruption, Machine Politics, and Political Change," pp. 1154–58, and Scott, "Patron-Client Politics," pp. 96, 110–12. I prefer not to refer to the local factions as boss machines, since, according to Scott's definition, the term boss machine "connotes the reliable and repetitive control it exercises within its jurisdiction" (Scott, "Corruption, Machine Politics, and Political Change," p. 1143). Most factions, however, appear to share control of their jurisdiction with at least one other faction.

ing its superior image of itself, is an additional reason why the national elite cannot be patient and forbear.

Indeed, such a national elite would probably find it difficult to be patient and forbear, even if the local political system was exactly like the model of democracy described above as somewhat close to its original intentions. The "somewhat close" model is also deficient, since it suggests occupational and class organizations freely competing for their own conceptions of their interests, in disregard for the elite's conception of the good of the whole political body. How much more difficult is it, therefore, to accept the transitional system, with its stress on the self-centered particularistic needs of individuals, small families, and small localities for security, wealth, and prestige; its strong tendency to cater to these needs through the distribution of great amounts of materials and services according to unscientific, seemingly haphazard, political criteria; its stress on personal relations and personal favors; and its manifestation (at least in the mind of the national elite) in a chaos of factional conflict noted for opportunistic, anything-goes tactics.

All this is very distressing to an elite which conceives of democracy as a process which arrives at the best choices of policies and leaders through a rational discussion of alternatives and which results in rational, scientific allocations of resources according to plans based on what is best for the general welfare, This conception of democracy in combination with the elite's participation in "The Movement" leads the elite to expect citizens and politicians in a democracy to be motivated to do their duty and fulfill their obligations, with less stress on their privileges and rights. Selflessness and a willingness to sacrifice for the greater good are central to the elite's view of actually functioning democratic systems. For the elite, such standards plus the unity and order according to which the process should run will give the country both the moral and physical strength necessary to insure its security, wealth, and status among the nations of the world.

At this point it may be helpful to remind the reader that it is not necessary for the elite to live up to these standards in order to believe in them. The elite may consistently violate them. But the elite's image of itself and the measures the second of the standards of the second of the second

cadres (i.e., the experts and cadres within its own ranks) coincides with what is democratic. But its experience had been just the opposite. It can only draw the conclusion that something is very wrong.

Perhaps the people are not yet mature enough for democracy. Perhaps the politicians and their factions are evil and are corrupting the electoral system.⁵² Certainly the interest groups arising out of the new economy or reflecting the prejudices of small localities are too selfish.⁵³ And perhaps the electoral system itself, in bestowing power on such selfishness, is deficient.⁵⁴

Fortunately, however—the elite is convinced—there is a force in society, its government, and party which understands what true democratic standards are. Moreover, this force is powerful enough to correct the evils of the local political system.⁵⁵ The tendency on Taiwan has been for the national elite to use its government and party to disallow "evil" activities and organizations, progressively restricting electoral activities until behavior conforms to the ideal. These restrictions, once established, are designed to remain in effect until the people mature.⁵⁶

Unfortunately, the standards of the elite are so high that it is unlikely that the people will ever mature sufficiently.⁵⁷ It appears that not even a small number of imperfections are acceptable to the national elite as the price of an unrestricted electoral system.⁵⁸

52See n. 31 above.

53For examples of the elite's attitudes concerning interest groups, see Pao, pp. 67, 69, and Ho, p. 30.

⁵⁴For examples of elite attitudes concerning the deficiency of the electoral system, see Liu, preface, p. 4, and pp. 1, 33-34, and Yang I-feng, p. 178.

55For examples of the national elite's faith in the government, see Pao, pp. 67-69, 107, and Liu, p. 34.

⁵⁶For the elite's attitude on what should be done about the local political system, see Pao, pp. 50-52, 68-69, 74, 99, Yang I-feng, pp. 117-79, and Ch'en, p. 14.

57In terms of the over-high standards which the elite insists that democracy must live up to, see Liu, preface, pp. 1-4, and Pao, p. 98.

58In terms of the national elite's unwillingness to accept even a small number of imperfections in the electoral system (and for further examples of what the elite thinks should be done about the local political system), see Pao, pp. 52, 69, 74, 99, and Chien Ho, "Ch'i Kao I Chao," in Chung-yang Jih-pao (Hang-k'ung-pan), September 14, 1971, p. 4. For further examples of the Taiwan national elite's attitudes toward the local political system, see material introduced in fns. 5, 31, 43, 44, and 46 above. For the reactions of India's national elite to their local political system, see Weiner, pp. 228-44. For the

As for many local politicians—both in Taiwan and elsewhere-imperfect as they know the local electoral system to be, they are used to its imperfections, identifying them with normal human behavior and taking them all in their stride. Moreover, they feel that their actions, whether formally legal or not, have truly been earning the votes of the electorate. The national elite may complain that they are acting against the general will of the people, but they are in daily contact with many persons, if not "the people," and feel that they are in a much better position than the national elite to determine what the will of the people is. On rare occasions one or another of Taiwan's local politicians will put aside a fear of repression and give voice to these feelings, publicly opposing the legitimacy of the national elite with his or her own electoral legitimacy.59

Amelioration of the Conflict on Taiwan

In many countries, the above conflict has led to the national elite despairing of electoral politics altogether, and replacing elections with forms of democracy based on the national elite's "unique capacity" to rely on its own background and skills to determine what the will and needs of the people are. A number of constraints are likely to check the tendency of Taiwan's national elite to move in this direction.

With all its unseemliness, the local political system has brought tangible rewards to the voters of Taiwan. Local politicians have also benefited. Moreover, the system has tied the politicians close to the voters and has made them leaders of opinion and action. If the national elite tried to do away with this system, it would be very difficult to convince the voters and politicians that any substitute would be more in their interests.

The problem is compounded because the national elite is identified as a nonelected group which did not even originate in the majority community. For the elite to repress the real elected leaders of the majority community and

reactions of other national elites to their local political systems, see Scott, "Corruption, Machine Politics, and Political Change," pp. 1155, 1157.

⁵⁹ For examples of the attitudes of local politicians, see *T'ai-wan Sheng I-hui Ti-san-chieh Ti-pa-tz'u Ta-hui Chuan-chi*, vol. 2, pp. 2525–26, 2534, 2569, 2574, 2579. These and other statements of local politicians are translated in Lerman, pp. 142–85. For the attitudes of India's local politicians, see Weiner, pp. 231, 241.

claim that somehow it is a better representative than they would be very difficult for the majority to accept.

Constraints such as the above, however, have not discouraged other national elites from doing away with electoral systems. The reason why the feelings of the majority community and its leaders are so important for the Taiwan national elite stems from the recent widespread rejection of the elite by the world's other national elites along with their respective national communities. Taiwan's national elite could probably not survive if its own majority community joined this general withdrawal of legitimacy.

Indeed, one of the only remaining supporters of the Taiwan elite, the United States, is far more tolerant of imperfect democracy and would likely become less supportive of the Taiwan elite if the latter did away with its fledgling democratic system.

Thus, in spite of its distaste for the local political system, the Taiwan national elite can only deepen its own complicity in electoral politics, even committing itself to a slow expansion of the scope of elections into national politics if it is to have a chance at remaining the governing elite of Taiwan.⁶⁰

60It hardly seems necessary to add that this is exactly the direction in which the national elite appears to be moving (though with great hesitation). See, for example, J. Bruce Jacobs, "Taiwan 1972: Political Season," Asian Survey, 13 (January 1973), 106–07, and J. Bruce Jacobs, "Taiwan 1973: Consolidation of the Succession," Asian Survey, 14 (January 1974), 26–29.

The Socio-Economic Determinants of Popular-Authoritarian Electoral Behavior: The Case of Peronism*

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In several highly mobilized Third World societies, rising levels of working-class political activism seem to have encouraged the development of political movements which are both popular and authoritarian. This popular authoritarianism melds intensive political mobilization of previously excluded social sectors with political structures which severely limit these groups' ability to affect public policy. Much of the research on popular authoritarianism has attempted to explain the phenomenon by identifying the socioeconomic determinants of popular-authoritarian electoral behavior. In an effort to clarify the relative merit of contending explanations, this study uses data from the prototypic case of Argentine Peronism to test six common hypotheses and then to construct a model which optimizes the explanatory ability of five major socioeconomic variables. The results indicate that an area's rate of industrial growth and the size of its working-class population account for more than four-fifths of the variation in Peronist electoral behavior that can be attributed to socioeconomic variables.

Most of the earliest analyses of Peronism's electoral popularity by United States diplomats, journalists, and social scientists were distorted by an ideological predisposition to discover a fascist mind beneath the cap of South American military officers trained in prewar Germany or Italy. Possibly for this reason alone it was once considered acceptable to explain Juan Perón's popular success by noting his demogogic ability to hoodwink Argentina's rapidly expanding urban proletariat into exchanging its political freedom for evanescent economic and psychic gratifications.1 Today, when an academic emphasis upon rational decision making often is combined with a renewed faith in the Third World working

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1 Spruille Braden, Diplomats and Demagogues: The Memoirs of Spruille Braden (New Rochelle, N.Y.: Arlington, 1971), pp. 340, 356-58; Fleur Cowles, Bloody Precedent (New York: Random House, 1952), p. 149; Ray Josephs, Argentine Diary: The Inside Story of the Coming of Fascism (New York: Random House, 1944), pp. xi-xlvi; Robert J. Alexander, The Perón Era (New York: Columbia University Press, 1951), p. 12; Arthur P. Whitaker, The United States and Argentina (Cambridge, Mass.: Harvard University Press, 1954), pp. 118-21.

class's autonomous role as an agent of social change, a new generation of social scientists has attempted to redefine our understanding of the relationship between the Peronist movement and the masses it purports to represent.²

The rationale for such a major intellectual expenditure rests at least in part on the assumption that Peronism is prototypic of a prominent political phenomenon found in several Third World societies exhibiting advanced levels of social mobilization and highly differentiated roles and institutions. In these

²Aggregate data studies include Gino Germani, Estructura social de la Argentina. Análisis estadístico (Buenos Aires: Raigal, 1955), Ch. 16; Pedro Huerta Palau, Análisis electoral de una ciudad en desarrollo, Córdoba, 1929–1957–1963 (Córdoba: Universidad Nacional de Córdoba, 1963); Peter G. Snow, "The Class Basis of Argentina Political Parties," American Political Science Review, 63 (March 1969), 163–67; Floreal H. Forni and Pedro D. Weinberg, "Reflexiones sobre la relación entre clases sociales y partidos políticos en la Argentina," Desarrollo Económico, 12 (July-September 1972), 421–36; Paul H. Lewis, "The Female Vote in Argentina, 1958–1965," Comparative Political Studies, 3 (January 1971), 425–41; Peter H. Smith, "The Social Base of Peronism," Hispanic American Historical Review, 52 (February 1972), 55–73; Walter Little, "Electoral Aspects of Peronism, 1946–1954," Journal of Inter-American Studies and World Affairs, 15 (August 1973), 267–84; E. Spencer Wellhofer, "The Mobilization of the Periphery: Perón's 1946 Triumph," Comparative Political Studies, 7 (July 1974), 239–51. To date the only major work on Peronism utilizing survey data is Jeane Kirkpatrick's important Leader and Vanguard in Mass Society: A Study of Peronist Argentina (Cambridge, Mass.: MIT Press, 1971).

nations, rising levels of working-class political activism have encouraged a type of government which is both popular and authoritarian. This popular authoritarianism melds intensive political mobilization of previously excluded social sectors with political structures which severely limit these groups' ability to affect public policy.³ Popular authoritarianism is an appropriate label not only for Peronism but also for the Vargas regime in Brazil, the Velasco Alvarado regime in Perú, and the Cárdenas regime in Mexico. As opposed to bureaucratic-authoritarian governments which attempt to exclude the mobilized popular sectors of society from political participation, these popular-authoritarian governments seek to activate and incorporate major segments of the popular sector into national political activity. As O'Donnell and others have noted, however, incorporation is accompanied by stringent restraints upon autonomous political activity.4

The ubiquitous nature of social and political mobilization suggests that popular authoritarianism may become increasingly common, providing the principal alternative to both bureaucratic authoritarianism and social revolution. For this reason, political researchers are now struggling to accommodate the needs of policy makers who, lacking alternative explanations, might continue to label (and relate to) the Third World's Peróns and Cárdenases as fascist and/or communist demagogues. But popular authoritarian regimes contain paradoxical elements—mobilization and repression—which make their ideologies and public policies appear

³As employed here, social mobilization refers to the process described by Karl W. Deutsch in "Social Mobilization and Political Development," American Political Science Review, 55 (September 1961), 494. Two studies of Argentine politics that employ beutsch's perspective are Gino Germani, Politica y sociedad en una época de transición. De la sociedad tradicional a la sociedad de masas, 4th ed., rzv. (Buenos Aires: Editorial Paidos, 1971); and Peter H. Smith, "Social Mobilization, Political Participation, and the Rise of Juan Perón," Political Science Quarterly, 84 (March 1969), 30–49. Political mobilization is the term used by Huntington to describe the political participation of previously passive social sectors. Samuel P. Huntington, Political Order in Changing Societies (New Haven, Conn.: Yale University Press, 1969), pp. 266–70, 354–55.

⁴Guillermo A. O'Donnell, Modernization and Bureaucratic Authoritarianism: Studies in South American Politics (Berkeley: Institute of International Studies, University of California Press, 1973), pp. 89–95, 111; James M. Malloy, "Authoritarianism and Corporatism in Latin America: The Modal Pattern," in Authoritarianism and Corporatism in Latin America, ed. James M. Malloy (Pittsburgh: University of Pittsburgh Press, 1977), p. 14.

vague and often contradictory to United States social scientists. Perhaps for this reason scholarly interest has centered upon a more easily understood aspect of popular authoritarianism: the attitudes and characteristics of popular authoritarian mass support. Given social scientists' belief that voters are capable of defining their needs and aspirations and then of rationally choosing to support candidates most willing to fulfill them, the analysis of popular-authoritarian electoral behavior has become a method for understanding the social base of such movements. With this knowledge at hand, if we can assume that popular-authoritarian elites are willing to respond to their constituents' needs, then it should be possible to explain or, perhaps, to anticipate some of the broader policies of future popular-authoritarian regimes.

While tenuous assumptions concerning voter rationality, elite responsiveness, and the political implications of social mobilization in the Third World continue to cry out for attention, concurrent questions must also be addressed. The answer to one of the most important of these questions involves determining the nature of popular-authoritarian mass support. Given the general absence of survey data in the Third World, for this task social scientists have tended to rely upon the comparative analysis of aggregate electoral behavior and socioeconomicdemographic variables. In the prototypic case of Argentine Peronism, more than 20 years have now passed since Germani's initial inquiry into the relationship between a central socioeconomic variable, social class, and Peronist electoral behavior. Since 1955 other students of Argentine politics have contributed analyses of additional variables and offered alternative explanations which have greatly refined our explanatory capability. By now virtually every socioeconomic variable has been mustered into the great explanatory fray, each accompanied by batteries of convincing arguments to protect its intellectual flanks.

The pages that follow identify the major hypotheses which have been offered to explain the socioeconomic nature of Peronist electoral behavior and then test these hypotheses with new, more sensitive data from the precincts and wards of the Federal Capital (Buenos Aires) and from the surrounding counties which, together with the Federal Capital, comprise Greater Buenos Aires. The analysis covers the entire 27-year electoral history of Peronism from 1946 to 1973. (The data and the manner in which they have been manipulated are described in the appendices.) The purpose of these tests is to assess the relative value of each

hypothesis by constructing an optimum model to explain the socioeconomic determinants of Peronist electoral behavior.

H₁: A Socially Mobilized Environment and a Plural Political Party Structure Encouraged Class-Oriented Electoral Behavior Before the Advent of Peronism

This initial hypothesis merits consideration because political mobilization has the potential to influence profoundly the nature of group consciousness.⁵ Unless it can be demonstrated that previously mobilized groups switched their allegiance when a more attractive actor appeared to champion their interests, social scientists will never be able to ascertain whether these groups' electoral behavior is a product of the aggregated social characteristics of a movement's supporters or the result of a mobilization-induced consciousness. This fact, and the disparate social physiognomy of the original Peronist coalition-organized urban labor, a nationalistic military, and, perhaps, the "new" industrialists⁶-should suggest special care be taken when analyzing the socioeconomic correlates of the Peronist electorate.

Questions concerning the independent influence of political mobilization upon voting behavior have led to several studies of pre-Peronist electoral patterns. Far from reaching a consensus on the issue of prior political mobilization, two contradictory hypotheses currently vie for acceptance as the conventional wisdom. Peter Smith has marshaled an impressive array of historical data to demonstrate that the processes of social mobilization, political mobilization, and political integration were well advanced throughout the lower classes years before the 1943 coup ousted President Castillo. Smith is particularly careful to avoid equating political mobilization with political influence:

In the late nineteen-thirties and early forties leaders ... sought to promote urban mass interests through established and constitutional

⁵For a more deterministic view of the interaction between mobilization and group consciousness, see Gerald A. Heeger, *The Politics of Underdevelopment* (New York: St. Martin's Press, 1974), p. 7.

⁶Eldon Kenworthy has persistently questioned this early conventional wisdom. See his "Did the 'New Industrialists' Play a Significant Role in the Formation of Perón's Coalition, 1943–1946?" in New Perspectives on Modern Argentina, by Alberto Ciria et al. (Bloomington: Latin American Studies Program, Indiana University, 1972), pp. 15–28; and "The Formation of the Peronist Coalition" (Ph.D. dissertation, Yale University, 1970).

political institutions. Before 1943 these leaders, mostly Socialists, were stopped at every turn by the ruling Concordancia; and the institutions through which they pressed their demands, particularly the national Congress, were devitalized and discredited. The critical point is that socially mobilized groups sought political participation, but were not given access to power.⁷

Gino Germani and Peter Snow, however, were unable to discern any significant pre-1943 lower-class political mobilization as indicated by class-oriented electoral behavior. Comparing the occupational structure and electoral statistics from each of the Federal Capital's 20 secciones, Germani found that "prior to 1946 the computed correlations do not reveal the existence of parties with homogeneous electorates from the point of view of their occupational composition."8 Using Germani's data and a study of electoral behavior in the interior city of Cordoba, Peter Snow subsequently reported that "prior to the 1943 revolution . . . correlations between social class and voting behavior were quite insignificant.... There is ... some evidence which indicates that occupation had little effect upon voting behavior during this period.... It also appears that occupation became an important factor in voting behavior after 1943 One should remember that just six years earlier such correlations virtually did not exist."9 It is important to note that Snow's only interest was in comparing the conservative Concordancia with the centrist Unión Cívica Radical (UCR). For that reason the labor-oriented Socialists, the party which ran second to the UCR in 1940 and first in 1942, received no mention.

Data from the Federal Capital's electoral precincts (circuitos) offer a serious challenge to the Germani-Snow thesis that class-oriented electoral behavior first appeared in Argentina

⁷Smith, "Social Mobilization, Political Participation, and the Rise of Juan Perón," p. 48.

⁸Germani, Estructura social de la Argentina, p. 260. [My translation.] The Spanish: "Así, mientras con anterioridad a 1946 las correlaciones computadas no revelan la existencia de partidos con electorado homogéneo desde el punto de vista de su composición ocupacional..."

⁹Snow, "The Class Basis of Argentine Political Parties," pp. 163–64. The Córdoba study is that of Huerta Palau, Análisis electoral de una ciudad en desarrollo. Supporting the Germani-Snow thesis is Ezequiel Gallo (h.) and Silvia Sigal, "La formación de los partidos políticos contemporaneos: la UCR (1890–1916)," in Argentina, sociedad de masas, by Torcuato S. Di Tella et al. (Buenos Aires: Editorial Universitaria de Buenos Aires, 1965), p. 163.

with Peronism's initial electoral victory. They also encourage the belief that lower-class political mobilization was well advanced by 1942. Table 1 presents the zero-order Pearson correlations between occupation and the vote for eight of the ten parties participating in the 1942 Federal Capital elections for national deputies.¹⁰ Strongly conservative voting patterns appear restricted to areas with high percentages of professionals and students. Since the Partido Conservador received less than 1 percent of the total vote and the Union de Contribuyentes scarcely fared better, the conservative vote is best indicated by results for the Lista de Concordancia, literally a list of candidates put forward by the semi-institutionalized coalition of Right and center-Right parties which governed Argentina throughout the 1930s and early 1940s. With over 21 percent of the popular vote, the Concordancia presumably provided conservative electors with an acceptable alternative to the centrist UCR and the three labor-oriented parties. These coefficients directly contradict Germani and Snow.

The same cannot be said of the vote for the UCR, for although 29 percent of the electorate selected this option, no particular social group supplied a disproportionate share of the total UCR electoral strength. While similarly weak correlations are displayed by the very small Partido Socialista Obrero, the two principal labor-oriented parties, the Partido Socialista (PS) and the Concentración Obrera, exhibit rather strong social class relationships. Since the latter received slightly less than 8 percent of the

10Two parties (the Partido Salud Pública and the Unión Republicana) received only nominal electoral support and have been excluded from all computations.

popular vote, much greater attention should be accorded the PS, which actually won the election with one-third of all votes cast. By suggesting a strong linkage between the working class and the vote for the labor-oriented Socialists, these data also contradict Germani and Snow.

This assertion is placed in sharper relief by collapsing the eight parties into three categories: labor, center, and conservative. The coefficients in Table 2 indicate fairly strong tendencies toward conservativism among professionals and toward laborism among blue-collar workers. Support for the centrist parties continues to be of a multiclass nature. In short, these data from the 1942 election suggest that there is simply no justification for the hypothesis that class-conscious voting patterns first emerged with the initial Peronist election in 1946.

Although it is difficult to overlook the Socialists' consistently strong prolabor ideology, among Argentine social scientists there exists a tendency to deny the working-class orientation of the party. ¹¹ But the fact that the PS adopted a parliamentary approach to reform and eschewed violent revolution should no more disqualify it as the working class' spokesperson in Argentina than it would the Labour party in Britain. Nor should the So-

11Ernesto Sábato, El otro rostro del peronismo, carta abierta a Mario Amadeo (Buenos Aires: Impresa López, 1956), p. 20; José Luis de Imaz, Los que mandan (Those Who Rule), trans. with an introduction by Carlos A. Astiz with Mary F. McCarthy (Albany, N.Y.: State University of N.Y. Press, 1970), p. 208; Dario Cantón, Party Alignments in Argentina between 1912 and 1955 (Buenos Aires: Instituto Torcuato Di Tella, 1967), p. 11; Carlos R. Melo, Los partidos políticos argentinos, 3rd ed. (Córdoba: Universidad Nacional de Córdoba, 1964), pp. 43-44.

Table 1. Social Class: Pearson Correlations between Occupation and Party Vote (Federal Capital, 1942; N = 209)

	Party										
Occupation	PS	Concen- tración Obrera	Socialista Obrero	UCR	Radical	Unión de Contribu- yentes	Concor- dancia	Conser- vador			
% Professionals	39	36	20	20	02	.53	.55	.00			
% Students	36	39	29	10	.00	.49	.50	08			
% Businessmen	.11	12	20	04	08	.17	.00	.00			
% White-collar employees	.13	08	.06	.13	.04	.09	21	.05			
% Blue-collar workers	.29	.49	.15	.02	.03	62	39	08			
% Unskilled workers	03	.21	.13	.02	.00	14	07	04			
% of total vote	33.1	7.5	4.1	28.9	2.5	1.8	21.4	0.7			

Source: Secretaría Electoral de la Capital Federal.

Table 2. Social Class: Pearson Correlations between Occupation and Party Vote (Federal Capital, 1942; N = 209)

	Party Orientation					
Occupation	Labor ^a	Center ^b	Conservative ^c			
% Professionals	49	14	.58			
% Students	49	05	.52			
% Businessmen	.04	06	.01			
% White-collar employees	.09	.17	21			
% Blue-collar workers	.45	04	43			
% Unskilled workers	.07	.01	08			

^aLabor: Concentración Obrera, Partido Socialista Obrero, PS.

Source: Secretaría Electoral de la Capital Federal.

cialists' middle-class intellectual leadership be interpreted as an obstacle to working-class attraction by the same intellectuals who acknowledged Jorge Abelardo Ramos and Silvio Frondizi as leaders of working-class parties in the early 1970s. In the enforcement of legislation related to social welfare concerns, Peronism accomplished what the Socialists could have accomplished had they enjoyed Perón's monopoly on coercion. His initial years in power were characterized by the enthusiasm with which he enforced, as previous non-Socialist chief executives had ignored, a rich legacy of progressive labor and welfare legislation introduced by Socialist legislators. 12

In any event, these and similar arguments concerning the commitment of the PS to the working class have been allowed to obscure clear evidence that prior to 1946 many members of the working class perceived the Socialists as their champion in Argentine politics. At the very least, the correlations in the preceding tables serve to support Smith's assertion that the working class was politically mobilized (and pro-Socialist) before the arrival of Peronism. Thus it is not unreasonable to conclude that the vote for the Peronist movement represents at least in part a response to perceived socioeconomic conditions rather than to a unique conciencia peronista produced during a hypothetical Perón-induced process of political mobilization.

12Partial listings of this legislation may be found in Adolfo Dorfman, Historia de la industrial argentina (Buenos Aires: Escuela de Estudios Argentinos, 1942), pp. 194–96; Carlos S. Fayt, La naturaleza del peronismo (Buenos Aires: Viracocha, 1967), p. 91. The PS was not the only party with an interest in labor and welfare legislation, for their minority status in Congress indicates that each bill became law only with the help of members of other political parties.

H₂: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to the Nature of its Social Class Structure

This hypothesis is so nearly a truism that it would hardly merit further consideration were it not for the fact that most prior analyses have interspersed their discussions of social class, particularly the working class, with other independent variables. Thus Peronism attracted "a growing mass of politically unrepresented urban laborers newly migrated from the rural areas" 13 which may have become inconstant following "acclimatization of the workers to city life," 14 Even those studies that do isolate social class as a causal variable often find it necessary to employ unwieldy units of analysis or to offer subjective ordinal classifications of electoral districts that not only limit the nature

13Peter Ranis, "Electoral Systems and Party Competition: The Case of Argentina," unpublished manuscript, Department of Political Science, The State University of New York at Stony Brook, n.d., p. 10.

14David J. Butler, "Charisma, Migration, and Elite Coalescence: An Interpretation of Peronism," Comparative Politics, 1 (April 1969), 433. See also Torcuato S. Di Tella, El sistema político argentino y la clase obrera (Buenos Aires: Editorial Universitaria de Buenos Aires, 1964), pp. 10, 13; Alfredo Parera Dennis, "Apuntes para una historia del peronismo," Fichas, 2 (October 1965), p. 13; Pedro Geltman, "Mitos, simbolos, y héroes en el peronismo," in El peronismo, by Gonzalo Cárdenas et al. (Buenos Aires: Carlos Pérez, 1969), p. 123; Jean-Claude García-Zamor, "Justicialismo en Argentina: la ideología política de Juan Domingo Perón," unpublished manuscript, Department of Government, University of Texas at Austin, n.d., pp. 5-6; Gino Germani, Política y sociedad en una época de transición, p. 324; Fayt, La naturaleza del peronismo, pp. 70, 86; Roberto Carri, Sindicatos y poder en la Argentina (Buenos Aires: Editorial Sudestada, 1967), p. 48.

bCenter: Partido Radical, UCR.

^cConservative: Partido Conservador, Lista de Concordancia, Unión de Contribuyentes.

of statistical inquiry but also are subject to the misperceptions which can accompany impressionistic classification. Employing the smallest units available for aggregate analysis, the present discussion focuses exclusively upon social class as measured by occupation.

The social class orientation of Argentine politics is demonstrated most dramatically by the correlations between occupation and candidate preference in 1946. The Perón-Quijano

15An example of the former problem is found in Germani's "El surgimiento del peronismo: el rol de los obreros y de los migrantes internos," Desarrollo Económico, 13 (October-December 1973), 435–88. The methodological and theoretical content of Germani's article has already received substantial comment. See Peter H. Smith, "Las elecciones argentinas de 1946 y las inferencias ecológicas," Desarrollo Económico, 14 (July-September 1974), 385–98; Eldon Kenworthy, "Interpretaciones ortodoxas y revisionistas del apoyo inicial del peronismo," Desarrollo Económico, 14 (January-March 1975), pp. 749–63; Tulio Halperin Donghi, "Algunas observaciones sobre Germani, el surgimiento del peronismo y los migrantes internos," Desarrollo Económico, 14 (January-March 1975), pp. 765–81. The latter problem of subjective classification can be partially overcome by careful selection of a limited number of cases as, for example, in Snow's "The Class Basis of Argentine Political Parties," pp. 165–66.

coefficients in Table 3 indicate the existence of a tendency toward upper-class and workingclass electoral polarization. 16 This is common knowledge, of course, and needs little emphasis here. The correlations between occupation and the Peronist vote in the 209 precincts of the Federal Capital between 1946 and 1973 are presented in Table 4; Table 5 contains similar data for the 39 wards and counties of Greater Buenos Aires. Unquestionably there exists a tendency for these aggregate electoral data and broad occupational categories to obscure fine distinctions and overemphasize the unidirectionality of upper-class and working-class electoral behavior. But it nevertheless remains apparent that areas of relatively large workingclass social composition are those which strongly support the Peronist movement.

These data linking the working class to Peronism were confirmed by Jeane Kirk-

16Special care must be taken when comparing these data with those from the 1942 election, for a radical realignment of parties occurred prior to the initial Peronist victory when virtually all of Argentina's established political parties confirmed Charles Dudley Warner's observation about strange bedfellows by uniting against Perón in the *Unión Democrática*.

Table 3. Social Class: Pearson Correlations between Occupation and the Peronist Vote (Federal Capital, 1946; N = 209)

Occupation	Perón—Quijano ^a
% Professionals	70
% Students	67
% Businessmen	41
% White-collar employees	04
% Blue-collar workers	.79
% Unskilled workers	.24

^aEndorsed by the Partido Laborista, UCR-Junta Renovadora, Juventud Renovadora Argentina, and the Partido Patriótico 4 de Junio.

Source: Secretaría Electoral de la Capital Federal.

Table 4. Social Class: Pearson Correlations between Occupation and the Peronist Vote (Federal Capital, 1946–1973; N = 209)

Occupation	PERON ^a	1946	1951	1957	1960	1962	1965	1973
% Professionals	71	70	66	61	67	66	65	71
% Students	72	67	68 ·	63	69	64	69	71
% Businessmen	47	41	39	44	43	44	49	45 ¹
% White-collar employees	05	04	11	12	05	.01	06	.04
% Blue-collar workers	.81	.79	.80	.70	.76	.71	.74	.70
% Unskilled workers	.33	.24	.26	.33	.33	.28	.35	.34

^aPERON is a composite variable representing the sum of the products of each election's standardized score and its factor score coefficient (see Appendix A).

Source: Secretaria Electoral de la Capital Federal.

patrick's 1965 survey analysis.¹⁷ While she emphasized that Peronism is not an exclusively single-class movement, Kirkpatrick produced the data in Table 6 to support her conclusion that the Peronists are disproportionately associated with the Argentine working class. And, significantly, she also found that these "lower-class Peronists demonstrated a class consciousness unique among Argentines." ¹⁸

The insignificant values registered by white-collar employees in the Federal Capital (Table 4) contradict similar data from Greater Buenos Aires (Table 5) and occasional assertions that the Argentine middle class was and remains vehemently anti-Peronist. Juan José Sebreli's influential work, for example, characterizes Peronism as "a challenge to petite bourgeois traditions, customs, established values, moral clichés, Philistine inhibitions, and hypocritical ideology of virtue.... The middle class reacted to this historical process which it did not understand with an hysterical anti-Peronism." 19 Recent empirical studies cast doubt

upon Sebreli's interpretation and the negative correlations in Table 5. The more reliable data from the Federal Capital's 209 precincts (Table 4) produce directionless correlation coefficients. In addition, Kirkpatrick's survey discerned a tendency for the Peronist movement to attract a substantial middle-class following. "Middle-income respondents are well represented among Peronists, comprising approximately one-third of the persons who supported Perón for President and a larger portion of those with sympathy for the movement."20 Both Kirkpatrick's analysis and the data in Table 4 suggest that very little faith should be placed in Sebreli's impressions or in the reliability of the middle-class coefficients in Table

Veinte, 1965), pp. 98, 99, 102–03. Somewhat similar opinions of the middle class' attitude toward Peronism can be found in Torcuato S. Di Tella, "Populism and Reform in Latin America," in Obstacles to Change in Latin America, ed. Claudio Véliz (London: Oxford University Press, 1965), pp. 71–72; and in Carri, Sindicatos y poder en la Argentina, p. 61.

²⁰Kirkpatrick, Leader and Vanguard in Mass Society, p. 97. Peter Snow has used aggregate data to confirm the existence of substantial Peronist support in the middle class. See "The Class Basis of Argentine Political Parties," p. 166.

Table 5. Social Class: Pearson Correlations between Occupation and the Peronist Vote (Greater Buenos Aires, 1946-1973; N=39)

Occupation	PERON	1946	1954	1957	1960	1962	1965	1973
% Professionals, 1960	96	83	95	90	89		84	96
% Students, 1960	80	60	80	71	75	81	72	86
% White-collar employees, 1960	72	47	74	68	73	74	64	77
% Blue-collar workers, 1960	.91	.76	.91	.88	.88	.92	.80	.90

Source: Ministerio del Interior, Dirección Nacional Electoral; Dirección Nacional de Estadística y Censos, Censo nacional de población 1960.

Table 6. Socioeconomic Characteristics and Orientations to Peronism, 1965

	Lower (N = 721)	Middle (N = 960)	Upper (N = 157)
Would vote for Peron	32.0	13.0	6.4
Generally approve of Peronist movement	38.7	18.4	12.7
Would support candidate supporting Perón	32.6	15.2	7.0
Perón did most to harm Argentina	19.4	39.5	65.0
Would hurt Argentina for Perón to return	34.8	56.7	77. 7

^aClass characterization based on interviewer appraisal.

Source: Kirkpatrick, Leader and Vanguard in Mass Society, 97.

¹⁷Kirkpatrick, Leader and Vanguard in Mass Society, p. 94.

¹⁸Kirkpatrick, Leader and Vanguard in Mass Society, p. 96.

¹⁹ Juan José Sebreli, Buenos Aires, vida cotidiana y alienación, 3rd ed. (Buenos Aires: Ediciones Siglo

H₃: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to its Population Growth Rate

Demographic change has always shared with social class the leading position on any list of variables most crucial to an adequate explanation of the development of the Peronist phenomenon. Here again, however, descriptive analyses have tended either to blur together discrete demographic characteristics-migrants "uprooted from their native environment and out of place in the new"21-or, more frequently, to place demographic variables alongside other socioeconomic variables without attempting to assess their separate impacts: "These displaced persons were not only poor and resentful; coming from the interior, where the Indian strain had been strong in colonial and early national times, they were also so swarthy that most of the older portenos referred to them contemptuously..."²²

Early in the data preparation a factor analysis of the demographic variables indicated the existence of two distinct demographic dimensions (see Appendix B, Table B2). One, urbanization, maintained a correlation of +.10 with the second, somewhat broader category of population growth (POPULATION GROWTH) which was defined as the percent average annual population increase and the percent average annual change in population density. Although migrants undoubtedly contributed to the area's population growth, no reliable data have ever been found to document their proportional share of the overall population increase.²³ Urbanization was simply defined as the percent of a county or ward's total population living in population clusters of 2,000 or more persons in the census years of 1947 and 1960.

Given the fact that in 1977 Greater Ruenos

Given the fact that in 1977 Greater Buenos Aires' eight million inhabitants are vying with those of Mexico City for the distinction of residing in the largest Spanish-speaking metropolis on earth, the area's negligible variation in percent urban population renders meaningless an analysis of the independent influence of urbanization upon the Peronist vote. While it is true that in 1947 and even in 1960 there was considerable variation in the percent urban population among Greater Buenos Aires' counties-the mean percent urban in 1947 was 92 percent with a range from 42 to 100 percentsocial scientists nevertheless define urbanization broadly as the exposure to an urban environment through media, economic interaction, or physical proximity. By this definition the entire population of Greater Buenos Aires has long been as urban as any in the world. Given its insufficient variance, then, urbanization was eliminated from this study.

The literature on popular authoritarianism indicates that the second dimension of demographic change, population growth, should maintain an important positive relationship with the Peronist vote. With a zero-order POPULATION GROWTH-PERON correlation of +.69, the data in Table 7 tend to confirm the findings of earlier studies. Because much of Buenos Aires' demographic change may be attributed to working-class migration from the interior, it is possible that a more fundamental association between social class and Peronism underlies the POPULATION GROWTH-PERON relationship. Apparently population growth is more than just an intervening variable, however, for despite the +.35 WORKING CLASS-POPULATION GROWTH correlation, the first-order partial between POPULATION GROWTH and PERON controlling for WORK-ING CLASS remains a very high +.70. As Table B9 indicates, only when controlling for the composite variable of industrial growth (see Appendix B) is the POPULATION GROWTH-PERON correlation reduced from +.69 to +.08. And here, unfortunately, the intractable problem of multicollinearity (the POPULATION GROWTH-INDUSTRIAL GROWTH correlation is +.84) distorts the meaning of partial correlation coefficients.

²¹Juan Carlos Rubenstein, "El peronismo y la vida argentina," Fichas, 2 (December 1965), p. 7.

²²Arthur P. Whitaker, Argentina (Englewood Cliffs, N.J.: Prentice-Hall, 1964), p. 107. See also Tomás Roberto Fillol, Social Factors in Economic Development: The Argentine Case (Cambridge, Mass.: MIT Press, 1961), p. 83; Parera Dennis, "Apuntes para una historia del peronismo," p. 13; Geltman, "Mitos, símbolos, y héroes en el peronismo," p. 103; Germani, Política y sociedad en una época de transición, p. 324.

²³It is important to note that this study specifically avoids any discussion of the political influence of rural-urban migration. There are no reliable data on migrants during the early Peronist years and, unfortunately, the more reliable data on migrants from later periods (as, for example, the individual-level sample from the 1960 census) do not provide information on political behavior or attitudes. Since the census contains only information on interprovincial migration, aggregate data correlations between migrants and Peronism are a matter of heroic guesswork.

N = 200 X

H₄: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to its Level of Industrial Development

The literature on the origin and persistence of Peronism hypothesizes a direct linkage with an area's absolute level of industrialization, narrowly defined here in terms of factories and operationalized as the composite variable IN-DUSTRY²⁴ (see Appendix B, Table B8). Table 8 presents the zero-order Pearson correlations

²⁴Among the many studies linking industrialization to Peronism are Fayt, La naturaleza del peronismo, pp. 92-93; Whitaker, Argentina, p. 149; Carri, Sindicatos y poder en la Argentina, p. 36; O'Donnell, Modernization and Bureaucratic-Authoritarianism, p. 56; and Carlos F. Diaz Alejandro, Essays on the Economic History of the Argentine Republic (New Haven, Conn.: Yale University Press, 1970), pp. 112-13.

between INDUSTRY, its component variables, and the vote for the Peronist movement in Greater Buenos Aires. As measured by the number of workers per industrial establishment, wages per industrial establishment, and value of production per industrial establishment, INDUSTRY seems to be nearly unrelated to Peronist voting patterns. In 1946 and to a lesser extent in 1957, a positive association exists between areas containing large factories (workers per industrial establishment) and those possessing substantial Peronist electoral strength, but over time this relationship becomes insignificant.

25It is difficult to determine the extent to which all industrial establishments are included in these data. By law all industrial firms must report their activity to the census bureau, but it is probable that many of the smaller establishments fail to do so.

Table 7. Pearson Correlations between Population Growth and the Peronist Vote (Greater Buenos Aires, 1946-1973; N = 39)

	PERON	1946	1954	1957	1960	1962	1965	1973
POPULATION GROWTH	.69	.65	.76	.67	.76	.73	.66	.64
% Average annual population growth, 1914–1947	.67	.63	.71	.62	.66	.67	.62	.56
% Average annual population growth, 1947–1960 % Average annual change in population density,	.70	.44	.71	.64	.72	.72	.71	<i>.</i> 78
1914-1947	.67	.63	.71	.63	.67	.67	.62	.56
% Average annual change in population density, 1947–1960	.70	.67	.70	.69	.68	.70	.63	.63

Source: See Table 5.

Table 8. Pearson Correlations between the Level of Industrialization and the Peronist Vote (Greater Buenos Aires, 1946-1973; N = 39)

	PERON 1946					40.4		400
	PERON	1946	1954	1957	1960	1962	1965	1973
INDUSTRY	.15	.24	.07	.25	.06	.10	.15	.12
Workers per industrial establishment, 1935	.06	.23	06	.16	01	.03	.02	.02
Workers per industrial establishment, 1941	.15	.34	.11	.22	.03	.10	.13	.04
Workers per industrial establishment, 1947	.23	.38	.15	.32	.17	.18	.18	.15
Workers per industrial establishment, 1963	08	01	12	04	09	13	10	05
Wages per industrial establishment, 1935	13	03	26	04	17	14	12	13
Wages per industrial establishment, 1941	13	.01	19	05	22	16	11	17
Wages per industrial establishment, 1947	.04	.13	05	.12	.01	.01	.02	.01
Wages per industrial establishment, 1963	.00	.04	02	.02	02	03	.00	.02
Value of production per industrial establishment,								
1947	.13	.26	.04	.22	.08	.11	.11	.07
Value of production per industrial establishment, 1963	.20	.27	.18	.19	.17	.18	.17	.21

Source: Ministerio del Interior, Dirección Nacional Electoral; Comisión Nacional del Censo Industrial, Censo industrial de 1935; Dirección General de Estadística y Censos, Estadística industrial de 1941; Ministerio de Asuntos Técnicos, IV censo genera! de la Nación; Instituto Nacional de Estadística y Censos, Censo nacional económico 1963.

The lack of substantial evidence to confirm the expected positive relationship between an area's Peronist vote and its level of industrial development encourages a re-interpretation of the original hypothesis. A closer inspection of the literature reveals, in fact, that the simple INDUSTRY-PERON linkage is offered as a harbinger of changes not in an area's economic structure but rather in the nature of its social class composition. No one suggests that industry directly causes a large vote for Peronism, but that industrial development tends to attract the types of people, generally working-class people, who then go to the polls in support of the Peronists. If an area's working-class population serves as an intervening linking variable between industrial development and the Peronist vote, a partial correlation between IN-DUSTRY and PERON controlling for WORK-ING CLASS will indicate the proportion of the +.15 zero-order relationship that may be directly attributed to industrialization. Not surprisingly, the first-order partial is -.37, demonstrating not only that all of the positive influence of INDUSTRY on PERON is due to the fact that industrialization attracts workingclass voters, but also that industrialization actually has a negative impact upon the Peronist vote. Should an area manage to industrialize without attracting a disproportionately larger working-class population, it will exhibit relatively anti-Peronist voting patterns. Because this occurs so infrequently (the INDUSTRY-WORKING CLASS correlation is +.45) the question need not be pursued beyond noting once more the crucial influence of an area's working-class population upon its electoral behavior.

Outside Greater Buenos Aires are nearly 500 counties possessing incredibly varied levels of industrialization, where such major industrial centers as Rosario and Cordoba may be compared with counties inhabited by more glaciers than blue-collar workers. The zero-order correlations between the level of industrialization and the Peronist vote in these countries are presented in Table 9. Here again, INDUSTRY appears associated with neither positive nor negative Peronist electoral performance. No coefficient is able to account for as much as nine percent of the variation in the vote for Peronism.

H₅: The Proportion of an Area's Popular-Authoritarian Vote is Directly Related to its Rate of Industrial Growth

It has been asserted that the economic basis of Peronism's electoral strength lies not in an area's level of industrial development nor in its type of industrial activity, but rather in Argentina's differential levels of economic change. This process of economic change is said to initiate a complex series of interconnected socioeconomic and sociodemographic transformations which, taken together, encourage the development of popular authoritarianism. As narrowly defined here in terms of industrial growth, economic change acts as a catalyst to "detraditionalize" a region's social and political behavior. ²⁶

26Marcos Kaplan, Economía y política del petróleo argentino (1939-1956) (Buenos Aires: Praxis, 1957), p. 34; Guido Di Tella and Manuel Zymelman, "Etapas

Table 9. Pearson Correlations between the Level of Industrialization and the Peronist Vote
(All Argentine Counties, a 1946-1965)

	1946	1954	1957	1960	1962	1965
Workers per industrial establishment, 1947 Workers per industrial establishment, 1963	.14 .12	.20 .09	.15 .23	.11 .25	.20 .25	.28 .13
Wages per industrial establishment, 1947 Wages per industrial establishment, 1963	.13 .10	.17 .07	.22	.13 .29	.25 .24	.28 .17
Value of production per industrial establishment, 1947	.13	.10	.22	.13	.25	.09
Value of production per industrial establishment, 1963	.06	.04	.21	.26	.27	.13
N =	290	328	348	308	304	289

^aExcluding Greater Buenos Aires.

Source: Ministerio de Asuntos Técnicos, IV censo general de la Nación; Dirección Nacional de Estadística y Censos, Censo nacional de población 1960.

This hypothesis is supported by the Peronist electoral data. The Pearson coefficients in Table 10 indicate an extremely strong positive relationship between the rate of industrial growth and Peronist voting strength. These correlation coefficients also disclose that the areas which grew most rapidly between 1935 and 1941 were those that least supported Peronism in subsequent elections. This is due to dramatic transformations in several established patterns of industrial growth in the years following this prewar period. Areas which had evidenced negative industrial growth rates between 1935 and 1941 blossomed forth with an impressive expansion of industrial facilities. The Pearson correlation between increases in industrial establishments from 1935 to 1941 and increases from 1941 to 1947 is -. 70. From 1935 to 1941 the annual average economic growth rate of Lanus ranked thirty-fifth among Greater Buenos Aires' 39 counties, while from 1941 to 1947 it was sixth. Similar leaps occurred in General San Martin (twenty-ninth to third), Lomas de Zamora (thirty-third to seventh), and Tres de Febrero (thirteenth to second). Not all of this growth was concentrated in areas that subsequently provided strong Peronist electoral support, of course, but the correlations in Table

del desarrollo económico argentino," in Argentina, sociedad de masas, pp. 184–86; Fayt, La naturaleza del peronismo, p. 70; Carri, Sindicatos y poder en la Argentina, p. 36; Germani, Polltica y sociedad en una época de transición, p. 324; Geltman, "Mitos, símbolos, y héroes en el peronismo," p. 123; James R. Scobie, Argentina: A City and a Nation (New York: Oxford University Press, 1964), p. 187.

10 intimate that after 1941 increases in industrial establishments and industrial workers became and remain the most reliable economic indicators of Peronist strength.

But while it is difficult to deny that areas undergoing industrial growth tend to be areas of strong Peronist electoral support, it is also important to emphasize that the simple correlation coefficients in Table 10 do not indicate the amount of direct independent influence of an area's industrial change upon its Peronist vote. As the high coefficients in the independent variable correlation matrix indicate (see Appendix B, Table B10), an area undergoing industrial change also tends to have a relatively rapidly growing population, to be relatively workingclass in social composition, and to exhibit relatively little economic satisfaction. The question of the direct and indirect effects of industrial change upon popular-authoritarian electoral strength remains to be addressed below.

A similar analysis of the influence of indicators of industrial growth upon the Peronist vote in the Argentine counties outside Greater Buenos Aires did not match the strong correlations between industrial growth and the Peronist vote in the metropolitan area. Outside Greater Buenos Aires not a single zero-order correlation coefficient could account for as much as ten percent of the vote for Peronism. Even when correlations were computed between PERON and the indicators of industrial growth for the 90 counties exhibiting more than the mean rate of industrial change, the results revealed nothing significantly different from zero. Only when the nine counties with

Table 10. Pearson Correlations between Industrial Growth and the Peronist Vote (Greater Buenos Aires, 1946-1973; N = 39)

	PERON	1946	1954	1957	1960	1962	1965	1973
INDUSTRIAL GROWTH	.79	.66	.81	.74	.81	.77	.75	.74
 % Average annual change in industrial establishments, 1941-1947 % Average annual change in industrial establishments, 	.59	.58	.60	.58	.60	.60	.57	.44
1947–1963	.80	.69	.78	.73	.82	.82	.75	.79
% Average annual change in industrial workers, 1941–1947	<i>.</i> 68	.58	.66	.68	.71	.69	.66	.58
% Average annual change in industrial workers, 1947–1963	.64	.45	.64	.52	.68	.66	.61	.71
Variables NOT used in construction of the composite v	ariable I	NDUS'	TRIAL	GROV	VTH			
 % Average annual change in industrial establishments, 1935–1941 % Average annual change in industrial workers, 	50	50	44	57	45	51	51	37
1935–1941	45	31	34	52	45	47	48	44

Source: See Table 9.

more than 500 industrial establishments were grouped apart did impressive coefficients finally begin to appear. And, using Guilford's formula to deflate artificially high estimates obtained from small samples, even these lost their strength.²⁷

H₆: The Proportion of an Area's Popular-Authoritarian Vote is Inversely Related to its Level of Economic Satisfaction

Nearly all theories attempting to explain the development of popular authoritarianism emphasize the influential role of economic dissatisfaction in exacerbating the tensions of traditional politics and reducing the perceived legitimacy of traditional political actors to the point that new political groups can obtain access to the arenas of power. Thus an entire literature exists which relates the growth and persistence of the Peronist phenomenon to Argentina's economic woes.²⁸ Little effort has been made to separate the independent influence of economic satisfaction from lower social class membership, however, and many studies define somewhat narrowly the types of persons who might be unhappy with their economic conditions. That the weighty economic burdens of Third World capitalist societies fall most heavily upon those shoulders least able to support them cannot be doubted; that the working class is the only dissatisfied social sector or that economic discontent influences Peronist electoral behavior are hypotheses in need of empirical support.

Of all the variables under discussion in this

27J. P. Guilford, Fundamental Statistics in Psychology and Education, 4th ed. (New York: McGraw-Hill, 1965), pp. 400-01. See the Communication from David Pfotenhauer, American Political Science Review, 68 (March 1974), pp. 205-06.

28Rodolfo Mario Pandolfi, "17 de octubre, trampa y salida," Contorno, número 7-8 (July 1956), pp. 21-28; Kaplan, Economía y política del petróleo argentino, p. 34; Fayt, La naturaleza del peronismo, pp. 92-93; Whitaker, Argentina, p. 149.

study, economic satisfaction is perhaps the least compatible with ecological analysis. Unlike social status, economic change, or population growth, economic satisfaction refers to a totally subjective phenomenon that simply cannot be measured accurately. The best that can reasonably be expected from aggregate data is a comparative analysis of similar groups which, through the grouping process itself, tends to hold constant a number of potentially significant intervening variables. Because the data for this paper are from a relatively industrialized area-Greater Buenos Aires-and because Peronism apparently finds its most substantial support among industrial workers, attention is confined to industrial employees (obreros and empleados) and their economic satisfaction is defined in the simplest of terms-wages. While the crudity of this estimating procedure is evident, in a relatively urban environment with well-developed means of communications it is not unreasonable to expect that differential wage rates would tend to create groups with different levels of economic satisfaction.

Given these constraints, then, are variations in areas' levels of economic satisfaction associated with variations in Peronist electoral strength? The data in Table 11 indicate that a fairly strong negative relationship exists between economic satisfaction and the vote for Peronism. The simple Pearson correlation between SATISFACTION and PERON of -.47 tends to confirm earlier analyses linking economically dissatisfied voters to the Peronist movement. But a closer inspection of partial correlations casts doubt upon this relationship and suggests that the SATISFACTION-PERON linkage may be spurious. At issue here is the influence of the size of an area's working-class population upon both variables. Experience with urban immigrant concentrations in the United States and elsewhere has demonstrated that recent arrivals are generally forced to accept relatively low-status employment. Because much of Greater Buenos Aires' working class may be composed of internal migrants who are generally more satisfied with

Table 11. Pearson Correlations between Economic Satisfaction and the Peronist Vote (Greater Buenos Aires, 1946–1973; N = 39)

	PERON 1946	1954	1957	1960	1962	1965	1973
SATISFACTION	4760	50	49	42	44	38	33
Wages per industrial employee, 1935	3445	38	37	29	31	26	24
Wages per industrial employee, 1941	6066	62	62	59	58	52	47
Wages per industrial employee, 1947	4253	43	46	35	40	35	30

Source: See Table 9.

their new economic positions than with their former existence,²⁹ it is imperative to analyze the influence of an area's working class upon its level of economic satisfaction.

As it turns out, areas that might be classified as economically satisfied tend to be areas exhibiting relatively small working-class populations—the SATISFACTION—WORKING CLASS correlation is —.54. Conversely, the areas of greatest Peronist voting strength have relatively large working-class populations—the WORKING CLASS—PERON correlation is +.77. For this reason, at any given level of working-class population, the first-order partial correlation between SATISFACTION and PERON plummets from —.47 to a nearly directionless—.11. In short, accepting the logic of the preceding paragraphs leads to the conclusion that economic satisfaction maintains only a marginal relationship with the Peronist vote.

Because its use would involve incredibly tenuous assumptions, another independent variable—percent annual increase in wages per industrial employee—was omitted from the composite variable of economic satisfaction. But Perón has often been described as a vote-buyer, and for that reason alone the data

29A massive literature now exists to confirm the relative satisfaction of rural-urban migrants. See Joan M. Neison's somewhat dated but excellent Migrants, Urban Poverty, and Instability in Developing Nations (Cambridge, Mass.: Center for International Affairs, Harvard University, 1969), pp. 15–20; and Wayne A. Cornelius' comprehensive "The Political Sociology of Cityward Migration in Latin America: Toward Empirical Theory," in Latin American Urban Research, Vol. 1, ed. Francine F. Rabinovitz and Felicity M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1971), pp. 95–147. Speaking in terms more general than those of economic satisfaction, perhaps our contemporary understanding of the political impact of cityward migration is best described in the final paragraph of Cornelius' "Urbanization and Political Demand Making: Political Participation among the Migrant Poor in Latin American Cities," American Political Science Review, 68 (September 1974), p. 1146.

in Table 12 deserve mention. They indicate what many have long assumed to be true: a substantial change occurred in the relationship between Peronist voting strength and increases in wages per industrial employee as Peronism began to attract workers in areas noted for their wage increases. This does not mean that Peronism's constituency changed; rather it suggests that the economic position of the same constituency was altered dramatically in a brief period. The correlation between increases in wages from 1935 to 1941 and increases in wages from 1941 to 1947 is -.42, demonstrating that the beneficiaries of earlier wage increases lost their relatively advantageous position after 1941. Perhaps more illuminating than this summary measure is a glance at specific areas. From 1935 to 1941 industrial employees in Buenos Aires' relatively wealthy Socorro received an average annual hike in wages per capita of 3.4 percent compared to negative adjustments in wages per capita in working-class areas such as General San Martin (-1.5 percent), Lanús (-3.8 percent), and Lomas de Zamora (-7.0 percent) during the same period.30 Between 1941 and 1947 these roles were reversed, as Socorro's substantial average annual wage increase of 8.2 percent was more than matched by the three working-class counties' average of 11.3 percent. For those employees accustomed to wage reductions, the increase might have appeared even greater.

Multivariate Analysis

It should now be possible to assess the relative importance of these independent variables from a vantage point provided by our

30It is probable that the working-class districts' decreases between 1935 and 1941 stemmed more from population growth than from actual wage cuts, as migrants continued to flood into parts of Greater Buenos Aires and exert downward pressure upon wages per capita.

Table 12. Pearson Correlations between Industrial Wage Increases and the Peronist Vote (Greater Buenos Aires, 1946-1973; N=39)

	PERON	1946	1954	1957	1960	1962	1965	1973
% Average annual increase in wages per industrial employee, 1935-1941	51	38	47	52	58	52	54	50
% Average annual increase in wages per industrial employee, 1941–1947	.54	.43	.54	.55	.61	.50	.47	.47
% Average annual increase in wages per industrial employee, 1947–1963	.22	.21	.26	.21	.22	.25	.21	.26

Source: See Table 9.

understanding of the manner in which they are individually associated with the vote for Peronism. The purpose here is not to construct and then to test a series of causal models in an attempt to explain Peronist electoral behavior. When the tendency of aggregate data to generate impressive correlation coefficients is combined with our not infrequent inclination to accept quantitative data on face value, it is possible that the results of an uncontrolled regression of PERON on all five composite independent variables (see Table 13) may be misinterpreted. Despite the impressive coefficient of determination, .86, this regression equation "explains" variance only in the statistical sense of the word. To employ these data as a substantive explanation of Peronism's voting behavior would be tacitly to presume not only that these aggregate data accurately reproduce individual-level attitudes and behavior but that the independent variables are all those most likely to determine the Peronist vote-presumptions many would consider untenable. Other variables which are not based entirely upon socioeconomic characteristics-party organization, factory experience, union membership, to name but a few-need to be included in any such explanatory model. While the purpose of this inquiry is somewhat less ambitious, it is an essential step toward eventual explanation: what is proposed is a clarification of only the socioeconomic basis of variation in Peronist voting patterns. It is assumed that socioeconomic variables contribute an unknown amount toward an understanding of the movement's electoral behavior. It is assumed that most if not all of the socioeconomic variables have been identified.31 We wish to determine which among them are the most influential.

and the preceding discussions of hypothesized associations rather than from the data themselves. The purpose of this multivariate approach, therefore, is not to employ causal inference to reduce a large number of logical models to one or more optimum explanatory sequences. Rather it is to propose a fixed model and then to determine the relative influence of each of the model's linkages. The procedures followed here do not begin with the development of prediction equations specifying zero relationships, then, but take a given model and compute the path coefficients. With the causal order fixed, no linkages will be eliminated. This seems only logical in the present context, where the goal is to analyze variables representing specific hypotheses found throughout the literature on Peronism and to determine which among them are most important and which possess relatively little impact on the Peronist vote. The core hypothesis which informs the basic

This tentative understanding of the manner

in which the variables relate to one another is in

large measure derived from deductive theory

The core hypothesis which informs the basic socioeconomic model is that the rate of industrial growth is the fundamental independent variable acting directly and indirectly to influence the proportion of an area's Peronist vote. Figure 1 presents this causal model, and the appropriate path coefficients are given in Table 14. The large number of paths in Figure 1 should not be allowed to obscure some of the most important linkages, particularly the influence of INDUSTRIAL GROWTH upon PERON, WORKING CLASS, and POPULATION GROWTH as well as the impact of WORKING CLASS upon both PERON and

Tract Material," Journal of the American Statistical Association Supplement, 29 (1934), 169-70; G. Udny Yule and Maurice G. Kendall, An Introduction to the Theory of Statistics (London: C. Griffin, 1950), p. 311.

Table 13. Uncontrolled Multiple Regression of PERON on the Composite Independent Variables (Greater Buenos Aires)

Variable	R	R ²	b/Beta ^a	Standard Error b
INDUSTRIAL GROWTH	.79	.62	.49	.14
WORKING CLASS	.92	.84	.60	.11
SATISFACTION	.93	.86	.17	.09
POPULATION GROWTH	.93	.86	.14	.13
INDUSTRY	.93	.86	02	.09

^aSince each of the variables is standardized, the path coefficients are identical to the regression coefficients,

³¹ And, because they have been aggregated, we further assume that they tend to bias upwardly the individual-level relationship with the Peronist vote. See C. Gehkle and K. Biehel, "Certain Effects of Grouping upon the Size of the Correlation Coefficient in Census

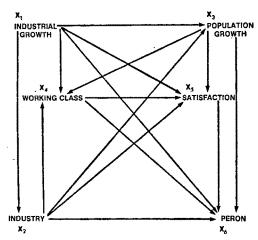


Figure 1. Six-Variable Causal Model:
Paths from Industrial Growth to the Peronist Vote

SATISFACTION. Table 14 indicates that industrial growth and working-class population totally dominate this six-variable model of Peronist electoral behavior.

Given the nature of the data it is extremely difficult to separate the influence of industrial growth and population growth upon Peronist voting patterns. Although these two variables undoubtedly reinforce one another, one intuitively suspects that an area's population would grow in response to industrial growth rather than vice versa. In any event, it is upon this logic that we must rely, for the POPULATION GROWTH-INDUSTRIAL GROWTH correlation of +.84 renders the magnitudes of the regression coefficients nearly impossible to assess.

One of the most intriguing questions raised by this model is the extent to which the rate of industrial growth directly influences the Peronist vote. In Table 15 an attempt has been made to determine the relative influence among the various paths from INDUSTRIAL GROWTH to PERON by first arriving at a basic correlation between industrial growth and PERON by summing the pathways of greatest theoretical interest. These pathways are the products of the path coefficients along each given path. With the basic correlation determined, the proportion of the correlations between IN-DUSTRIAL GROWTH and PERON attributable to various pathways can be calculated.32 Nearly 52 percent of the correlation is accounted for by a direct linkage. Linkages passing through the demographic variable (POPULATION GROWTH) and the other economic variables (INDUSTRY and SATISFAC-TION) scarcely appear to affect the relationship between industrial growth and the Peronist vote. More than 43 percent of the basic INDUSTRIAL GROWTH-PERON correlation may be attributed to a path through WORKING CLASS, the single social structure variable.

Figure 2 is a simplified version of the same model which reflects the relative importance of the various independent variables. All path coefficients of less than .200 have been omitted; all those of more than .200 but less than .500 are indicated by broken lines; and all those of .500 and over are indicated by solid lines. The three independent variables which maintain direct links to PERON in Figure 2 (INDUSTRI-

³²The format for Table 15 is a modified copy of Table 1 in Norman H. Nie, G. Bingham Powell, Jr., and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships, II," *American Political Science Review*, 63 (September 1969), 812.

Table 14. Path Coefficients for Figure 1

Dependent Variable Equation	B ₁	B ₂	В3	B ₄	B ₅
$X_6 = B_{61}X_1 + B_{62}X_2 + B_{63}X_3 + B_{64}X_4 + B_{65}X_5 + e_6$.512	071*	.044*	.709	.201
$X_5 = B_{51}X_1 + B_{52}X_2 + B_{53}X_3 + B_{54}X_4 + e_5$.029*	.288*	236*	607	
$X_4 = B_{41}X_1 + B_{42}X_2 + B_{43}X_3 + e_4$.605	.564	066*		
$X_3 = B_{31}X_1 + B_{32}X_2 + e_3$.830	031*			
$X_2 = B_{21}X_1 + e_2$	208				
$\mathbf{X}_1 = \mathbf{e}_1$					

X₁ = INDUSTRIAL GROWTH

 $X_2 = INDUSTRY$

 X_3^2 = POPULATION GROWTH

X4 = WORKING CLASS

 $X_5 = SATISFACTION$

 $X_6 = PERON$

^{*}Path coefficients with associated regression coefficients less than twice their standard errors.

AL GROWTH, WORKING CLASS, and SATIS-FACTION) account for all but one percent of the total variation in the Peronist vote that can be attributed to these socioeconomic variables. The addition of INDUSTRY and POPULATION GROWTH increases the coefficient of determination from .856 to .863.

It is difficult to overestimate the combined influence of an area's rate of industrial growth and the size of its working-class population upon Peronism's electoral behavior. By themselves they explain 84 percent of the total variation in PERON, or slightly more than 97 percent of the variation in PERON explained by all five independent variables.

Conclusion

These data from Argentina Peronism provide a number of insights into the socioeconomic determinants of popular-authoritarian electoral behavior. Prime among these is the crucial role assumed by industrial growth in (1) directly contributing to an area's popular-authoritarian voting strength, and (2) inducing changes in an area's social class composition which, in turn, also dramatically affect the vote for popular-authoritarian political movements.

One of the most persistent questions to arise out of the theoretical literature on social and political mobilization in the Third World concerns the specific impact of economic variables upon political behavior. At the center of this study is an inquiry into the direct and indirect ability of one crucial variable—the rate of industrial growth—to produce a particular type of political participation, i.e., electoral support for popular-authoritarian movements. If the Argentine data may be extrapolated to other relatively modern Third World nations, it ap-

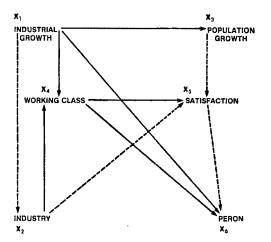


Figure 2. Six-Variable Causal Model: Selected Paths from Industrial Growth to the Peronist Vote

pears that industrial growth not only exerts an impressive direct influence upon the level of the popular-authoritarian vote, but substantially (beta = +.605) adds to an area's working-class population as well. Once this has occurred, these two independent variables then explain an extremely large percentage (in Argentina, 84 percent) of the vote for popular-authoritarian political movements.

The Argentine case also provides insight into the negligible influence of other variables often considered of primary importance in determining the popular-authoritarian vote. An area's level of industrial development, in particular, maintains no strong direct relationship with its popular-authoritarian electoral strength, although it does tend to exhibit a significant indirect influence through its ability to attract large numbers of working-class voters. This may

Table 15. Proportional Components of the Relationship between Industrial Growth and the Peronist Vote (Greater Buenos Aires)

Proportion of Basic Correlation Attributable to:		Path Coefficients		
1. Direct link to PERON		.512	52.0%	
2. Indirect link through demographic variable:				
INDUSTRIAL GROWTH \rightarrow POPULATION GROWTH \rightarrow PERON		.036	3.7	
3. Indirect links through other economic variables:				
INDUSTRIAL GROWTH → INDUSTRY → PERON INDUSTRIAL GROWTH → INDUSTRY → SATISFACTION → PERON INDUSTRIAL GROWTH → SATISFACTION → PERON	.014 011 .005			
		.008	0.8	
4. Indirect link through WORKING CLASS		.428	43.5	
Basic correlation between INDUSTRIAL GROWTH and PERON		.984	100.0%	

seem so obvious as to merit scant mention, but factory experience per se is believed to be a crucial link in the creation of a participant working class. Factories are Inkeles' "schools for citizenship."33 Thus the absence of a relationship between industrial development and popular authoritarianism suggests either that the factory does not develop participant citizens or, more likely given the thoroughness of Inkeles' study, that the factory creates styles of participation which, relatively speaking, reject popular authoritarianism. While the data in this paper hardly bear comparison to those of Inkeles and Smith, they do provide the foundation for justifying future analyses of an entirely unexpected association.

These data also suggest an interesting insight into the relationship between a demographic variable, population growth, and popular-authoritarian electoral behavior. No doubt part of the negligible direct association between Argentine population growth and the vote for Peronism can be attributed to the inability of these data to distinguish between growth due to migration and nonmigratory increases in the resident population (see note 23). But population growth has long been thought to be closely linked to an area's vote for popular authoritarianism, for growth transmutes all too easily into instability and, until recently, demographic instability in Third World environments carried with it the specter of anomic disposable masses willing to trade their votes and multiple anxieties for strong leadership from charismatic politicians. Now that we are aware of the pitfalls inherent in a macro-micro leap from growth to anomic political behavior (see note 29), there is no reason to expect to discover a direct association between population growth and the vote for popular-authoritarian political movements, and none was found in Argentina.

Finally, because these data appear to explain such a large proportion of the popular-authoritarian vote that may be attributed to socioeconomic variables, they provide the opportunity to suggest that future research into the phenomenon of popular-authoritarian mass support might now concentrate upon the amount of the total variation in the popular-authoritarian vote that can be explained by socioeconomic variables. This would involve the testing of

ŝ,

models in which the variables presented here share prominence with other variables, particularly party organization and the differential impact of the often-mentioned caudillo ethic. Unlike many socioeconomic variables, most of these variables can only be approached with reasonable confidence through the use of survey data. Fortunately, these data are slowly increasing in quality, quantity, and availability.34 Just as we have progressively refined our knowledge of the socioeconomic determinants of the popular-authoritarian vote since Germani's 1955 analysis of Peronist electoral behavior, so we might now begin to assess the relative worth of that knowledge in a broader universe of independent variables.

Appendix A: The Source and Nature of Peronist Electoral Data

The electoral data presented here are for all 209 circuitos, or precincts, in the Federal Capital (Buenos Aires) between 1942 and 1973. They were obtained from the Secretaria Electoral de la Capital Federal. With the exception of the data for 1957 and 1973, all of the electoral data from the Federal Capital were calculated by adding the vote of every party for every mesa of every circuito in eight elections between 1942 and 1973. The number of parties varied from 2 to 26, and the number of mesas from 3500 to more than 7000. Electoral personnel aggregated the 1957 and 1973 data to the circuito level.

The 39 units of Greater Buenos Aires comprise the second data set used in this article. They are the sum of the Federal Capital's 20 secciones, or wards, and the 19 partidos, or counties, which together with the River Plate surround the Federal Capital. (Two terms are employed to indicate the Argentine equivalent of a U.S. county. The province of Buenos Aires alone retains a semblance of Hispanic purity by the use of partido; elsewhere in this land of Francophiles only departamento is officially acceptable. The Federal Capital's secciones are commonly referred to as circunscripciones.) While interesting because of the large number of variables available for analysis, the units themselves vary considerably in size and are much larger than the Federal Capital's precincts.

³³Alex Inkeles, "Participant Citizenship in Six Developing Countries," American Political Science Review, 63 (December 1969), 1137; Alex Inkeles and David H. Smith, Becoming Modern: Individual Change in Six Developing Countries (Cambridge, Mass.: Harvard University Press, 1974).

³⁴Frederick C. Turner, "The Study of Argentine Politics Through Survey Research," Latin American Research Review, 10 (Summer 1975), 90-100.

Each of the eight elections was unique. The 1946 presidential election is studied in the data sets from both the Federal Capital and Greater Buenos Aires. By the time of the 1951 presidential election, however, Argentina had temporarily switched to single-member districts unrelated to county boundaries, and so only votes in the Federal Capital's unchanged precincts are available for analysis. As a surrogate, the 1954 congressional elections are utilized for Greater Buenos Aires. Following the 1955 Revolución Liberatadora, isolation of the Peronist vote becomes impossible at times. For this reason the 1958 presidential election (in which the Peronists were ordered to vote for the UCRI candidate, Arturo Frondizi) and the 1963 presidential election (in which many Peronists joined frondizistas and other small groups in casting blank ballots) are excluded. All blank ballots cast in the 1957 special election for delegates to a constitutional convention are considered Peronist, as are the blank ballots cast in the 1960 election for members of the Chamber of Deputies. While it is widely believed that many Peronists broke party discipline and voted for one candidate or another in both elections, and while a certain portion of the non-Peronist electorate also cast blank ballots (particularly the Communists in 1957), the overwhelming majority of blank ballots in 1957 and 1960 was Peronist. In elections for representatives to the Chamber of Deputies in 1962 and 1965, Peronists sought office under the banner of the Unión Popular. In 1962 the picture is complicated by the facts that the very small Partido Socialista Argentino de Vanguardia supported Peronist candidates in the Federal Capital and that the neo-Peronist Partido Tres Banderas dropped out of the contest in the Federal Capital two days prior to the election but after its ballots had become available. In 1965 two minor parties (the Alianza de la Justicia Social and the Movimiento Las Flores-Luján) supported candidates of the Peronist Union Popular. Finally, the September 1973 election was the one in which Juan Perón once more captured the Argentine presidency.

Outside Greater Buenos Aires data are from the 1946 presidential election, the 1954 congressional election, the 1957 constitutional Peronism's electoral strength in 1962: Union Popular (Federal Capital and Buenos Aires province), Partido Populista (Catamarca, Corrientes, Chubut, Santa Cruz), Partido Laborista (Córdoba, Jujuy, La Pampa, Santa Fe, Tucumán), Movimiento Cívico Bandera Popular (Chaco), Partido de la Provincia de Chubut (Chubut), Partido Tres Banderas (Entre Rios, Mendoza, Santa Fe, Santiago del Estero), Partido Blanco (Mendoza, Rio Negro), Partido Justicialista (Misiones), Movimiento Popular Neuguino (Neuquen), Partido Laborista Nacional (Salta). Peronist votes were indistinguishable from those of other parties with whom alliances were formed in Formosa, La Rioja (both with the Partido Democrata Cristiano), San Juan (UCR Bloquista), and San Luis (Partido Demócrata Liberal). Five provinces held their elections before the March 18 general election date: Catamarca, Santa Fe, and San Luis (all on December 17, 1961), Formosa (January 14, 1962), and La Rioja (February 25, 1962).

The 1965 Peronist parties are the following: Union Popular (Federal Capital, Buenos Aires. Catamarca, Córdoba, Corrientes, Chaco, Chubut, Entre Ríos, Formosa, La Pampa, La Rioja, Mendoza, Misiones, Salta, San Juan, San Luis, Santa Cruz, Santa Fe, Santiago del Estero, Tucumán), Partido Tres Banderas (Entre Riós, Santiago del Estero), Movimiento Popular Mendocino (Mendoza), Movimiento Popular Neuquino (Neuquén), Partido Justicialista (Salta, Mendoza), Partido Laborista (Salta), Partido Blanco (Río Negro, Salta, Santa Fe), Acción Provinciana (Tucumán), Partido de la Justicia Social (Córdoba, Santa Fe, Tucumán), Alianza de la Justicia Social (Buenos Aires), Movimiento Las Flores-Luján (Buenos Aires), Unión Provincial (Misiones), Movimiento de la Justicia Social (Misiones). There were no elections in Jujuy. In 1963 the newly elected members of the Chamber of Deputies drew lots to determine which half would have two-year and which half would have normal four-year terms in office. Five provinces (Catamarca, Formosa, La Rioja, Misiones, and Salta) had deputies who all drew four-year terms, and so their 1965 election results are for provincial deputies.

A composite dependent variable PERON is introduced in Table 4 and utilized thereafter to

products of each variable's standardized score and its factor score coefficient (see Table A1). Table A2 contains the simple Pearson correlations between PERON and the separate elections' Peronist vote in the Federal Capital and Greater Buenos Aires.

Appendix B: The Sources and Nature of the Socioeconomic Variables

The independent socioeconomic variables have also been separated into two distinct data sets, the Federal Capital's 209 precincts and Greater Buenos Aires' 39 wards and counties. Additionally, on two separate occasions reference is made to the influence of two selected independent variables—the level of industrialization and the rate of industrial growth—upon the Peronist vote in the Argentine counties outside Greater Buenos Aires.

The precinct-level data from the Federal Capital refer only to social class as measured by occupation. I believe they are the most reliable data available for use in the ecological analysis of Argentine electoral behavior. These data were obtained from a ten percent random-start, fixed-interval sample of registration forms for all male voters as of October 1959. Then, using the same precinct tables (mesas) sampled in

1959, a further ten percent sample was made of the 1945 voter registration lists (that is, ten percent of the 1959 ten percent) and very few changes in the occupational structure of the Federal Capital's precincts were found to have occurred during the intervening 14 years. Working-class areas, in particular, maintained an extremely stable class structure. Martinez Estrada noted long ago that the class ecology of Buenos Aires had adopted its fundamental outlines by the turn of the century. 35

In Argentina male voter registration is tied to registration for military service, and a very severe sanction (up to an extra year of military duty) is imposed upon those who fail to register within three months of their eighteenth birthday. Because they exclude so few males, the registration lists provide an unusually accurate estimate of a precinct's occupational structure.³⁶ It is impossible to obtain a stratified female sample. More than 50 percent of registered females are classified as housewives or unemployed, and a large majority of the

35 Ezequiel Martinez Estrada, Radiografía de la pampa, Vol. 2, 3rd ed. (Buenos Aires: Editorial Losada, 1946), p. 30.

³⁶Not everyone shares this evaluation of the data's reliability. See Germani, "El surgimiento del peronismo," p. 437–38.

Table A1. Factor Score Coefficients and Component Variables of PERON

Component Variable	Factor Score	Mean	Standard Deviation
% vote for Peronism, 1946	.14	55.8	8.27
% vote for Peronism, 1954	.15	59.2	8.64
% vote for Peronism, 1957	.15	22.1	6.97
% vote for Peronism, 1960	.15	25.9	7.76
% vote for Peronism, 1962	.15	32.3	9.94
% vote for Peronism, 1965	.15	38.6	8.77
% vote for Peronism, 1973	.15	58.4	13.22

Table A2. Pearson Correlations between PERON and Its Component Variables

,		PI	ERON
Election	Election	Greater Buenos Aires	Federal Capital
1946	<u> </u>	.89	.90
1951	(1954)	.97	.95
1957	•	.97	.95
1960		.96	.96
1962		.99	.92
1965		.98	. 97 .
1973		.96	.93
N=		37	209

remainder apparently is engaged in some aspect of dressmaking. This activity is subdivided into as many as 30 processes, each claiming a rather ambiguous rank on a nearly incoherent occupational pecking order.

With the assistance of more than a dozen employees of the Secretaria Electoral and several Argentine and United States social scientists living in Buenos Aires, six occupational categories were rank-ordered according to contemporary Argentine standards of social stratification. Of highest status is the "professional" category-attorneys, doctors, engineers, owners or managers of large enterprises, persons supported by their investments, etc. Argentine professionals appear to occupy the same relative social position as the United States' upper middle class. No attempt was made to identify the upper class; presumably its members are included in the data as professionals. Next in social status are students. Given the orientations of Argentina's universities, students who pursue their studies until graduation become professionals. For this reason it is not surprising that the ecological correlation between percent professionals and percent students is +.65. Businessmen are third on the scale. This is an extremely vague category for all types of middle-class men who are self-employed in the broadest possible sense. Readers familiar with the varied use of comerciante will appreciate the imprecision of this category. Fortunately, there are relatively few self-declared businessmen in Buenos Aires; in no precinct are they more than nine percent of the registered male population and the mean is slightly less than five percent. "Employees" are white-collar workers, "workers" are blue-collar workers, and "unskilled workers" are peones.

Treatment of the independent variables in the second data set, that of Greater Buenos Aires, proved infinitely more difficult. The fundamental problem is multicollinearity: several of the conceptually distinct variables tend to vary together. This is only natural, or course, for economic change can hardly occur without demographic instability or industrialization. Nor are large numbers of urban working-class citizens able to cluster together in zones of temporary huts with the same population density of the zones of middle and upper-class high-rise apartment complexes, for flat terrain and relatively efficient mass transportation encourage extensive rather than intensive land use by the Buenos Aires working class.

As expected, therefore, an initial factor analysis of the 26 variables employed in this study yielded five rather unclear orthogonal factors (see Table B1). Because the factors

appeared so difficult to interpret and, more importantly, because this was to be a test of hypotheses from a variety of previous analyses, the factors in Table B1 were bypassed and the variables were separated into six groups (WORKING CLASS, POPULATION GROWTH, URBANIZATION, SATISFACTION, INDUS-TRY, and INDUSTRIAL GROWTH) according to prior usage. When prior usage proved inconsistent among authors, decisions were made by appealing to political science's ever-ambiguous court of last resort, common sense. No reader will agree with all of the groupings, although some (particularly WORKING CLASS, POPU-LATION GROWTH, and INDUSTRIAL GROWTH) will be far less controversial than others.

Factor analyses then explored the relationships among the composite groups. Results included verification of the differences that underlie the separation of POPULATION GROWTH and URBANIZATION (see Table B2). Internally, the six groups were considered relatively homogeneous when principal factoring without iteration produced only one factor with an eigenvalue greater than unity. Then the six composite variables were constructed in the same manner as PERON, by summing the products of each variable's standardized score and its factor score coefficient.

At this point the problem of multicollinearity among WORKING CLASS, POPULATION GROWTH, URBANIZATION, and INDUS-TRIAL GROWTH became acute. The zeroorder Pearson correlation between WORKING CLASS and POPULATION GROWTH was +.71: between WORKING CLASS and URBAN-IZATION it was -.79; and between POPULA-TION GROWTH and INDUSTRIAL GROWTH it was +.79. These correlations would make several partial correlations with PERON all but meaningless, and so factor analysis was employed once more, this time using only the 13 variables grouped into WORKING CLASS, POPULATION GROWTH, URBANIZATION, and INDUSTRIAL GROWTH. The results of an orthogonal analysis were disappointing, but an oblique (oblimin) rotation produced three distinct factors that could be clearly labeled WORKING CLASS, POPULATION GROWTH, and URBANIZATION. The four INDUSTRIAL GROWTH variables loaded randomly and were eliminated from further factoring. The nine WORKING CLASS, POPULATION GROWTH, and URBANIZATION variables were factored once more with an oblique rotation, and the results are presented in Table B3. Then the composite variables WORKING CLASS, POPU-LATION GROWTH, and URBANIZATION

were re-computed using the newly generated factor score coefficients. By this method the original WORKING CLASS-POPULATION GROWTH correlation of +.71 was reduced to +.35, the WORKING CLASS-URBANIZATION

correlation of -.79 to -.16, and the URBANI-ZATION-POPULATION GROWTH correlation of -.38 to -.30. The redefinition of POPULA-TION GROWTH led to an increase in the POPULATION GROWTH-INDUSTRIAL

Table B1. Orthogonally (Varimax) Rotated Factor Matrix^a for All Independent Variables

Variables	Factor 1	Factor 2	Factor 3	Factor 4	Factor 5
1. % Industrial workers, 1947 ^c	(.58) ^b	.37	(54)	.03	.00
2. % Industrial workers, 1960	.17	(.78)	13	.48	.00
3. % Illiterates, 1960	.17	.47	32	(.63)	.13
4. % Population growth, 1914–1947	08	(.87)	19	.11	23
5. % Population growth, 1947–1960	15	(.66)	.01	(.61)	.00
6. Population density increase, 1914-1947	09	(.87)	19	.12	23
7. Population density increase, 1947–1960	.06	(.72)	35	.33	11
8. % Urban population, 1947	.20	13	03	(90)	04
9. % Urban population, 1960	.19	01	.06	(86)	01
10. Wages per industrial employee, 1935	07	13	(.88)	16	.16
11. Wages per industrial employee, 1941	.06	(51)	(.82)	02	12
12. Wages per industrial employee, 1947	.12	28	(.89)	.04	.08
13. Workers per industrial establishment, 1935	(.81)	09	09	14	.32
14. Workers per industrial establishment, 1941	(.92)	04	23	10	01
15. Workers per industrial establishment, 1947	(.92)	.06	18	04	.22
16. Workers per industrial establishment, 1963	(.63)	20	.08	.04	(.63)
17. Wages per industrial establishment, 1935	(.73)	15	.39	20	.30
18. Wages per industrial establishment, 1941	(.90)	24	.18	12	04
19. Wages per industrial establishment, 1947	(.93)	06	.17	04	.22
20. Wages per industrial establishment, 1963	.46	06	.20	.03	(.77)
21. Value of production per industrial establishment, 1947	(.97)	01	.03	06	.12
22. Value of production per industrial establishment, 1963	(.50)	.02	03	.15	(.73)
23. Increase in industrial establishments, 1914–1947	10	(.89)	16	29	07
24. Increase in industrial establishments, 1947–1963	04	(.80)	25	.36	.16
25. Increase in industrial workers, 1914–1947	16	(.90)	15	11	.11
26. Increase in industrial workers, 1947-1963	24	(.68)	02	.46	.37
Eigenvalues	6.96	6.85	3.31	3.27	2.19
% Total variation	26.77	26.37	12.75	12.56	8.43
% Common variation	31.06	30.59	14.79	14.57	9.78

^aPrincipal factoring without iteration using as the extraction criterion factors with eigenvalues greater than

Table B2. Orthogonally (Varimax) Rotated Factor Matrix^a for Demographic Variables

Variables	Factor 1	Factor 2
1. Average annual percent population growth, 1914-1947	(.97) ^b	08
2. Average annual percent population growth, 1947–1960	(.59)	(66)
3. Average annual change in population density, 1914–1947	(.97)	08
4. Average annual change in population density, 1947–1960	(.84)	29
5. Urban population as a percent of total population, 1947	17	(.90)
6. Urban population as a percent of total population, 1960	03	(.93)
Eigenvalues	2.96	2.21
% Total variation	49.40	36.78
% Common variation	57.11	42.52

^aPrincipal factoring without iteration using as the extraction criterion factors with eigenvalues greater than unity.

bFactor loadings ≥ .50 shown in parentheses.

bFactor loadings ≥ .50 shown in parentheses.

^cSee Tables B4-B9 for complete variable label.

GROWTH correlation of +.79 to +.84. The independent variable URBANIZATION was subsequently eliminated from the analysis due to insufficient variation.

The five composite independent variables were constructed as indicated in Tables B4 through B8. Because each of the 26 variables is standardized, the factor score coefficients de-

termine the relative impact of each component variable upon the composite variable.

Table B9 contains the first and second-order partial correlations between PERON and each of the five composite independent variables. Table B10 is an independent composite variable correlation matrix.

Table B3. Oblique Primary Factors of Nine Socioeconomic Variables^a

	Pattern			Structure		
Variable	Factor 1	Factor 2	Factor 3	Factor 1	Factor 2	Factor 3
1. % Industrial workers, 1947 ^b	.06	.22	(.95)	.33	.06	(.93)
2. % Industrial workers, 1960	(.52)	36	.43	(.77)	(57)	(.67)
3. % Illiterates, 1960	.10	(51)	(.67)	.48	(64)	(.78)
4. % Population growth, 1914–1947	(.99)	.08	.08	(.98)	.21	.34
5. % Population growth, 1947–1960	(.51)	(60)	.00	(.69)	(75)	.28
5. Population density increase, 1914–1947	(.99)	.07	.00	(.97)	22	.34
7. Population density increase, 1947–1960	(.73)	16	.21	(.85)	41	.49
8. % Urban population, 1947	10	(.91)	.13	32	(.92)	05
9. % Urban population, 1960	.13	(.92)	04	15	(.89)	14
	(Popula- tion Growth)	(Urbani- zation)	(Working Class)	(Popula- tion Growth)	(Urbani- zation)	(Working Class)

²Oblimin (δ = -.5) solution. Initial criterion value 5.65, final criterion value 3.64. Loadings and coefficients \geq .50 shown in parentheses.

Table B4. Factor Score Coefficients and Component Variables of WORKING CLASS

Component Variable	Factor Score		
1. Industrial workers as a percent of economically active population, 1947	.630		
2. Industrial workers as a percent of economically active population, 1960	.225		
3. Illiterates as a percent of total population, 1960	.410		
4. Average annual percent population growth, 1914-1947	087		
5. Average annual percent population growth, 1947–1960	078		
6. Average annual change in population density, 1914–1947	087		
7. Average annual change in population density, 1947–1960	.065		

Table B5. Factor Score Coefficients and Component Variables of POPULATION GROWTH

Component Variable	Factor Score
1. Industrial workers as a percent of economically active population, 1947	049
2. Industrial workers as a percent of economically active population, 1960	.106
3. Illiterates as a percent of total population, 1960	068
4. Average annual percent population growth, 1914–1947	.352
5. Average annual percent population growth, 1947–1960	.153
6. Average annual change in population density, 1914–1947	.351
7. Average annual change in population density, 1947–1960	.223

^bSee Table B4 for complete variable label.

Table B6. Factor Score Coefficients and Component Variables of SATISFACTION

Component Variable			Factor Score
Wages per industrial employee, 1935 Wages per industrial employee, 1941			.358 .370
3. Wages per industrial employee, 1947	•.		370

Table B7. Factor Score Coefficients and Component Variables of INDUSTRIAL GROWTH

Component Variable	
	.273
-	.296
	.299
	.270

Table B8. Factor Score Coefficients and Component Variables of INDUSTRY

Component Variable	Factor Score
1. Blue-collar workers per industrial establishment, 1935	.120
2. Blue-collar workers per industrial establishment, 1941	.124
3. Blue-collar workers per industrial establishment, 1947	.130
4. Blue-collar workers per industrial establishment, 1963	.129
5. Wages per industrial establishment, 1935	.119
6. Wages per industrial establishment, 1941	.128
7. Wages per industrial establishment, 1947	.134
8. Wages per industrial establishment, 1963	.107
9. Value of production per industrial establishment, 1947	.129
10. Value of production per industrial establishment, 1963	.097

Table B9. Partial Correlations between PERON and the Composite Independent Variables

	Working Class	Popula- tion Growth	Satisfac- tion	Industry	Industrial Growth
Zero order:			,		
PERON	.77	.69	47	.15	.79
First order controlling for:				•	
WORKING CLASS		.70	11	34	.79
POPULATION GROWTH	.7 7		23	.41	.54
SATISFACTION	.69	.60		.21	.72
INDUSTRY	.79	.74	49		.84
INDUSTRIAL GROWTH	.77	.08	15	.48	
Second order controlling for:					
WORKING CLASS, POPULATION GROWTH			.21	06	.53
WORKING CLASS, SATISFACTION	•	.71		33	.81
WORKING CLASS, INDUSTRY		.65	.05		.76
WORKING CLASS, INDUSTRIAL GROWTH		.16	.29	.05	
POPULATION GROWTH, SATISFACTION	.77			41	.51
POPULATION GROWTH, INDUSTRY	.72		25		.60
POPULATION GROWTH, INDUSTRIAL GROWTH	.77	•	- 14	.49	
SATISFACTION, INDUSTRY	.71	.67			.79
SATISFACTION, INDUSTRIAL GROWTH	.78	.07		.49	
INDUSTRY, INDUSTRIAL GROWTH	.68	.15	17		* *

Table B10. Independent Composite Variable Product-Moment Correlation Matrix

	Working Class	Population Growth	Satisfaction	Industry	Industrial Growth
WORKING CLASS POPULATION GROWTH SATISFACTION INDUSTRY INDUSTRIAL GROWTH	1.00	.35 1.00	54 47 1.00	.45 21 .08 1.00	.43 .84 50 18 1.00

Spatial Voting Models for the French Fifth Republic*

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In this study formal spatial models are applied to cross-sectional analysis of district results on the second ballot of French legislative elections. A model of probabilistic spatial voting better accounts for the data than either standard "ecological" models or a model of deterministic spatial voting. There are three substantive findings concerning voter behavior. First, the adjustment of voters to external information can be largely viewed as a shift in the spatial (Left-Right) distribution of voters. This shift, plus decisions by parties and candidates as to which districts parties will contest, determines the first ballot outcome. In arriving at second ballot choices, voters then appear to utilize decision rules that have a substantial degree of temporal stability. A second and related finding is that the second ballot can be reasonably accounted for by a single Left-Right dimension. Third, in those districts with three or more candidates on the second ballot, there may be substantial strategic voting with voters switching from candidates close to their ideal points but unlikely to win to more distant candidates who are more likely to win. The existence of strategic voting is suggested by the finding that models based solely on spatial preferences perform well for two-candidate districts, but less well for three- or four-candidate districts.

The legislative elections of the French Fifth Republic provide an advantageous setting for empirical tests of formal spatial models of voter behavior. France utilizes a two ballot general election. In districts where no candidate obtains a majority on the first ballot, a second ballot is held one week later. Any candidate receiving more than ten percent of the first ballot vote is eligible for the second ballot (rather than just the top two candidates as in runoff primaries in the United States).1 Eligible candidates may elect not to run on the second ballot. When they do drop out, they typically endorse one of the remaining candidates. We report three substantive findings concerning voter behavior in the French electoral process.

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¹The second ballot eligibility percentage was 5 percent in the 1958 and 1962 elections.

²Party positions are viewed as fixed over time. It should be recognized, however, that a shift of voters is, to a large extent, observationally equivalent to a shift in party positions.

The first finding is that the adjustment of voters to external information can be largely viewed as a shift in the spatial (Left-Right) distribution of voters.² This shift, plus decisions by parties and candidates as to which districts parties will contest, determines the first ballot outcome. In arriving at second ballot choices, voters then appear to utilize decision rules that have a substantial degree of temporal stability. The external information is largely incorporated in the first ballot shift and need not be considered in a model of the second ballot. To a "reasonable" degree of accuracy, the second ballot can be forecast on the basis of first ballot results and party decisions as to which eligible candidates will in fact contest the second ballot.3 For example, voters appear to have responded to the student-worker demonstrations of May 1968 with a shift to the Right. Once voters located their ideal points on the Left-Right continuum for 1968, however, their second ballot behavior in the June election was close to identical to that of voters with the same ideal points in the more serene circumstances of 1967.

A second and related finding is that the second ballot can be reasonably accounted for by a single Left-Right dimension. We do not

³Candidates eliminated from the second ballot may, and usually do, counsel their first ballot supporters about which candidates to support on the second ballot. A plurality wins the second ballot.

deny that a variety of foreign and domestic issues are avidly discussed in the campaign. Thus there are many dimensions. If we are able to use just one, it is partly because each of the other issues is, in varying degree, correlated with the underlying Left-Right conflict. Further, most specific issues, such as Middle East policy or regional decentralization will be salient only to small minorities of voters. Consequently, most specific issues can be ignored when considering district-level results. One general dimension is adequate. Multidimensional models might improve our results, but such improvements should be minor.

Third, we find, in those districts with three or more candidates on the second ballot, that French voters may engage in significant strategic voting, switching from less viable candidates close to their ideal point to more viable candidates. Gaullist supporters, for example, might switch to a Centrist to insure defeat of a Socialist. Specifically, strategic voting is suggested by the finding that our spatial, preference-based models perform well for two-candidate districts, but less well for three- or four-candidate districts.

In presenting formal models of the second ballot process, we rely on methods developed in an earlier article for approximating candidate locations and voter preference distributions and for modeling abstention and spoiled (blank) ballot behavior. That research covered the first

⁴That is, voters must consider not only their preference regarding a candidate but also that candidate's chances of winning the election. For discussions on viability effects, see Richard D. McKelvey and Peter C. Ordeshook, "A General Theory of the Calculus of Voting," in *Mathematical Applications in Political Science*, VI, ed. James F. Hernden and Joseph C. Bernd (Charlottesville, Va.: University of Virginia Press, 1972), pp. 32–78, and the debate between Gerald Kramer and Thomas Casstevens, in "Communications," *American Political Science Review*, 62 (September 1968), 955–56, and "Communications," *American Political Science Review*, 65 (March 1971), 187–89, concerning Casstevens' article, "A Theorem about Voting," *American Political Science Review*, 62 (March 1968), 205–07.

⁵Viability will not affect choice in two-candidate

⁵Viability will not affect choice in two-candidate races if voter utility is associated solely with "winlose" outcomes. In other contexts, where the margin of victory is important to the voter, viability effects may also be present in two-candidate races. See William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," American Political Science Review, 62 (March 1968), 25–42. Much more direct findings concerning strategic voting are reported in Richard P. Castanias II, Timothy W. McGuire, and Howard Rosenthal, "The French Voter as Rational Man" (Paper prepared for the Third World Congress of the Econometric Society, Toronto, Canada, August 20–25, 1975).

four elections of the Fifth Republic, those of 1958, 1962, 1967, and 1968. For the same elections, we now develop and estimate models to analyze the *vote proportions* received by various political parties on the second ballot. Estimation is for actual voting data at the legislative district level. As a postscript, we replicate and validate our preferred model, the "all-candidates" model, with data from the 1973 elections. This data became available subsequent to the initial writing and presentation of this paper.

We will first discuss several features of the French political system which facilitate the development of formal voting models and then present the set of criteria used to evaluate the models in this paper. Subsequently, we describe the models in detail and present empirical results for each model. There are two models motivated by spatial theory-the "a-priori" and the "all-candidates" models. In both models, voters vote probabilistically. Voters have a higher probability of voting for a close candidate than for a distant one. The models differ as much in the technical aspects of functional forms as in substantive spirit. The main substantive difference is that the "all-candidates" model permits voter decision rules to vary with spatial location. Thus, leftists need not have the same parameters as rightists. In contrast, for the a-priori models, all voters have the same decision rules; their only differences are in ideal points. Performance of these models is compared to that of two null models-a heuristic spatial model, the "closest-candidate" model, and the standard nonspatial "ecological" (constant proportions) model. Following a tactic employed in our nonvoting article, the earlier elections of 1958 and 1962 are used for model development, evaluation, and experimentation with various functional forms. On the basis of this analysis, we concluded in favor of the all-candidates model. Only this model was analyzed for the three latest elections.

Features of the French Political System That Facilitate Model Construction

The existence of the two ballot system is very helpful in model construction since the first ballot voting patterns provide important

⁶Howard Rosenthal and Subrata Sen, "Electoral Participation in the French Fifth Republic," *American Political Science Review*, 67 (March 1973), 29-54.

⁷Our analysis applies to all metropolitan districts with second ballots, except for Corsica where vote fraud has been common.

information that can be incorporated in models of voter behavior on the second ballot. We start by assuming that voter preferences are revealed on the first ballot. In other words, an individual voting for the Communist candidate (for example) on the first ballot is assumed to prefer the Communist candidate relative to the other candidates on the first ballot.⁸

As only one week separates the two ballots, it is also reasonably safe to assume that in this week preferences are not substantially modified by the host of exogenous variables that affect preferences and, hence, voting behavior. Consequently, we can examine the short-term essence of an electoral process: how preferences are

⁸Since voting is a function of both preference and candidate viability, some strategic voting undoubtedly occurs on the first ballot. However, casual empiricism suggests that strategic voting occurs much less often on the first ballot than on the second, decisive ballot. In any event, our models assume that voters always vote for their most preferred candidate on the first ballot.

Our claim here that voters reveal information about their true preferences on the first ballot is to some extent inconsistent with our view, expressed later, that spatially oriented voters vote probabilistically on the second ballot. A more accurate, if perhaps less intuitively appealing, statement of our overall approach is that we assume voters vote probabilistically on the second ballot, the probabilities being functions of the spatial position of the expressed first ballot choice of the voter and the spatial positions of the second ballot candidates.

translated into a decision in the choice situation occasioned by a specific context.⁹

The French multiparty system with its emphasis on a unidimensional Left-Right conflict is crucial to the analysis. Relatively simple survey items can be used to locate the parties on the Left-Right dimension. This article uses the Barnes and Pierce "all" measurements shown in Table 1. And, since mass media and other aspects of modernization have led the legislative elections to turn primarily on national rather than local issues, we can, without too much error, regard candidates as undifferentiated representatives of their national parties, having a spatial location equal to that of the national party.

Evidence that candidates are not strongly differentiated intraparty is available from the Guttman scales developed by David Wood for the last legislature of the Fourth Republic and the first legislature of the Fifth. On Wood's major scales, most deputies seeking reelection had the median scale position of their parties

⁹That is, we deliberately ignore political socialization, campaigns, etc., as determinants of voting behavior.

Table 1. Spatial Positions of French Party Groups and Percentage of Incumbents at Party Median Position on Wood's Major Scales

			Incumbents a	Party Median	1
	Barnes and Pierce	19	58	1962	62
Party	Spatial Position ^a	%	Np	%	. <i>N</i>
Communists	0.000	100	127	100	10
PSU	1.230	36	11	_	_
Socialists, 1958 and 1962	1.557	100	66	92	38
Federation of the Left, 1967 and 1968	1.148		_	_	_
Radicals, 1958 and 1962	2.049	65	26	3	35
MRP, Centre Démocrate, PDM, Mouve- ment pour la Réforme, Technique et					
Démocratie	3.607	98	53	50	48
UNR, C UDR, Gaullists	4.670	27	11	76	190
Independents, Independent Republicans,	С				
Right	4.180	13	76	11	81
Extreme Right, Alliance Républicaine ^c	5.000	73	22	60	5
Total		75	392	56	407

^aAll measures are normalized so that Communists = 0.0 and Extreme Right = 5.0.

¹⁰ See Rosenthal and Sen, pp. 35-36.

¹¹Samuel H. Barnes and Roy Pierce, "Public Opinion and Political Preferences in France and Italy," *Midwest Journal of Political Science*, 15 (November 1971), 643-60.

 $b_N = number of incumbents.$

^cLabels used by Barnes and Pierce.

(see Table 1).¹² (These scales contained 52 ordinal classes for 1958 incumbents and 37 for 1962; hence, a single scale position is a highly specific location.) Even though the evidence pertains only to the roll-call behavior of incumbents, it does suggest that little is lost by assigning candidates the spatial position attributed to their party on the basis of survey data.

Given the use of national sample surveys to estimate the perceived spatial positions of the candidates, it is still necessary to locate voters along the Left-Right dimension. Using the preferences revealed by the first ballot, we can approximate the distribution of voters in each district. As indicated in greater detail in our earlier article, the approximation procedure assigns a probability mass to each of the Left-Right locations occupied by parties represented on the first ballot. This mass is equal to the proportion of valid votes received by the party on the first ballot. We assume the approximation's validity not only for voters but also for those who abstained or cast blank ballots on the first ballot.13

The preceding discussion has focused on the substantive features of the political setting that facilitate model building: relative isolation from exogenous variables that affect voter preferences and relatively facile estimation of the spatial positions of candidates and voters. There is also an important statistical feature: the roughly 400 second ballot districts per election present the large number of observations needed for reliable estimation of complex multivariate models.

Design Criteria in Model Construction

Constructing models for voting as well as nonvoting leads to a complex ten-equation

model in which different but overlapping sets of observations apply to each equation. The large number of equations stems from our use of eight party groups in addition to abstentions and blank ballots. The overlapping is a consequence of the fact that each party is present in only a subset of the second ballot districts. Observations for all second ballot districts are present only for the abstention and blank ballot equations; the other equations have fewer observations. This complexity suggests the need for a brief statement of our design criteria for model formulation and evaluation.

Theory. Models whose equations are a consequence of a well-specified theory are to be preferred to other types of models. This criterion establishes a preference for this essay's models compared, for example, to a regression of second ballot votes against the first five factors from a factor analysis of a large number of social and economic indicators.

Parsimony. Although there are many ways to consider the parsimony of a scientific model, we here employ three operational standards: (1) the number of parameters that are estimated from the data; (2) the scope of the equations as indicated by the number of districts postdicted by the model-for example, a model which postdicts all parties in all districts has greater scope than a model which excludes some of the data; (3) the number of equations -for example, a model in which a single equation is run for the Communist party has greater parsimony than a model for which there are several Communist equations, the observations having been segregated by number and party affiliation of other candidates.

The "Adding Up" Condition. The probability that registered voters will abstain, the probability that they will cast a blank ballot, and the probabilities with which they vote for the various parties must all sum to 1.0. Theoretical models of individual voter behavior, when viewed across equations, must also have this property. When the theoretical models "add up" at the individual level, the aggregated proportions at the district level will also add to 1.0.

Explanatory Power. Again, although there are a variety of ways in which to evaluate the explanatory power of a model, we employ the conventional criterion of squared error.

¹²David M. Wood, "Majority vs. Opposition in the French National Assembly, 1956–1965: A Guttman Scale Analysis," American Political Science Review, 62 (March 1968), 88–109. In Table 1, the incumbent deputies have been classified by their party affiliation used in the election (as against their parliamentary group in the legislature). Of those seeking reelection in 1958, 75 percent had the median position; in 1962, 56 percent. The only parties with over half the deputies not at the median were, in 1958: the PSU, the UNR, and the Right; in 1962: the Radicals and the Right. Similar results are obtained for the other Wood scales for each legislature.

¹³Rosenthal and Sen, pp. 35-37. The approximation is tolerable because there are typically far fewer candidates on the second ballot than on the first. This implies that the differences of the distances from a voter to the various candidates will be large relative to the measurement error.

Agreement Between Estimated Parameters, Theory, and Substance. Explanatory power is not enough. The signs and magnitudes of the estimated coefficients should agree with those called for by the theory in question. In addition, estimated voting probabilities and proportions should all lie in the interval [0, 1]. Furthermore, estimated proportions should be checked against substantive considerations external to the theoretical model. For example, we would discredit a model which showed 98 percent of the Communist first ballot supporters voting for the Gaullists on the second ballot. 14

Tradeoffs. Judged against the multidimensional criteria we have outlined, none of the models to be presented can be considered "best." Rather, tradeoffs abound, particularly between parsimony and explanatory power. These will be apparent in the empirical results, which follow presentation of the models.

A-Priori Models

Rather than modeling voters as voting with certainty—if they cast a ballot—for the "closest" candidate, we developed models in which the probability of voting for a candidate was a function of candidate distances. ¹⁵ Naturally, the probability of voting for the closest candidate should, ceteris paribus, be greater than the probability of voting for any other candidate.

In addition to creating models of probabilistic choice among competing candidates, we also needed to model, for each constituency, the probability that a voter at a given spatial

14We also prefer models for which an analytical solution leads to global minimization of the loss function, in this case squared error. This leads us to regressions with a proportion as the dependent variable. As Theil points out, such regressions can have undesirable econometric properties. However, the fact that the vote proportion is modeled as a sum of proportions aggregated across spatial locations implies that in this case we cannot apply the attractive alternative models Theil discusses (Henri Theil, "A Multinomial Extension of the Linear Logit Model," International Economic Review, 10 [October 1969], 251–59). We have to fall back on an assumption of approximate linearity over the range of the proportion under investigation and a reliance on the robustness of the estimates given our large number of observations.

15Probabilistic models provide a means of coping with error introduced both in our approximation of the Left-Right distribution and in our use of a single dimension. Discussion of why aggregation problems force the direct expression of probabilities rather than deriving them from an individual utility maximization perspective is contained in Castanias, McGuire, and Rosenthal.

location would cast a valid ballot. The appropriate estimates, available from our abstention and blank ballot models, ¹⁶ allowed us to estimate the spatial distribution of valid ballot casters (as opposed to the distribution of registered voters). We then had only to build a model of voter behavior conditional on the fact that a valid ballot was cast. Our dependent variables are thus proportions of the vote cast (exprimés) rather than proportions of registered voters (inscrits). ¹⁷

Our first set of "conditional" models of voter behavior are termed the "a-priori" models because a specific functional form determines voting probabilities as a function of distances and no parameters are estimated beyond those in the abstention and blank ballot equations. An a-priori model is obviously strong on parsimony, and the functional forms chosen guarantee a reasonable theoretical and substantive interpretation and that "adding up" is satisfied.

In developing the a-priori models, we first need to set forth some notation that will be useful throughout the article. Let:

 $j=1,\ldots,J$ index the districts.

 $i=1,\ldots,I$ index the spatial locations of the voters.

 $k_j = 1, \ldots, N_{2j}$ index the candidates on the second ballot in district j. Where the meaning is clear, the j subscript is dropped.

 D_{ij}^k = the distance from spatial location i to candidate k in district j.

 DIS_{ij}^{t} = the distance from spatial location i to the t-th closest candidate in district j. (No tied distances exist in the data.)

 PV_{ij} = the probability that voters will cast a valid ballot given that they are at location i in district j.

¹⁶Rosenthal and Sen, pp. 40-52.

17We eliminated, using newspaper sources, candidates who had withdrawn from the race but whose names were still on the ballot. The actual dependent variable then becomes the proportion of the vote cast for the "true" candidates. In our earlier paper, we used the heuristic decision rule of eliminating all candidates with less than 1,000 votes on the second ballot. This decision rule eliminated practically all the appropriate candidates for 1958, 1967, and 1968. For 1962, the approximation was somewhat worse. With the 1973 replication, we were forced to rely on the 1,000 vote rule.

 P_{ij} = the proportion of registered voters in district j at spatial location i. This is approximated by the first ballot share of the valid ballots obtained by the party with spatial location i. If a party does not have a candidate on the first ballot, $P_{ij} = 0$. If there is more than one candidate from the same party or tendency, their votes are summed. We are guaranteed $\sum_{i} P_{ij} = 1.0$.

 Q_{ij} = the proportion of voters casting valid second ballots in district j that are located at i. Again, $\sum Q_{ij} = 1.0$.

 $Pr(k)_{ij}$ = the probability that voters at spatial location i vote for candidate (party) k on the second ballot, given that they cast a valid ballot.

 $V(k)_j$ = the proportion of the valid vote in district j for candidate k.

The a-priori models were designed to satisfy the following two axioms:

1. Voters vote for a candidate at their ideal point with a probability of 1.0. Formally, $Pr(k)_{ij} = 1.0$ if $D_{ij}^k = 0$.

2. When a voter's own party is not repre-

2. When a voter's own party is not represented on the ballot, voting probabilities are ordered with distance to the left or the right of the voter. However, it is possible that voters will vote with higher probability for a candidate on their left (right) who is more distant than a candidate on their right (left). Actually, this possibility occurs only for model II (see below). Otherwise, the greater the distance, the lower the probabilities. To state the axiom formally, let x(i), x(a), x(b) represent the spatial positions on the Left-Right continuum of voter i and second ballot candidates a and b. Then if $x(i) \le x(a) < x(b)$ or $x(b) < x(a) \le x(i)$, then $Pr(a)_{ij} > Pr(b)_{ij}$ or $Pr(a)_{ij} = Pr(b)_{ij} = 0$.

We now describe three a-priori models which satisfy these axioms.

Model I. The first model expresses $Pr(k)_{ij}$ for a specific candidate located at a, as follows:

$$Pr(a)_{ij} = \frac{1/D_{ij}^{a}}{\sum_{k=1}^{N_{2j}} 1/D_{ij}^{k}}$$

Note that if a is located at i, (1) $D_{ij}^{a} = 0.0$ and $Pr(a)_{ij} = 1$ in the limit; (2) for all $b \neq a$, $Pr(b)_{ij} = 0$ in the limit. Otherwise, if the party located at i is not on the second ballot, $0 < Pr(k)_{ij} < 1$ for all candidates k on the second ballot. This model is a simple version of the "gravity" models used in marketing studies. ¹⁸

Model II. The second model expresses $Pr(k)_{ij}$ as follows for the case when i is not on the second ballot:

1. If a is the closest candidate to i and all candidates are to the right or all are to the left of i,

$$Pr(a)_{ij}=1.0.$$

2. If i is located between candidates a and b,

$$Pr(a)_{ij} = D_{ij}^{b}/(D_{ij}^{a} + D_{ij}^{b})$$
 and $Pr(b)_{ij} = D_{ij}^{a}/(D_{ij}^{a} + D_{ij}^{b})$. Hence,

$$Pr(a)_{ij} + Pr(b)_{ij} = 1.0.$$

Of course, if the party located at i is on the second ballot, it is voted for with probability 1.0. In this model $Pr(k)_{ij} > 0$ for at most two candidates on the second ballot: the closest candidates to the right and left of location i. For the remaining candidates, $Pr(k)_{ij} = 0$.

Model III. This version states that when the party located at i is not on the second ballot, the probability of voting is split between the two closest candidates, a and b, as follows:

$$Pr(a)_{ij} = D_{ij}^b/(D_{ij}^a + D_{ij}^b)$$
 and
 $Pr(b)_{ij} = D_{ij}^a/(D_{ij}^a + D_{ij}^b) = 1 - Pr(a)_{ij}$.

In contrast to Model II, the probability of voting is split between the two closest candidates even if they are both on the right or left of i. As in Model II, $Pr(k)_{ij} > 0$ for at most two second ballot candidates.

The three models described above are intuitively sensible models of voter behavior that satisfy the "adding up" condition. Though one can propose other models satisfying the basic axioms, we will restrict our analysis to these three models.

Aggregation of Individual Voters. We wish to use $Pr(k)_{ij}$ to determine $V(k)_{j}$, the proportion of valid votes (exprimés) cast on the second

¹⁸David L. Huff, Determination of Intraurban Retail Trade Areas (Los Angeles: University of California, Real Estate Research Program, 1962). The relationship to gravity models was suggested by Richard Staelin.

ballot for candidate k in district j. $V(k)_j$ can be expressed as follows:

$$V(k)_{j} = \sum_{i=1}^{I} Pr(k)_{ij} Q_{ij} + \epsilon_{kj}$$
 (1)

while the predicted proportion can be calculated as:

$$\hat{V}(k)_j = \sum_{i=1}^{I} Pr(k)_{ij} Q_{ij}$$
 (2)

where ϵ_{kj} is a random disturbance and Q_{ij} is the proportion of voters casting valid second ballots in district j that are located at i. Q_{ij} is estimated from the vote data on the first ballot as follows: Let PV_{ij} represent the probability that a voter in location i and district j will vote on the second ballot. Then,

$$\hat{PV}_{ij} = 1.0 - \hat{ABS}_i - \hat{\alpha}_i(BB) - \hat{\gamma}_iDIS_{ij}^1$$

where ABS_j = the estimated proportion of second ballot abstentions in district j;

 $\hat{\alpha}_{j}(BB)$ = the nonspatial component of the blank ballot predictive equation for district j;

 $\hat{\gamma}_i DIS_{ij}^1$ = the spatial component of the blank ballot predictive equation for location i and district j.

 \hat{ABS}_i can be computed from the heuristic abstention model. Similarly, $\hat{\alpha}_j(BB)$ and $\hat{\gamma}_iDIS_{ij}^i$ can be computed from the heuristic-alienation blank ballot model. ¹⁹ Then, weighting by the proportion of all voters located at i and normalizing gives:

$$Q_{ij} = \frac{(P_{ij})(\hat{PV}_{ij})}{\sum\limits_{i'=1}^{\Sigma} (P_{i'j})(\hat{PV}_{i'j})}$$

The Q_{ij} can now be used to compute the predicted proportions, $\hat{V}(k)_j$, using equation (2). These predictions are consistent in that $\sum_{k} \hat{V}(k)_j = 1.0$. They require no parameter

estimation. The predictive accuracy of each a-priori model can be tested by computing the root mean square error (RMSE) for the candidates of each party as follows:

$$RMSE_k = \sqrt{\sum_{j} [\hat{V}(k)_j - V(k)_j]^2/m_k}$$

where m_k = the number of districts where k is on the second ballot.

The reported root mean square errors multiplied by 100 represent percentage prediction errors. We have used RMSE rather than R^2 in part to emphasize that the RMSEs are greater than those found by regressing $V(k)_j$ on $\hat{V}(k)_j$. In other models, where R^2 is applicable, there is, of course, a one-to-one inverse relationship between RMSE and R^2 —that is, low RMSE means high R^2 :

For the a-priori models, RMSE calculations were performed separately for each party. For a given party, the model is limited to the districts in which a candidate of the party ran on the second ballot. Therefore, for the model testing results to be meaningful, we have to restrict ourselves to those parties which have a reasonably large number of second ballot candidates.20 Of the eight principal French "parties," only the Communists, Socialists, and Gaullists satisfied this criterion.²¹ Consequently, our analysis is limited to these three parties only. For each party, Tables 2 and 3 report model tests performed for all districts where the party had a second ballot candidate. Tests, reported in Tables 5 to 10, were also performed for various subsets of these districts. For 1962, the subsets were: (1) districts with two second ballot candidates, including the party's candidate, (2) Communist vs. Gaullist second ballot duels, and (3) Socialist vs. Gaullist second ballot duels. For 1958, the subsets were: (1) three-candidate second ballot districts and (2) Communist vs. Socialist vs. Gaullist second ballot truels. These particular subsets were used because they were the only relevant subsets containing relatively large numbers of second ballot districts. The subsets are used later in the article to provide a point of comparison with the standard ecological model which, as explained below, is typically effective only for district subsets.

It should be noted that a-priori models I and III are equivalent for districts with two candidates on the second ballot. Hence RMSEs for models I and III are identical for the two sets of duels and for the two-candidate district subsets. Also, the Communist RMSE and the Gaullist RMSE are identical for districts involving Communist-Gaullist duels as are the Socialist RMSE

¹⁹Rosenthal and Sen, pp. 44-46, 49-52. The nonlogarithmic models are used here.

²⁰This restriction does not apply to the estimation techniques used for the all-candidates model (described below) in which all second-ballot districts are pooled to estimate simultaneously the parameters of the model which applies to all parties.

²¹The "party" groupings are indicated in Table 1.

and the Gaullist RMSE for Socialist-Gaullist duels (see Tables 8 to 10). Since the analysis is performed for the same pair of parties over the same set of districts and the predicted vote proportions for the two parties must sum to 1.0, the RMSE will be the same whether we predict the vote proportion of one party or the other.

Results for the A-Priori Models. Turning to the overall results (shown in Tables 2 and 3), no single a-priori model clearly dominates the others in terms of explanatory power. As all three models are intuitively reasonable models of voter behavior, a possible reason why no one model emerges as the best is that each model describes the behavior of different subsets of voters. Moreover, in many cases, two or more of the models make identical predictions. Consider, for example, the predictions for the 1962 Communists. Identical predictions were obtained for all three models for 98 (or 46.7 percent) of the 210 districts. Apart from these 98 districts, models I and III yield identical predictions for 19.1 percent of the districts while models II and III show identical predictions for another 24.8 percent of the districts.

The RMSE values are reasonably small for the a-priori models, especially for 1962 where the percentage errors vary between 3.4 and 5.7 percent. In 1958, there are larger errors, reaching 11 percent in the case of the Gaullists. In our earlier analysis of nonvoting, 1958 was also the year in which spatial models performed least well, perhaps in part because parties and voters were both feeling their way in a new constitutional system. Indeed, the results are poorer for the new party, the Gaullist UNR,

than for the older Socialists and Communists. The 1958 Gaullists were an amalgamation running from leftist intellectuals to diehard colonialists. Only 11 of their candidates were incumbents from the Fourth Republic, and our assumption of a unique spatial location may be least appropriate in their case. It would indeed take over a decade and the death of General de Gaulle before the Gaullists would be more precisely defined as a modern conservative party.

With the foregoing proviso for 1958, the a-priori models are reasonably successful, especially considering their parsimony. Nonetheless, the RMSEs are sufficiently large to suggest a gain from a more sophisticated model. We thus turn to the all-candidates model.

The All-Candidates Model

While the a-priori models were successful in a number of respects-the models include all districts, have more explanatory power than the ecological models, and are frequently competitive with the other spatial models-it was felt that a parameterized model, termed the "allcandidates" model, should be investigated. In studying blank ballots, we had found that voters' sensitivity to their distance from the closest candidate, the "alienation" coefficient, varied with spatial location. Similarly, voters' spatial locations could affect the response of their voting probabilities to distance. For example, Communist voters might well be expected to be more disciplined that their counterparts on the Right in voting for close candidates. Therefore, we now describe a model which considers the voter's spatial location and all

Table 2. Root Mean Square Error for the 1958 A-Priori Models: All Districts

T 	Communist	Socialist	Gaullist
Model I	0.0466	0.0619	0.1008
Model II	0.0479	0.0704	0.1075
Model III	0.0484	0.0674	0,1046
Number of districts	385	208	268

Table 3. Root Mean Square Error for the 1962 A-Priori Models: All Districts

	Communist	Socialist	Gaullist	
Model I	0.0344	0.0538	0.0506	
Model II	0.0405	0.0462	0.0572	
Model III	0.0353	0.0528	0.0548	
Number of districts	210	117	327	

second ballot candidates in specifying an individual's probability of voting.

The Model of Individual Voter Behavior. In the all-candidates model, the voting probabilities for an individual located at i are shown in Table 4 for up to four second ballot candidates. The table can be easily extended if the number of second ballot candidates exceeds four. β_i is a model parameter which has to be estimated for each voter location i. The estimated value of β_i ought to have the following properties:

- 1. $\beta_i \geq 0$. If $\beta_i = 0$, individuals vote for their closest candidate with a probability of 1.0. As β_i increases in magnitude, the individual's voting probability is dispersed more among the other candidates. Therefore, β_i can be interpreted as a propensity for voting for the closest candidate. It should be noted that it is possible for $(1 \beta_i DIS_{ij}^1)$ to become negative for sufficiently large values of $\beta_i DIS_{ij}^1$. This linear functional form is chosen only because it simplifies estimation. However, we will want to verify that the estimated values of β_i are such that $(1 \beta_i DIS_{ij}^1)$ is positive for all actual values of DIS_{ij}^1 in our data.
- 2. The probability of voting for the closest candidate must be greater than the probability of voting for the second closest candidate. (The model formulation guarantees that the probability of voting for the second closest is greater than that for the third, etc.) Consequently, for all districts in which $P_{ij} > 0$, $(1 \beta_i DIS_{ij}^1)$ must be greater than the appropriate expression for the probability of voting for the second closest candidate.

Finally, it should be noted that the functional forms used in the all-candidates model yield $\sum Pr(k)_{ij} = 1.0$ for any number of candidates and satisfy the axioms listed in the section describing the a-priori models.

The all-candidates model turns out to be the best compromise between parsimony and explanatory power. Furthermore, the theory is demanding not only as to the signs but also as to the magnitudes of the estimated coefficients. Consequently, we have several checks on the desirability of this parameterized model over the a-priori models.

Aggregation to Regression Models. Since the available data consist of aggregate data for election districts, individual voter behavior must be aggregated over all voters to permit analysis using multiple regression. The aggregation is performed by using the Q_{ij} as indicated in equation (1). The all-candidates model leads to a regression without an estimated intercept but with a fixed, nonunit column. Consider, for example, a district with a Communist-Gaullist second ballot duel with only the Communists (denoted by the letter C), Socialists (denoted by S) and the Gaullists (denoted by G) present on the first ballot. The Barnes and Pierce scale indicates that the Socialists are closer to the Communists than to the Gaullists. Hence, using equation (1) and the vote probability relationships in Table 4, the aggregated equations for the Gaullists and the Communists are as follows:

Table 4. Specification of the Individual's Voting Probabilities for the All-Candidates Model

Probability of Voting	Number of Candidates on the Second Ballot				
for the:	2	3	. 4		
Closest candidate	$1 - \beta_i DIS_{ij}^1$	$1 - \beta_i DIS_{ij}^1$	$1 - \beta_i DIS_{ij}^1$		
Second- closest candidate	$\beta_i DIS_{ij}^1$	$\beta_i DIS_{ij}^1 \left[0.5 + \frac{DIS_{ij}^1}{DIS_{ij}^1 + DIS_{ij}^2} \right]$	$\beta_i DIS_{ij}^1 \left[0.5 + \frac{DIS_{ij}^1}{DIS_{ij}^1 + DIS_{ij}^2} \right]$		
Chird- closest candidate		$\beta_{i} DIS_{ij}^{1} \left[0.5 - \frac{DIS_{ij}^{1}}{DIS_{ij}^{1} + DIS_{ij}^{2}} \right]$	$\beta_{i} DIS_{ij}^{1} \left[0.5 - \frac{DIS_{ij}^{1}}{DIS_{ij}^{1} + DIS_{ij}^{2}} \right] \left[0.5 + \frac{DIS_{ij}^{2}}{DIS_{ij}^{2} + DIS_{ij}^{3}} \right]$		
Fourth- closest candidate			$\beta_{i} DIS_{ij}^{1} \left[0.5 - \frac{DIS_{ij}^{1}}{DIS_{ij}^{1} + DIS_{ij}^{2}} \right] \left[0.5 - \frac{DIS_{ij}^{2}}{DIS_{ij}^{2} + DIS_{ij}^{3}} \right]$		

$$\begin{split} V(G)_{j} &= (0)Q_{Cj} + (\beta_{S}DIS_{5j}^{1})Q_{Sj} + (1)Q_{Gj} \\ &+ \epsilon_{Gj} = Q_{Gj} + \beta_{S}(DIS_{Sj}^{1}Q_{Sj}) + \epsilon_{Gj} \\ V(C)_{j} &= (1)Q_{Cj} + (1 - \beta_{S}DIS_{5j}^{1})Q_{Sj} + (0)Q_{Gj} \\ &+ \epsilon_{Cj} = (Q_{Cj} + Q_{Sj}) + \beta_{S}(-DIS_{5j}^{1}Q_{Sj}) \\ &+ \epsilon_{Cj} \end{split}$$

For this district, Q_{Gj} is the fixed component for the Gaullist equation while $(Q_{Cj} + Q_{Sj})$ is the fixed component for the Communist equation. Clearly, such components vary from district to district depending upon the candidates present on the two ballots. Consequently, before using regression analysis to estimate β_i , this nonunit component is first subtracted from the dependent variable, $V(k)_j$, giving us a regression without an intercept term.

Estimation. The parameter estimates were not obtained from a set of regressions whose dependent variables were the vote proportions of the various parties. Instead we used ordinary least squares to estimate a pooled regression in which the votes for all parties were predicted by means of a single regression equation. This procedure is appropriate because β_i , although variant with the spatial position of the voter, is invariant with the party vote being predicted. However, in each district we have $\sum V(k)_j = 1$.

Because of this constraint, we always deleted the leftmost candidate in each district.²² The deleted candidate's predicted vote was given as one minus the sum of the predicted votes for the other candidates. For each district, then, we had $(N_{2j}-1)$ observations; total observations were $\sum N_{2j}-J$.

22The constraint also implies that it is incorrect to assume uncorrelated errors for observations within a given district. The ordinary least squares (OLS) estimates are thus inefficient. An estimation problem similar to ours has been addressed in T. W. McGuire, J. U. Farley, R. E. Lucas, Jr., and L. W. Ring, "Estimation and Inference for Linear Models in Which Subsets of the Dependent Variable Are Constrained," Journal of the American Statistical Association, 63 (December 1968), 1201–13. They obtain efficient estimates by deleting one observation per district and by applying a generalized least squares procedure. Their procedure cannot be applied directly because it would require that the same set of parties contest every district on the second ballot. A modified procedure we developed yielded estimated coefficients quite similar to those found by OLS. On the other hand, the results suggest that the standard error estimates produced by OLS are overly conservative.

Results for the All-Candidate Model. When we make RMSE comparisons between the all-candidates model and the previously discussed apriori models, we find that the all-candidates model has a moderately greater level of success. Tables 5 to 10 display, for the three major parties, the relevant RMSEs for 1958 and 1962. Since there are three a-priori models, there are $3 \pmod{s} \times 2 \pmod{s} \times 3 \pmod{s} = 18 \pmod{s}$ comparisons. For the pooled regression covering all districts, the all-candidates model has a lower RMSE in 15 of the 18 cases. While at worst it leads to a 13.4 percent increase in RMSE, at best it reduces RMSE by 43.5 percent. The average RMSE reduction is 17.1 percent. The pattern of RMSEs by year and party is similar to the results for the a-priori models. In particular, the worst predictions again result for the 1958 Gaullists.

In 14 of 15 comparisons the all-candidates model also does better than the a-priori model when we control for the number of candidates. In 1958, the modal number of candidates was three. For three-candidate races, the nine possible comparisons all favor the all-candidates model. For the modal two-candidate races of 1962, five of the six comparisons favor the all-candidates model.²³ The only RMSE increase in 1962 is 2.0 percent, substantially less than the maximum reduction of 24.4 percent. (Discussion of comparisons for the duels and truels of Tables 5 to 10 is deferred until the section on ecological models.)

The RMSE reduction afforded by the allcandidates model is far from uniform, and, indeed, in some comparisons it is inferior to one or more of the a-priori models. These facts may in part reflect our lack of knowledge concerning voter decision rules. What we can, with some degree of safety, conjecture about spatially oriented voters is contained in our pair of axioms; these are extremely weak in their implications. The three a-priori models and the all-candidates model are only four members of the class of decision rules consistent with the axioms. Of the four, the all-candidates model is, in one aspect, least satisfying since the linearity imposed to facilitate estimation implies that the model can only be consistent with the axioms over a restricted range of distances. It is not surprising then that the greater flexibility incorporated in the β_i coefficients of the all-candidates model is sometimes offset by the relative

²³There are six, not nine, comparisons because of the equivalence of models I and III for two-candidate races.

inappropriateness of the form of the decision rule.

Since the additional reduction in RMSE afforded by the all-candidates model is modest, there ought to be a strong concordance between theoretical requirements and the signs and magnitudes of the estimated coefficients if we are to sacrifice the parsimony of the a-priori models. In fact, agreement is exceptionally strong. First, as required, all coefficients, for all four years, are positive (see Table 11). An indication of how the calculated probabilities match up with the requirement that the probability of voting for the closest candidate be greater than that for the second closest is

provided in Table 12. To understand Table 12, consider the 1958 entry, "MRP" for the Communist voter location. This implies that if the closest candidate for a Communist voter in a two-candidate race is an MRP candidate, the Communist voter's probability of voting for the MRP candidate will be less than 0.5, thus violating a requirement on the magnitude of the estimated β_i parameters. If the closest candidate for the Communist voter is from a party to the Left of the MRP party (e.g., Socialist) this requirement is not violated. Although Table 12 shows that for the majority of the coefficients, scenarios could arise in which the requirement would be violated, such scenarios are nonexis-

Table 5. Root Mean Square Error Comparisons for 1958 Communist Predictions

Model .	All Districts	Three-Candidate Districts	Truels ^a
A-Priori			
Model I	0.0466	0.0379	0.0283
Model II	0.0479	0.0416	0.0208
Model III	0.0484	0.0397	0.0217
All-candidates	0.0274	0.0287	b
Closest-candidate			
CC Model ^c	0.0379	0.0439	0.0404
HCC Model ^d	0.0142	0.0155	0.0151
Ecological			
Exprimés Model	0.0261	0.0223	0.0111
Number of districts	385	229	68

^aDistricts with Communist-Socialist-Gaullist truels on the second ballot.

Table 6. Root Mean Square Error Comparisons for 1958 Socialist Predictions

Model	All Districts	Three-Candidate Districts	Truelsa
A-Priori			
Model I	0.0619	0.0636	0.0746
Model II	0.0704	0.0733	0.0851
Model III	0.0674	0.0702	0.0839
All-candidates	0.0613	0.0616	b
Closest-candidate			
CC Model ^c	0.0628	0.0672	0.0704
HCC Model ^d	0.0486	0.0469	0.0446
Ecological			
Exprimés Model	0.0721	0.0518	0.0448
Number of districts	208	126	68

^aDistricts with Communist-Socialist-Gaullist truels on the second ballot.

b_{Not} computed.

^cClosest-candidate model with a constant nonspatial component.

dHeuristic + closest-candidate model.

b_{Not} computed.

^cClosest-candidate model with a constant nonspatial component,

dHeuristic + closest-candidate model.

tent or rare in practice. For example, there is an entry "Extreme Right" for the PSU coefficient in 1967. As we are guaranteed, however, that there will be a candidate to the Left of the "Extreme Right," no violation occurs.

Let us consider the 1968 situation in detail. No violation is possible for the Communist, PSU, and Gaullist locations. For the Center, no violation occurs empirically for it is never the case that the only candidate is a Communist. For the Right, violations will occur only in the absence of a nonleftist candidate; for the Federation, only in the absence of a leftist. Just a handful of districts correspond to these

conditions. Only the very small Extreme Right group shows a serious violation. Similarly, there are few actual violations for the three other years.

The stability of the estimated coefficients for the *last three* elections is another factor supporting the model. As can be seen from Table 11, the range of variation of the coefficient for a given spatial location for 1962–1968 is small compared to the total range of variation. The only exceptions are the PSU and the Extreme Right which represent the smallest numbers of voters. For example, the Gaullist coefficient ranges from 0.094 to

Table 7. Root Mean Square Error Comparisons for 1958 Gaullist Predictions

Model	All Districts	Three-Candidate Districts	Truels ^a
A-Priori			
Model I	0.1008	0.0994	0.0894
Model II	0.1075	0.1068	0.0783
Model III	0.1046	0.1057	0.0873
All-candidates	0.0917	0.0969	b
Closest-candidate			
CC Model ^c	0.0808	0.0807	0.0539
HCC Model ^d	0.0719	0.0732	0.0434
Ecological			
Exprimés Model	0.1232	0.1043	0.0483
Number of districts	268	144	68

^aDistricts with Communist-Socialist-Gaullist truels on the second ballot.

Table 8. Root Mean Square Error Comparisons for 1962 Communist Predictions

Model	All Districts	Two-Candidate Districts	Comm-Gaul Duels ^a
A-Priori			
Model I	0.0344	0.0342	0.0339
Model II	0.0405	0.0426	0.0342
Model III	0.0353	0.0342	0.0339
All-candidates	0.0390	0.0349	b
Closest-candidate			
CC Model ^c	0.0553	0.0612	0.0637
HCC Modeld	0.0287	0.0245	0.0257
Ecological			`
Exprime's Model	0.0489	0.0283	0.0214
Number of districts	210	114	99

^aDistricts with Communist-Gaullist duels on the second ballot.

b_{Not} computed.

^cClosest-candidate model with a constant nonspatial component.

dHeuristic + closest-candidate model.

b_{Not} computed.

^cClosest-candidate model with a constant nonspatial component.

dHeuristic + closest-candidate model.

0.189 while the coefficients for the five major locations range from .041 to .662.

This stability is impressive in view of the varying substantive context of the three elections. The 1962 election was conducted with Algeria still an issue, and the controversy over De Gaulle's referendum on direct election of the president was its immediate issue. The 1967 election was much more representative of classic Left-Right economic and social welfare issues while the 1968 election was characterized by "the great fear" that followed the student-

worker demonstrations and strike of May-June Apparently, from the viewpoint of the model, these substantive changes can be represented as shifts in the spatial distribution shown by the first ballot. On the other hand, voter second ballot decision rules were stable in the sixties and the all-candidates model thus had predictive capacity.

The differences between 1958 and the other years might, as we suggested in our earlier article, be due either to lack of experience or to a substantive difference. In effect, the Left was

Table 9. Root Mean Square Error Comparisons for 1962 Socialist Predictions

Model -	All Districts	Two-Candidate Districts	Soc-Gaul Duels ^a
A-Priori			
Model I	0.0538	0.0516	0.0526
Model II	0.0462	0.0442	0.0408
Model III	0.0528	0.0516	0.0526
All-candidates	0.0425	0.0390	b
Closest-candidate		4	1
CC Model ^c	0.0422	0.0365	0.0306
HCC Model ^d	0.0358	0.0339	0.0287
Ecological _			
Exprime's Model	0.0904	0.0480	0.0263
Number of districts	117	76	. 58

⁸Districts with Socialist-Gaullist duels on the second ballot.

Table 10. Root Mean Square Error Comparisons for 1962 Gaullist Predictions

Model .	All Districts	Two-Candidate Districts	Gaul-Comm Duels ^a	Gaul-Soc Duels ^b
A-Priori	<u> </u>			
Model I	0.0506	0.0487	0.0339	0.0526
Model II	0.0572	0.0457	0.034 2	0.0408
Model III	0.0548	0.0487	0.0339	0.0526
All-candidates	0.0508	0.0383	c	***
Closest-candidate				
CC Modeld	0.0358	0.0318	0.0285	0.0293
HCC Model ^e	0.0320	0.0312	0.0285	0.0285
Ecological	•			•
Exprimés Model	0.0788	0.0548	0.0209	0.0283
Number of districts	327	195	99	58

^aDistricts with Gaullist-Communist duels on the second ballot.

b_{Not} computed.

^cClosest-candidate model with a constant nonspatial component.

dHeuristic + closest-candidate model.

^bDistricts with Gaullist-Socialist duels on the second ballot.

^cNot computed.

dClosest-candidate model with a constant nonspatial component.

^eHeuristic + closest-candidate model.

Table 11. Spatial β Coefficients for the All-Candidates Model (All Districts)

	Estimated β Coefficients ^a			
Voter Location	1958	1962	1967	1968
Communist	.141	.041	.069	.084
	(.079)	(.015)	(.016)	(.019)
PSU	.820	.326	.137	.057
	(.251)	(.066)	(.056)	(.051)
Socialist Federation in 1967 and 1968	.412 (.045)	.222 (.027)	.265 (.021)	.295
Radical) redefation in 1907 and 1908	.371 (.039)	.225 (.028)		(.026)
MRP-Center	.104	.366	.429	.194
	(.045)	(.046)	(.030)	(.041)
Right-Rep. Ind.	.097	.412	.662	.522
	(.046)	(.075)	(.103)	(.070)
Gaullist	.251	.094	.189	.105
	(.067)	(.036)	(.035)	(.040)
Extreme Right	.127	.447	1.016	.328
	(.134)	(.232)	(.369)	(.512)
RMSE	.0695	.0507	.0463	.0388

^aStandard errors of the coefficients are in parentheses.

Table 12. Analysis of Voting Probabilities for the All-Candidates Model (All Districts):
Closest Candidate for Which a Theoretical Violation Occurs

Voter Location	1958	1962	1967	1968
Communist	MRPa	None	None	None
PSU	Communist	MRP	Extreme Right	None
Socialist Federation in Radical 1967 and 1968	Communist Communist	Right Gaullist	Center	Center
MRP-Center	None	Extreme Right	Extreme Right	Communist
Right	None	Radicals	Extreme Right	Federation
Gaullist	Radicals	None	PSU	None
Extreme Right	Communist	MRP	Gaullist	Gaullist

^aParty closest to location for which probability of vote is less than 0.5 if the party is actually the closest candidate in a two-candidate race.

Table 13. How Viability Affects the Postdictions of the All-Candidates Model: RMSEs by Year and Number of Candidates

	,	Number of Candidates		
Year	. 2	3	4	All Districts
1958	.048 (83) ^a	.071 (240)	.073 (91)	.070 (422)
1962	.040 (229)	.060 (125)	b (11)	.051 (365)
1967	.033 (329)	.066 (65)	(2)	.046 (396)
1968	.032 (270)	.063 (46)	(1)	.039 (316)

a Number of districts are in parentheses.

^bToo few districts for computation.

divided in 1958 while it tacitly or overtly made second ballot coalitions in the later years. ²⁴ On the other hand, the failure of purely spatial models to accommodate viability factors suggests such models are inappropriate for multicandidate races. In turn, these were by far most numerous in 1958 (reflecting the division of the Left). If we can link large prediction errors to viability considerations, the more distinct coefficients found for 1958 can be viewed as the result of specification error rather than a modification of voter behavior.

We began our investigation of viability effects with a detailed examination of 1962 Gaullist residuals. No geographic, urban-rural, or other socioeconomic pattern was suggested. On the other hand, of the largest residualsthose over .10 in magnitude-22 of 28 came from districts with three or more candidates. precisely those where viability effects are present. Viability thus appears as the critical element missing from our all-candidates model specification. To isolate its effects, we reestimated the model controlling for the number of candidates, with the results shown in Table 13. Two-candidate races, where viability is not a consideration, have substantially lower RMSEs. Although coefficient differences between 1958 and later years persist, interyear RMSE differences are attenuated if not entirely eliminated. The importance of viability is further demonstrated by results from the closest-candidate model described below.

The Closest-Candidate Model

The Model of Individual Voting Behavior. In the closest-candidate model, the individual's voting behavior is represented as before, by $Pr(k)_{ij}$. In this model, $Pr(k)_{ij}$ contains a nonspatial component in addition to a spatial component. The spatial component of $Pr(k)_{ij}$ is expressed as a function of the difference between: (1) the distance of candidate k and (2) the distance of the candidate closest to location i. The larger the value of this difference, the smaller should be the value of $Pr(k)_{ij}$. On the other hand, when k is the candidate closest to i, $Pr(k)_{ij}$ should attain its maximum value (all other factors remaining constant). Thus, $Pr(k)_{ij}$ may be expressed as:

$$Pr(k)_{ij} = \alpha_{jk} + \delta_{ik}(DIS_{ij}^{1} - D_{ij}^{k})$$

where α_{jk} is the nonspatial component of the model and δ_{ik} is a coefficient to be estimated. δ_{ik} should have a positive sign. Although

aggregation procedures follow those of the previous models, a separate regression is run for each party for this model.

Specification of the Model's Nonspatial Component. The nonspatial component of the model, α_{jk} , can be represented by a constant, α , or by an expression containing a constant and one or more variables. α_{jk} is represented here as a heuristic model consisting of a constant and the following explanatory variables:

- 1. V1: the proportion of first ballot votes cast for candidate k. As an indicator of the candidate's viability, the coefficient of V1 should clearly be positive.
- 2. $1/N_2$: N_2 is the number of second ballot candidates. The larger the value of N_2 , the smaller should be the vote proportion for candidate k. In the regression, we use the reciprocal of N_2 , $1/N_2$.²⁵ The coefficient of $1/N_2$ should have a positive sign.
- 3. COMP: the ratio of the votes obtained on the first ballot by candidate k to the first ballot votes obtained by the leading candidate. In general, $0 < COMP \le 1$. COMP equals 1 if candidate k is the leading candidate on the first ballot. Clearly, the coefficient of COMP should have a positive sign.

Potential explanatory variables such as the proportion of blank ballots and the proportion of abstentions on the first ballot were included in the model initially but provided no explanatory power; neither did party coalition dummy variables or regional dummy variables.²⁶ The failure of the dummy variables to provide explanatory power provides support for our view that the nonspatial component is of greater importance to the participation decision than to the voting choice decision.

There were two major problems with the closest-candidate model. First, the signs of the estimated spatial parameters demonstrated poor agreement with the microtheory of individual spatial voting.²⁷ Next, the model violated the adding-up condition at the microlevel although

²⁴Rosenthal and Sen, p. 45.

²⁵Use of the reciprocal is motivated in Rosenthal and Sen, p. 42.

²⁶These variables are defined in Rosenthal and Sen, p. 43.

²⁷Subrata K. Sen, "The Calculus of Voting: Empirical Studies for France," (Ph.D. dissertation, Carnegie-Mellon University, 1971), Ch. 3, pp. 41-42.

at the macro, constituency, level the sum of the estimated vote shares for the parties was "close" to 1.0 in all districts. Despite these deficiencies, the closest-candidate model was highest in "explanatory power," if only marginally better than other models more satisfying from a theoretical viewpoint. We will use the closest-candidate model only as a benchmark to suggest a level of root mean square error that might be achieved by a better theoretical model using the same input data.

Because the microlevel results for this model are poor, we omit any discussion of the spatial coefficients. Moreover, without the nonspatial component, the RMSEs of this model (i.e., the CC model in Tables 5 to 10) for "All Districts" regressions fail to dominate those of the allcandidates model except for the 1958 and 1962 Gaullists. On the other hand, including the nonspatial component makes this model (i.e., the HCC model in Tables 5 to 10) best from the viewpoint of low RMSE. It is also the case that V1 and COMP, the two variables most clearly related to viability, always have the correct sign, except for the COMP coefficient in the 1958 Socialist truels. However, the t-statistic (-.88) indicates that this coefficient does not differ significantly from zero. 1/N2 has an incorrect sign only for the 1962 Gaullists.

These results suggest that a viability component could be added to the all-candidates model. One way to accomplish this would be to allow the vote accorded the closest candidate to be calculated as:

$$1 - B_{1i}DIS_{ij}^{1} - B_{2i}V_{i}^{1} - B_{3i}(1/N_{2}) - B_{4i}COMP_{i}^{1}$$

where V_i^1 is the first ballot vote of the candidate closest to i and $COMP_i^1$ is the ratio of the vote of the candidate closest to i to the leading candidate on the first ballot. Table 4 could then be readily revised to yield the vote for the other candidates. Potential problems are that V_i^1 may be highly collinear with spatial terms and that the variables involved are relatively poor proxies for viability effects, which involve a consideration of the chances of all the candidates and not just the preferred candidate.

Ecological Models

The results for the closest-candidate model complete our presentation of spatial models. For comparison purposes, we now turn to the standard ecological models other researchers have used to analyze French legislative elections.²⁸ Consider the subsets of individuals exercising a given choice on the first ballot: either voting for a party, abstaining, or casting a blank ballot. Ecological models are based on the assumption that in each subset the expected value of the proportion voting for a given party on the second ballot is constant for all districts.

With i = 1, ..., I again indexing the parties, let i = I + 1 index abstention and I + 2 index blank ballot. Then let R_{ii} equal the proportion of registered voters in district j who made choice i on the first ballot, and $R(k)_{ij}$ equal the (conditional) proportion of those choosing i on the first ballot who also choose k(k = 1, ..., I+ 2) on the second ballot. The standard ecological model assumes:

$$E[R(k)_{ii}] = \mu_{ik},$$

i.e., a constant for all districts. Aggregating for all first ballot choices over all voters in the district, we obtain

$$R(k)_{j} = \sum_{i=1}^{I+2} \mu_{ik} R_{ij} + \epsilon_{jk}$$
leading to the regression equation:
$$R(k)_{j} = \rho_{0k} + \sum_{i=1}^{I+1} \rho_{ik} R_{ij} + \epsilon_{jk}$$

$$R(k)_{j} = \rho_{0k} + \sum_{i=1}^{I+1} \rho_{ik} R_{ij} + \epsilon_{jk}$$

where the ρ 's are coefficients to be estimated and $R(k)_i$ is the total proportion of registered voters making choice k on the second ballot in district j. Blank ballots $(R_{I+2,j})$ are purposely omitted from the set of explanatory variables in order to avoid a linearly dependent set of variables (since all vote proportions must sum to 1.0). We can now express the μ_{ik} in terms of the regression coefficients as follows:

$$\mu_{ik} = \rho_{0k} + \rho_{ik}, i = 1, 2, \dots, I+1$$

 $\mu_{I+2.k} = \rho_{0k}.$

Since the μ_{ik} are voting probabilities, one should obtain estimates such that $0 \le \rho_{0k} \le 1$, and $0 \le (\rho_{0k} + \rho_{ik}) \le 1$, for i = 1, 2, ..., I+1.

These models of registered vote proportions are not directly suitable for comparisons with the spatial models of exprime's (valid vote)

²⁸For French applications see: B. Mendès-France and L. Laumonier, "Une Application des Methodes de l'Analyse Statistique à l'Estimation des Déplacements r'Analyse Statistique à l'Estimation des Déplacements de Voix entre les Deux Tours des Elections Présidentielles de 1965," Revue Française de Science Politique, 17 (February 1967), 110-14, and Alain Lancelot and Pierre Weill, "Les Transferts de Voix du Premier au Second Tour des Elections de Mars 1967; Une Analyse de Regression," in Les Elections Legislatives de Mars 1967, Centre d'Etude de la Vie Politique Française (Paris: A. Colin 1970), pp. 373-88 Francaise (Paris: A. Colin, 1970), pp. 373-88.

proportions. Clearly, we can convert the $R(k)_j$ predictions to $V(k)_j$ predictions through the formula:

$$V(k)_{j} = \frac{R(k)_{j}}{\sum_{\substack{k'=1}\\k'=1}} R(k')_{j}$$

We will refer to this as the ecological (inscrits-exprimes) prediction.

Similarly, we can convert the spatial predictions of $V(k)_j$ to predictions of $R(k)_j$ through the formula:

$$\hat{R}(k)_j = \hat{V}(k)_j [\sum_{i=1}^I P_{ij} \hat{PV}_{ij}] \,. \label{eq:reconstruction}$$

We will refer to this, for the all-candidates model, as the all-candidates (exprimés-inscrits) prediction.

Another form of comparison, however, is to construct an ecological model based on exprimes proportions. The regression model is:

$$V(k)_j = \rho_{0k} + \sum_{i=1}^{I-1} \rho_{ik} P_{ij} + \epsilon_{jk}$$

and the μ_{ik} are now given as:

$$\mu_{ik} = \rho_{0k} + \rho_{ik}, i = 1, 2, ..., I - 1,$$

 $\mu_{Ik} = \rho_{0k}.$

As stated earlier, the standard ecological models assume that the expected proportion of voters voting for a specific party on the second ballot is constant over all districts, given party choice or abstention or blank ballot on the first ballot. Naturally, such an assumption has little face validity, as the probability of a Socialist voting for a Communist, say, ought to depend heavily on whether a Socialist is on the ballot. Consequently, unlike our models, which are designed to have full scope and apply to all districts, ecological models are traditionally run on subsets of the districts for which the "constant" assumption is reasonable, an example being districts with Communist-Gaullist second ballot duels. In fact, one of the aims of this study is to show that the spatial model in large part eliminates the need for subsets and, in addition, has the advantage of providing analysis for district types that cannot be included in one of the subsets. The 11 fourcandidate races in 1962 illustrate districts that would normally be excluded by ecological models.

Results for the Ecological Model. In discussing the ecological model we emphasize comparisons with the all-candidates model, our preferred spatial model. Only results for the *exprimés* model are shown; results for the *inscrits* model are similar. The ecological model clearly cannot be used with full scope. When applied to all districts, both *inscrits* and *exprimés* comparisons between the ecological model and the all-candidates model show the all-candidates model dominating. Even the a-priori model dominates the ecological model, with the exception of the Communists in 1958 (see Tables 5 to 10).

For two-candidate races in 1962, the ecological model is also dominated by the all-candidates model (except for the Communists) and is roughly equalled in explanatory power by the a-priori model. Only in duels between specific parties does the ecological model tend to postdict better. There are exceptions, however, and the ecological model's advantage is slight, postdictions RMSEs being lowered by less than .003 on the average. Moreover, in data sets restricted to specific parties, an unpooled allcandidates model is equivalent to an ecological model except that (1) the estimated Q_{ii} 's are used in place of the first ballot results and (2) the estimated probabilities are constrained to satisfy our first axiom. The lack of this constraint is primarily responsible for the slight advantage of the ecological model. This advantage is often bought, however, at the cost of estimated probabilities being greater than 1.0.

Similarly, for the 1958 data, the ecological model does well only in highly specific subsets, namely Communist-Socialist-Gaullist truels. For all three-candidate districts, the ecological model is better than the all-candidates model for Communists and Socialists but worse for Gaullists.

The microlevel results for the ecological models are poor. When estimated for all districts, the μ coefficient of the party that is the dependent variable in the equation is always greater than 1.0 for the 12 possible cases (2 years x 2 models x 3 parties). When this coefficient is tested against 1.0 (as against the usual test of 0.0), the t-statistics range from 0.70 to 13.89, with only two of 12 less than 2.0. While the microlevel results for duels are satisfactory, they are not so for the 1958 truels. Consider, for example, the 1958 Communist inscrits model. (The inscrits-exprime's predictions outperform the direct exprime's predictions here.) For the truels, the Communist μ coefficient is substantially greater than 1.0 while the coefficients for three of the other parties are less than zero, two of them substantially so. Multicollinearity, of course, can have a heavy hand in these results, giving another

reason for preferring the more constrained all-candidates model.

For the voting equations, therefore, the ecological models appear to be less desirable than the spatial models, especially when the greater scope and parsimony of the spatial models is considered. We also recall that ecological models are even less successful for nonvoting than for voting.²⁹ This suggests that spatial models should replace ecological models as the mode for analyzing aggregate voter shifts between the two ballots.

Summary of Results for the Four Models

We have described and estimated four types of models designed to predict the vote proportions of candidates on the second ballot. The first two types, the a-priori models and the all-candidates model, are theoretically based. The remaining models, the closest-candidate model and the ecological model, are more heuristic in nature and are viewed as null models for the two theoretical types.

The empirical results indicate that of the two types of theoretical models, the all-candidates model is superior. Its coefficients appeared stable throughout the 1960s. When viability considerations were eliminated by restricting the analysis to two-candidate races, candidate vote proportions were postdicted with very satisfactory root mean square errors (3.2 to 4.8 percent). The closest-candidate null model gives rise to good macro predictions but does not provide microlevel interpretation. The ecological model is good only when the domain of analysis is restricted to districts involving duels or truels. Compared to the null models,

²⁹Rosenthal and Sen, pp. 38-40.

the two theoretical models also rely on a smaller number of estimated parameters. (Table 14 displays the total number of parameters needed when the models are applied to predicting abstentions, blank ballots, and the vote for eight parties.) Moreover, as compared to the ecological model, the theoretical models, particularly the all-candidates model, have reasonably low RMSEs when applied to the entire body of data. These results, coupled with those of our earlier article, demonstrate the importance of spatial models in empirical analysis.

Postscript: Validation and Replication

Subsequent to the original writing and presentation of this paper, we were able to acquire and code the 1973 election results. These are now used for purposes of replication and cross-validation of the all-candidates model.

The replication was performed by reestimating the model using the new data. In terms of RMSE (as can be seen comparing Tables 15 and 13), the model performed better for 1973 and 1968 than for any of the earlier years. Similarly, in our nonvoting paper, we found that the alienation model performed better for later elections. The 1973 results therefore lend weight to the conjecture advanced in the earlier article that, through learning initiated with the new system of 1958, the French electorate is tending toward more patterned and systematic behavior.

The replication also reinforces our findings with regard to viability: once again results were substantially better when estimation was restricted to districts with only two candidates. The only disappointing aspect of the replication concerned the negative coefficient estimated

Table 14. Number of Parameters Estimated for Each Model

	*		
	8 Parties	8 Parties Plus Abstentions and Blank Ballots	
A-Priori Models ^a	28	28	
All-Candidates Model ^a	36	36	
Closest-Candidate Model ^a Without heuristic variables With heuristic variables	64 88	92 116	
Ecological Model Exprimés version Inscrits version	64 80	b 92	

^aParameters estimated for the abstention and blank ballot models in Rosenthal and Sen, "Electoral Participation in the French Fifth Republic," *American Political Science Review*, 67 (March 1973), 44-51, are included in the total.

bDoes not apply.

for the PSU. However, the PSU point encompasses only a small fraction of the electorate, and the coefficient is not large relative to its estimated standard error.

We also cross-validated the model by using estimated 1967 and 1968 nonvoting and voting coefficients to make simulated ex ante forecasts of the 1973 results. Clearly, one would expect the 1967 and 1968 coefficients to lead to poorer results than those estimated from the data. (Because our model uses the Q_{ii} terms estimated from the nonvoting equations, there is no absolute guarantee that 1973 estimated coefficients will outperform those from prior elections.) Consequently, we need a benchmark that indicates "how much worse is worse." As a benchmark, we calculated (as shown in Table 15) RMSEs for the "deterministic" model which says that if voters vote, they vote for the closest candidate with probability 1.0. This is equivalent to the all-candidates model with all β_i constrained to zero. The benchmark has a RMSE of 5.0 percent as against 3.9 percent for the all-candidates model (4.7 vs. 3.0 percent in the case of duels).

The cross-validation results using the 1968 coefficients are impressive. As shown in Table 16, for all districts, the RMSE is 4.1 percent as against 3.9 percent for the estimated 1973 model. For two-candidate races, the RMSE increases from 3.0 to 3.7 percent but this is still substantially less than the benchmark of 4.7 percent. We stress that the Barnes and Pierce spatial position measures were computed from a 1968 survey. These combined with the 1968 coefficients would thus have led to a reasonably successful, if very short-run, forecast of the second ballot.

Table 16 also discloses that 1968 could have been forecast with some success from 1967 coefficients. On the other hand, use of 1967 coefficients on 1973 data leaves one little better off than the benchmark.

In conclusion, the replication and validation results show that a one-dimensional spatial model of the second ballot has substantial explanatory power. While there are some changes in time, the coefficients of the model are sufficiently stable to tentatively suggest that coefficients from the previous election, if not

Table 15. The All-Candidates Model Vs. Deterministic Voting for the Closest Candidate: Spatial (β) Coefficients and RMSEs for 1973

	Spatial (β) Coefficients ^a			
•	All-Candidates Model		Deterministic Model	
Voter Location	All Districts	Two Candidates	All Districts	Two Candidates
Communist	.039 (.011)	.060 (.009)	0.0	0.0
PSU	136 (.081)	066 (.060)	0.0	0.0
Socialist	.198 (.016)	.222 (.012)	0.0	0.0
Radical	.230 (.040)	.217 (.029)	0.0	0.0
Center	.266 (.026)	.301 (.019)	0.0	0.0
Right-Rep. Ind.	.339 (.078)	.592 (.069)	0.0	0.0
Gaullist	.102 (.041)	.160 (.032)	0.0	0.0
Extreme Right	.516 (.105)	.215 (.129)	0.0	0.0
RMSE	.0385	.0300	.0497	.0472
Number of districts	483	352	483	352

^aStandard errors of all estimated coefficients are in parentheses.

still earlier elections, can be used for forecasting purposes. The success of the model is particularly apparent for two-candidate races where, paralleling theoretical arguments, empirical evidence now indicates that viability considerations are of small import. The major area for improvement lies with a model of strategic voting that would ameliorate the all-candidates model's performance for multicandidate races.

Table 16. Replication and Validation:
RMSEs by "Election Predicted" and "Election Used to Estimate Coefficients"

	Election Used to Estimate Coefficients		
Election Predicted	1967	1968	1973
1967:			
All districts	.046		
Two-candidate districts	.033		
1968:			
All districts	.044	.039	
Two-candidate districts	.040	.032	
1973:			
All districts	.047	.041	.039
Two-candidate districts	.046	.037	.030

Political Parties and Macroeconomic Policy*

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This study examines postwar patterns in macroeconomic policies and outcomes associated with leftand right-wing governments in capitalist democracies. It argues that the objective economic interests as
well as the subjective preferences of lower income and occupational status groups are best served by a
relatively low unemployment-high inflation macroeconomic configuration, whereas a comparatively high
unemployment-low inflation configuration is compatible with the interests and preferences of upper
income and occupational status groups. Highly aggregated data on unemployment and inflation outcomes
in relation to the political orientation of governments in 12 West European and North American nations
are analyzed revealing a low unemployment-high inflation configuration in nations regularly governed by
the Left and a high unemployment-low inflation pattern in political systems dominated by center and
rightist parties. Finally, time-series analyses of quarterly postwar unemployment data for the United
States and Great Britain suggests that the unemployment rate has been driven downward by Democratic
and Labour administrations and upward by Republican and Conservative governments. The general
conclusion is that governments pursue macroeconomic policies broadly in accordance with the objective
economic interests and subjective preferences of their class-defined core political constituencies.

In so far as stable prices are regarded as desirable for their own sake, as contributing to social justice, it must be recognized that justice to the rentier can be achieved only by means of the injustice to the rest of the community of maintaining a lower level of effective demand than might otherwise be achieved. We are here presented with a conflict of interests ... and actual policies are largely governed by the rival influences of the interests involved. (Joan Robinson, Essays in the Theory of Employment (New York: Macmillan, 1937), p. 35.)

From one important point of view, indeed, the avoidance of inflation and the maintenance of full employment can be most usefully regarded as conflicting class interests of the bourgeoisie and the proletariat, respectively, the conflict

*This article is taken from my longer monograph Economic Interest and the Politics of Macroeconomic Policy. Earlier versions of the paper were delivered to the Econometric Society World Congress, Toronto, Canada, August 1975, and the Annual Meeting of the American Political Science Association, San Francisco, August 1975. The research has been supported by National Science Foundation Grants GS 33121 and SOC75-03773. The Computer Research Center of the National Bureau of Economic Research provided computational support. I am indebted to Hayward Alker, Suzanne Berger, Bob Brito, Randy Forsberg, J. David Greenstone, David Held, Mike Intriligator, Robert Jackman, Peter Lemieux, Frank Lerman, Andrew Martin, Benjamin Page, Adam Przeworski, Martin Rein, William Schneider, Robert Solow, and Paolo Sylos-Labini for comments on an earlier draft. The research assistance of Warren Fishbein, Marilyn Shapleigh and especially Nick Vasilatos is gratefully acknowledged. I retain the usual responsibility for errors of fact and judgment.

being resolvable only by the test of relative political power in the society. (Harry G. Johnson, "Problems of Efficiency in Monetary Management," Journal of Political Economy, 76 (September/October 1968), p. 986.)

We tend to get our recessions during Republican administrations.... The difference between the Democrats and the Republicans is the difference in their constituencies. It's a class difference ... the Democrats constitute the people, by and large, who are around median incomes or below. These are the ones whom the Republicans want to pay the price and burden of fighting inflation. The Democrats [are] willing to run with some inflation [to increase employment]; the Republicans are not. (Paul A. Samuelson, "Some Dilemmas of Economic Policy," Challenge, 20 (March/April 1977), pp. 30-31.)

The most important problem of macroeconomic policy facing public authorities in industrial societies during the postwar period has been the unfavorable trade-off that exists between unemployment and inflation—the socalled "Phillips curve." Although the unemployment/inflation trade-off has not exhibited great stability in recent years—for example, the U.S. economy is undoubtedly more vulnerable to inflation at low levels of unemployment now than it was a few years ago—there is widespread agreement among economists that in capitalist economies wage and price stability requires relatively high levels of unemployment, and, conversely, that low rates of unemployment yield relatively high rates of inflation. Put another way, price stability and full employment are incompatible goals in the sense that conventional macroeconomic policy has not been able to achieve both simultaneously. Since political authorities can (and do) influence the rate of unemployment and inflation by man-pulation of monetary and fiscal policy instruments, macroeconomic policy has been the focus of intense controversy and conflict between key political actors and interest groups.

This article examines postwar patterns in macroeconomic policies and outcomes associated with left- and right-wing governments in capitalist democracies. The main body of the article has three parts. The first section briefly reviews evidence, which is documented in great detail elsewhere,² indicating that different unemployment/inflation outcomes have important, class-linked effects on the distribution of national income. It is argued that the economic interests at stake in various macroeconomic configurations are (implicitly) reflected in public opinion data on the relative aversion of different income and occupational groups to unemployment and inflation. The second part of the article presents a general scheme rank-ordering the preferences of political parties, arrayed along the traditional left to right spectrum, toward various economic goals, and analyzes highly aggregated data on unemployment and inflation outcomes in relation to the political orientation of regimes in 12 West European and North American nations. These international comparisons suggest that the "revealed preference" of leftist governments has been for relatively low unemployment at the expense of high rates of inflation, whereas, comparatively low inflation and high unemployment characterize political systems dominated by center and right-wing parties. The third and longest section of the article presents time-series analyses of quarterly postwar data on unemployment in the United States and Great Britain. The estimation results from the time-series models support the conclusion that unemployment has been driven downward during the tenure of Democratic and Labour administrations and has moved upward during periods of Republican and Conservative

rule in the United States and Great Britain, respectively.

The general conclusion of the study is that the macroeconomic policies pursued by leftand right-wing governments are broadly in accordance with the objective economic interests and subjective preferences of their classdefined core political constituencies.

Unemployment and Inflation: Objective Economic Interests and Subjective Preferences

A common rationalization for deflationary macroeconomic policies is that inflation adversely affects the economic position of wage and salary earners and, in particular, erodes the economic well-being of the poor. Empirical studies, however, give little support to this argument. The work of Blinder and Esaki, Hollister and Palmer, Metcalf, Thurow, Schultz, and others strongly indicates that a relatively low unemployment-high inflation macroeconomic configuration is associated with substantial relative and absolute improvements in the economic well-bring of the poor and, more generally, exerts powerful equalizing effects on the distribution of personal income.³

Although these studies suggest that inflationary periods with tight labor markets are associated with a general equalization of the income distribution—the poor and certain middle income groups gaining at the expense of the rich—it nevertheless has been argued that the economic position of a substantial fraction of the labor force suffers a net decline during periods of vigorous economic expansion. The usual observation is that price rises tend to outstrip money wage increases during cyclical upswings and real wage rates therefore fall. Moreover, business expansions bring a general inflation of profits which yields increases in the

¹A detailed review of the theoretical and empirical literature on Phillips-curve inflation models is given in my *Economic Interest and the Politics of Macroeconomic Policy*, No. C/75-14, Center for International Studies, M.I.T., Cambridge, Mass., January 1976. Copies of this monograph are available at cost from the C.I.S. Publications officer.

²Ibid.

³See, for example, A. Blinder and H. Esaki, "Macroeconomic Activity and Income Distribution in the Postwar U.S." (mimeo., November 1976); Robinson G. Hollister and John L. Palmer, "The Impact of Inflation on the Poor," in Redistribution to the Rich and the Poor, ed. K. E. Boulding and M. Pfaff (Belmont, Calif.: Wadsworth, 1972), pp. 240–70; Charles E. Metcalf, An Econometric Model of the Income Distribution (Chicago: Markham, 1972); Lester C. Thurow, "Analyzing the American Income Distribution," American Economic Review: Papers and Proceedings, 60 (May 1970), 261–69; and T. Schultz, "Secular Trends and Cyclical Behavior of Income Distribution in the United States: 1944–1964," in Six Papers on the Size Distribution of Wealth and Income, ed. L. Soltow (New York: National Bureau of Economic Research, 1969), pp. 75–100.

share of the national income going to capital.⁴ If the profit-inflation and wage-lag hypotheses are accurate, it is possible in principle that the relative and absolute gains enjoyed by lower income groups during economic booms come at the expense of other wage earning groups and conceal substantial declines in the national income share of labor as a whole.

However, contemporary empirical work provides little or no evidence in favor of either the profit-inflation or wage-lag hypothesis. Long's examination of historical relationships in the United States (1860 to 1958) found that real wage movements were not countercyclical, as Keynes and others argued, but on the whole corresponded quite closely to business fluctuations.⁵ Bodkin's analysis of postwar quarterly and longer-run annual data on trend-corrected real wage changes in Canada and the United States detected no systematic association one way or the other between real wage movements and unemployment in Canada, whereas inverse associations prevailed in the United States.6 Finally, studies by Bach and Stephenson, Boddy and Crotty, Burger, Hibbs, Kuh, Hultgren, and the Organization for Economic Cooperation and Development on the cyclical behavior of factor shares, i.e., shares of the national income going to capital and labor over the business cycle, show that in general the ratio of profits to wages increases steadily after a trough in business activity, reaches its highest point about midway through an expansion, and thereafter drops off markedly. Thus the latter halves of business upswings, during which unemployment typically falls and the rate of inflation rises, are associated with a pronounced squeeze on profits and are more accurately described as periods of wage-lead and profitdeflation. Although it is difficult to say whether these patterns in the cyclical behavior of wages and profits would persist in prolonged expansions, the evidence does demonstrate that the economic position of wage and salary earners as a group improves substantially, both in relative and absolute terms, during periods of relatively low unemployment and high rates of inflation.

If sustained economic expansions confer such obvious benefits on wage and salary earners generally and on low and middle income groups in particular, why have macroeconomic policy makers exhibited such keen sensitivity to the inflationary consequences of full employment? One explanation of why political authorities have been willing to accept less than full employment is that the mass of wage and salary earners have an "irrational" aversion to inflation, perhaps because people tend to view rising prices as an arbitrary "tax."8 Deflationary macroeconomic policies may therefore represent the political response to widespread anti-inflation sentiment in the mass public.9 Sample survey evidence for the United States and Great Britain squarely contradicts this argument. For more than 20 years George Katona and has associates at the Survey Research Center of the University of Michigan

⁴These hypotheses have a distinguished pedigree. They have appeared, among other places, in Earl J. Hamilton, "Prices and Progress," Journal of Economic History, 12 (Fall 1952), 325–49; Alvin Hansen, "Factors Affecting the Trend of Real Wages," American Economic Review, 15 (March 1925), 27–42; John Maynard Keynes, The General Theory of Employment, Interest and Money (New York: Harcourt, 1936); Jacques Rueff, "Nouvelle discussion sur le chomage, les salaires et les pris," Revue d'Economic Politique (1951), 761–91; and Sidney Weintraub, An Approach to the Theory of Income Distribution (Westport, Conn.: Greenwood Press Inc., 1958). Weintraub, for example, has flatly asserted that "... only entrepreneurs and the actual unemployed have an unequivocal stake in maximum employment, while rentiers and the employed find their interests better served at lower levels of activity," p. 60.

⁵Clarence D. Long, "The Illusion of Wage Rigidity: Long and Short Cycles in Wages and Labor," Review of Economics and Statistics, 42 (May 1960), 140-51.

⁶Ronald G. Bodkin, "Real Wages and Cyclical Variations in Employment: A Re-Examination of the Evidence," *Canadian Journal of Economics*, 2 (February to November 1969), 353-74.

⁷G. L. Bach and James B. Stephenson, "Inflation and the Redistribution of Wealth," *Review of Eco-*

nomics and Statistics, 61 (February 1974), 1–13; Raford Boddy and James Crotty, "Class Conflict and Macro-Policy: The Political Business Cycle," Review of Radical Political Economics, 7 (Spring 1975), 1–19; Albert Burger, "Relative Movements in Wages and Profits," Federal Reserve Bank of St. Louis Review, 55 (February 1973), 8–16; Hibbs, "Economic Interest"; Edwin Kuh, "Income Distribution and Employment over the Business Cycle," in Brookings Quarterly Econometric Model of the United States, ed. J. Dusenberry et al. (Chicago: Rand McNally, 1965), pp. 227–78; Thor Hultgren, Costs, Prices, and Profits: Their Cyclical Relations (New York: National Bureau of Economic Research, 1965); and OECD, Inflation, The Present Problem (Paris: OECD Publications, 1970).

⁸This has been proposed, for example, in William D. Nordhaus, "The Political Business Cycle," *Review of Economic Studies*, 42 (April 1975), 169-90.

⁹As one White House economist reportedly put it in April of 1975 "One hundred percent of the people have been hit by inflation. Only 10 percent really worry about unemployment." Quoted by S. Golden, "High Joblessness Expected to Persist as a Condition of U.S. through Decade," New York Times (April 21, 1975), p. 46.

have polled national samples of American households about their expectations and attitudes toward inflation, unemployment, and other socioeconomic issues. Katona writes that until 1973 more people felt that unemployment was a greater evil than inflation. Moreover, a majority of the respondents in the SRC surveys repeatedly indicated that they were hurt "little" or "not at all" by inflation and that they would not be willing to accept substantial increases in unemployment in order to halt increasing prices. 10

My own analyses of survey data from Great Britain and the United States on public aversion to unemployment and inflation supports the inferences of Katona and his associates. Space permits me to report only the general conclusions of these analyses here. 11 First, the British and American public opinion data clearly show that in the period through 1972 (which is the relevant period for the purposes of this study) solid majorities of the mass public(s) typically expressed greater aversion to unemployment than inflation. Second, popular concern about unemployment and inflation is class-related. Low and middle income and occupational status groups are more averse to unemployment than inflation, whereas, upper income and occupational status groups are more concerned about inflation than unemployment. Although the available survey evidence is by no means definitive, it does appear that the subjective preferences of class or status groups are at least roughly in accordance with their objective economic interests, insofar as these are reflected by the behavior of wages, profits, and the distribution of personal income under various unemployment/inflation macroeconomic configurations. 12

10 George Katona, "Disputing Galbraith," New York Times (December 22, 1974), and George Katona et al., Aspirations and Affluence (New York: McGraw-Hill, 1971). Many of the results from these surveys appear in annual volumes of the Survey of Consumer Finances (Ann Arbor: Survey Research Center, 1960–72). Results of surveys taken before 1960 are available as mimeo reports from the SRC.

¹¹The analyses are presented fully in the section "Public Opinion Toward Inflation and Unemployment" in Hibbs, "Economic Interest," pp. 24-40.

12 The class interests at stake in unemployment/inflation outcomes and policies show up in the policy positions taken by organized labor and capital as well as in the distribution of mass opinion. Throughout the postwar period, trade union spokesmen have invariably placed primary emphasis on the objective of full employment, while business elites have attached far more importance to price stability. A clear statement of labor's position is given by Nat Goldfinger, "Full Employment: The Neglected Poli-

Having outlined the group or class cleavages surrounding the unemployment/inflation trade-off, we now turn to the main task of this article and consider to what extent these cleavages are reflected in the economic policies pursued by governments of different political orientations.

Macroeconomic Policies and Outcomes: International Comparisons

The evidence reviewed in the previous section suggests that the objective economic interests and subjective preferences of lower income, blue-collar groups differ markedly visà-vis the unemployment/inflation trade-off from those of higher income, white-collar groups. Although the importance of socioeconomic status as a basis of electoral cleavage varies substantially across party systems, the mass constituencies of political parties in most advanced industrial societies are distinguished to a significant extent by class, income, and related socioeconomic characteristics. Even a casual examination of the historical record makes it clear that differences in the economic interests and preferences of income and occupational groups are reflected in the contrasting positions toward various economic goals associated with left- and right-wing political parties. (This is not to suggest, incidentally, that the influence linkages between mass constituencies and party elites are unidirectional.) Hence, labor-oriented, working-class-based Socialist and Labor parties typically attach far greater importance to full employment than to inflation, whereas business-oriented, upper middleclass-based Conservative parties generally assign higher priority to price stability than to unemployment. The implied preferences or issue positions of political parties (or tendances), arrayed along the traditional left-right spectrum, are outlined more systematically in Table 1. The table is adapted from a study by Kirschen et al. and is based on questionnaires administered to experts in eight industrial societies.13 The most important thing to notice

cy?" The American Federationist, 79 (November 1972). Data on corporate thinking on the inflation and unemployment issues is presented in L. Silk and D. Vogel, Profits and Principles: The Social and Political Thinking of American Businessmen (New York: Simon and Schuster, 1977).

¹³E. S. Kirschen et al., Economic Policy In Our Time, Vol. I (Amsterdam: North-Holland, 1964). With the exception of the balance of payments issue (the importance of which depends critically on the inter-

in the table is the reversal in the relative preferences of the parties regarding various economic goals as one moves from left to right across the political spectrum. In particular, notice that the party preferences concerning unemployment and inflation are consistent with the class-related cleavages surrounding these issues that were identified previously.

Since political authorities in the post-Keynesian age have considerable influence on macroeconomic outcomes, we would expect to observe (ceteris paribus, of course) a relatively low unemployment-high inflation macroeconomic configuration under leftist regimes and conversely under rightist regimes. Highly aggregated, cross-national evidence supporting this proposition appears in Figure 1, which shows a Phillips curve-like scatterplot of the average rates of unemployment and inflation over the 1960 to 1969 period in 12 industrial societies. The vertical and horizontal axes in this Figure identify the median average rates of unemployment and inflation, respectively. Five of the six nations enjoying an average level of unemployment below the West European-North Ameri-

national economic position of a given nation), the positions attributed to the various tendances were homogeneous across countries. For a similar scheme, see Bruno Frey and Lawrence J. Lau, "Towards a Mathematical Model of Government Behaviour," Zeitschrift für Nationalökonomie, 28 (1968), 355–80.

can median (i.e., the nations to the left of the vertical axis) are countries with large Socialist or Social Democratic parties (closely linked to organized labor) that have governed for much or most of the time since World War II. Looking at the postwar period as a whole, Socialist parties have been in power (or have shared power as members of coalition governments) for the entire period in Sweden, for the bulk of the period in Denmark, Finland, and Norway, and for about two-thirds of the period in the Netherlands. As one would anticipate from the Phillips curve (inverse association of unemployment and inflation), the majority of the nations' lying below the unemployment median have on the average experienced abovemedian rates of inflation. The principal exception to these generalizations is West Germany, which has been governed for most of the postwar period by the conservative CDU party and has experienced both low unemployment and low rates of inflation.

With the exception of Belgium and to a lesser extent the United Kingdom, the governments of all nations in Figure 1 falling above (i.e., to the right of) the average unemployment median have been dominated by center or right-wing political parties. In the United States and Canada, where problems of deficient aggregate demand are chronic, unemployment rates have consistently been the highest in the Western industrial world. Neither of these countries has politically important Socialist or

Table 1. Preferences of Political Parties in Advanced Industrial Societies Regarding Various Economic Goals^a

	Socialist-Labor	Center .	Conservatives
	Full Employment		· Price Stability
ø.	Equalization of Income Distribution		
of Goals		Price Stability	
of	Economic Expansion		
ခ္ခ		Economic Expansion	Balance of Payments Equilibrium
ortz		Full Employment	
Impc		Equalization of Income Distribution	
sing	Price Stability		Economic Expansion
Decreasing Importance		Balance of Payments Equilibrium	Full Employment
	Balance of Payments Equilibrium		
	•		Equalization of Income Distribution

^aBased on Kirschen et al., 1964,

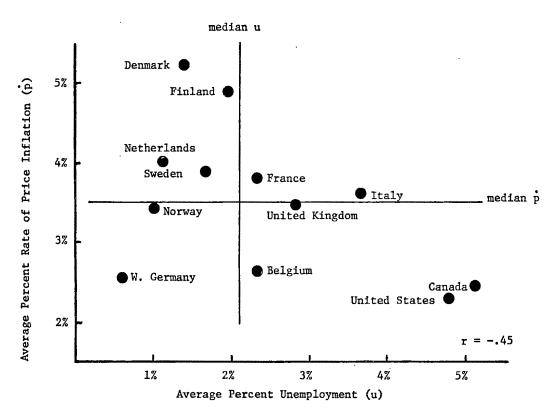


Figure 1. Mean Inflation and Unemployment, 1960-1969, in 12 West European and North American Nations

Source: Unemployment data for Canada, France, Italy, Sweden, U.K., and W. Germany are adjusted to the U.S. definition and are from Constance Sorrentino, "Unemployment in the United States and Seven Foreign Countries," Monthly Labor Review, 93 (September 1970), 12-23. All other data are from I.L.O., Yearbook of Labor Statistics, various volumes.

Labor parties, 14 and centrist or rightist governments have ruled throughout the postwar era.

The Communist and Socialist political blocs in France and Italy have commanded a sizable share of the vote in all postwar elections, but aside from the governments of national unity in the immediate postwar period and the marginal representation of the French and Italian socialists in various Center coalition governments, they have been largely frozen out of positions of executive power.¹⁵ Belgium deviates from

annual data shows that unemplcyment was lower and inflation higher during Mollet's government (as well as during the subsequent Center-Left government of Bourgès-Maunoury) than during the right-wing Gaullist governments of the late 1950s and 1960s. The Center-Left governments of the middle 1950s clearly assigned higher priority to full employment and expansion than the Gaullist regime, which pursued policies geared to disinflation and economic "stabilization." As a result, France's location on the "international Phillips curve" has changed dramatically. (Contrast the data shown in Figure 1 to a similar display of average rates of inflation and unemployment reported by D. Smyth, "Unemployment and Inflation: A Cross-Country Analysis of the Phillips Curve," American Economic Review, 61 (June 1971), 426–29, for the period 1950–1960.) Of course France's entry into the EEC in 1958 increased the importance of the external balance-of-payments constraint during the Fifth Republic. However, the deflationary policies of the Gaullist governments must be attributed to some extent to the priorities of the regime. See M Maclennan et al., Economic Planning and Policies in Britain, France and Germany (New York: Praeger, 1968).

¹⁴Canada's New Democratic Party, a genuinely socialist party with close connections to organized labor, has exhibited increasing political vitality in recent years (capturing several provincial governments) but remains at this writing a "minor" party with little influence on national policy.

¹⁵ Actually there was one brief period of Socialistled rule in France after 1951: Guy Mollet's government of February 1956 to May 1957. Analysis of

the general pattern in that the Socialists have ruled (in coalition with other parties) for just over half of the postwar years, and the average rate of unemployment stands just above the West European-North American median, However, unemployment has on the average been lower (and the rate of inflation on the average higher) during the tenure of Socialist coalition governments than during periods of Center-Right rule. Great Britain also constitutes something of an exception. The Labour and Conservative parties have alternated in power (although the Conservatives ruled continuously from 1951 to 1964) and the average unemployment rate is above the median. The mean British unemployment rate, however, is substantially less than the average rates prevailing in the United States, Canada, and Italy.

Taken as a whole, the evidence in Figure 1 indicates that the "revealed preference" of governments of the nations in the northwest quadrant of the figure has been for relatively low unemployment at the expense of high inflation, whereas the opposite appears to be true for governments of the countries in the southeast quadrant of the figure. This is rein-

forced by Figures 2 and 3; which show simple scatterplots of the average rates of inflation and unemployment in relation to average government participation (percentage of postwar years in the executive branch) of Socialist and Labor parties. These plots merely provide a slightly different illustration of the earlier argument. Nations in which Social Democratic and Labor parties have governed for most or much of the postwar period have generally experienced high rates of inflation. Conversely, low rates of inflation have prevailed in countries where center and right-wing parties have dominated the policy-making process (Figure 2). The reverse is true of the association between average unemployment and average Socialist-Labor executive participation. Comparatively low rates of unemployment characterize systems in which left-wing parties have regularly controlled the executive, and high unemployment rates have been typical in systems governed primarily by center and right-wing parties.16

¹⁶Since the macroeconomic policies (and outcomes) of the 1960s were to a significant extent

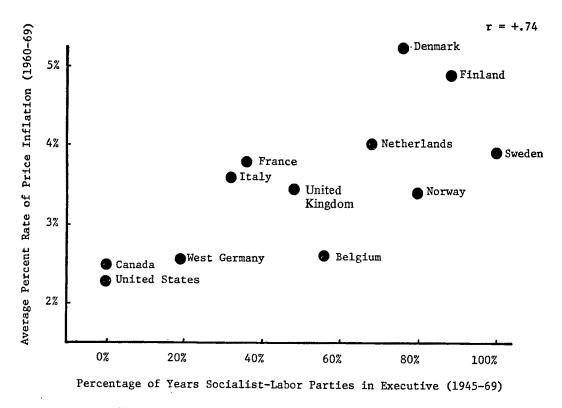


Figure 2. Mean Inflation and Socialist-Labor Executive Participation in 12 West European and North American Nations

If a common unemployment/inflation trade-off (or "menu of policy choices") confronted each of the nations appearing in Figures 1 through 3, the cross-national variation in unemployment/inflation configurations might be attributed primarily to systematic differences in the short-run monetary and fiscal policies pursued by political authorities. 17 The

influenced by the performance record of the late 1940s and 1950s (especially in countries in which Social Democratic-led governments managed to maintain full employment after the war), the Socialist-Labor participation rate has been calculated over the entire postwar period (1945–69) rather than for the years 1960 to 1969 alone.

17This has been suggested, for example, in reference to the difference in unemployment rates between

modest but inverse relationship between the average rates of inflation and unemployment (the correlation is -.45) suggests that there is some merit in this interpretation. Rates of unemployment even approaching those typical of Canada and the United States are simply not politically feasible or acceptable in countries with large Socialist-Labor parties that are frequently governed by the Left. Prior economic performance and continued emphasis on low unemployment in political discourse has gen-

North America and Western Europe, by Albert Rees, "The Phillips Curve as a Menu for Policy Choice," *Economica*, 37 (August 1970), 227–38. Monetary policy instruments include interest rates and the supply of credit and money. Fiscal policy instruments include taxation and public spending.

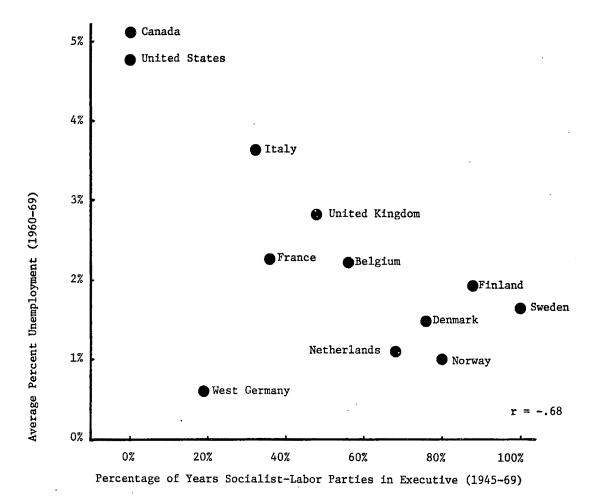


Figure 3. Mean Unemployment and Socialist-Labor Executive Participation in 12 West European and North American Nations

erated widespread public expectations of sustained full employment, which insures that short-run macroeconomic policy is geared to preserving the low unemployment, continuous inflation pattern observed in the northwest quadrant of Figure 1.

However, empirical time-series studies have established that unemployment/inflation tradeoffs exhibit considerable cross-national diversity. A comparative investigation by Flanagan, for example, has shown that over the 1951 to 1968 period the Phillips curve trade-off available to political authorities in the United States was less favorable than the trade-off curves for Great Britain and Sweden. 18 Evidence of this sort indicates that international differences in institutional and structural arrangements underlie, at least to some extent, the cross-national variation in aggregate, equilibrium outcomes, depicted in Figures 1 through 3. In particular, the enormous emphasis placed on full employment in nations with large Socialist-Labor parties has led to the introduction of centralized economic planning and coordination, extensive public sector investment, and, perhaps most important, a wide range of labor market and manpower policies that are designed to minimize the incidence and duration of unemployment.19 Hence, the critical historical role of the Left in shaping longer-run policies and institutional arrangements must also be considered in order to account adequately for cross-national variation in unemployment/inflation configurations.

Macroeconomic Policies and Outcomes: Time-Series Analyses

Thus far only static, aggregated evidence has been presented in support of the hypothesis

18 Robert J. Flanagan, "The U.S. Phillips Curve and International Unemployment Rate Differentials," American Economic Review, 63 (1973), 114-31. For additional evidence on cross-national variation in Phillips curves, see Ronald G. Bodkin et al., Price Stability and High Employment: The Options for Canadian Economic Policy (Ottawa: Economic Council of Canada, 1967).

19Of course leftist governments have not been equally effective in this regard. For example, British Labour governments have been much less imaginative in developing macroeconomic policy (and have pursued a more centrist political strategy) than Swedish Social Democratic administrations. See the perceptive comparative analysis in Andrew Martin, The Politics of Economic Policy in the U.S.: A Tentative View from a Comparative Perspective (Beverly Hills: Sage Professional Paper in Comparative Politics, 1973). The best treatment in English of the archetypal Swedish model is probably A. Lindbeck, Swedish Economic Policy (Berkeley: University of California Press, 1974).

that macroeconomic outcomes systematically covary with the political orientation of governments. A dynamic country-by-country analysis of postwar time-series data might provide a more convincing test of this general proposition except for the major constraint that many advanced industrial societies have simply not experienced very much partisan variation (defined in the traditional left-right sense) in their governments.

Time-series analyses of unemployment rates have been undertaken for Great Britain and the United States. Great Britain is an ideal candidate for dynamic analysis in that national political power has oscillated between the working class-based Labour party and the middle class-based Conservative party. In comparison to the British Labour and Conservative parties, the two dominant American political parties are less distant ideologically and have more heterogeneous social bases.²⁰ Nonetheless, the Democratic party has relatively close connections to organized labor and lower income and occupational status groups, while the Republican party is generally viewed as being more responsive to the interests of capital or business and upper income and occupational status groups.²¹ Other things being equal, we would therefore expect to observe a downward movement in the unemployment rate during the tenure of Democratic and Labour governments and an upward movement in the unemployment rate during periods of Republican and Conservative rule in the United States and Great Britain, respectively.

In order to evaluate this proposition rigorously, we need a model that permits estimation of the hypothesized effects of government macroeconomic policies on the unemployment rate, net of trends, cycles and stochastic fluctuation in the unemployment time-series observations. In contrast to more conventional approaches, I have used the "intervention analysis" scheme of Box, Jenkins and Tiao.²²

²⁰ See, for example, Robert Alford, Party and Society (Chicago: Rand-McNally, 1963).

²¹For an argument that organized labor and the Democratic party in the United States are interpenetrated in a way that is at least partially equivalent to Socialist party-labor union alliances in much of Western Europe, see J. D. Greenstone, Labor in American Politics (New York: Alfred A. Knopf, 1969).

²²See G. E. P. Box and G. M. Jenkins, *Time Series Analysis; Forecasting and Control* (San Francisco: Holden-Day, 1970), part III; and G. E. P. Box and G. C. Tiao, "Intervention Analysis with Applications to Economic and Environmental Problems," *Journal of the American Statistical Association*, 70 (March 1975), 70–79. The scheme of Box, Jenkins, and Tiao

Box-Jenkins or Box-Tiao models represent time-series observations on the endogenous variable (in this case unemployment) as the realization of a linear stochastic process of autoregressive, moving average, or mixed, autoregressive-moving average form. The autoregressive-moving average (ARMA) model provides a stochastic benchmark against which intervention-induced changes in the slope and/ or level of the endogenous time-series are assessed. Intervention occurrences (in this case partisan changes in government) are represented by binary variables (0, 1) or by related coding schemes (e.g., +1, -1), and the effects of interventions are specified by simple "transfer functions."

Regarding the problem at hand, the most plausible hypothesis is that shifts in the political orientation of governments during the postwar period in Great Britain and the United States will be associated with gradual changes in the net levels of the British and American unemployment rates. The intervention models therefore take the general form

$$U_{t} = \frac{\beta}{1 - \delta L} G_{t-1} + \frac{\theta_{0} + \theta_{q}(L)}{\phi_{p}(L)(1 - L)^{d}} a_{t}$$
 (1)

where: U_t = the percentage of the civilian labor force unemployed (quarterly data);

G_t = +1 during Labour or Democratic administrations; -1 during Conservative or Republican administrations;

 β, δ = parameters describing the effects of shifts in G_t on U_t ;

 $L = \text{lag operator such that } LU_t = U_{t-1}, L^iU_t = U_{t-i}, \text{etc};$

 $(1-L)^d = a$ lag difference operator such that $(1-L)U_t = U_t - U_{t-1}, (1-L)^2 = (1-2L+L^2)U_t = U_t - 2U_{t-1} + U_{t-2},$ etc.;

$$\theta_q(L) = 1 - \theta_1 L - \theta_2 L^2 + \ldots + \theta_q L^q$$

$$\phi_p(L) = 1 - \phi_2 L - \phi_2 L^2 - \ldots - \phi_p L^p$$
are moving average and autoregressive polynomials in L of order p and q , respectively;

is contrasted with the conventional structural equation approach in Douglas A. Hibbs, Jr., "On Analyzing the Effects of Policy Interventions: Box-Jenkins and Box-Tiao vs. Structural Equation Models," in *Sociological Methodology 1977*, ed. D. Heise (San Francisco: Jossey Bass, 1977), pp. 137–79.

- θ_0 = a constant indexing a deterministic time trend of degree d in U_t ;
- $a_t = a$ sequence of independently distributed random variables with mean zero and variance σ_a^2 .

Equation (1) simply expresses the proposition that-net of trends, cycles, and stochastic fluctuation in the unemployment time-series, which are captured by the autoregressive-moving average terms in the model²³-we anticipate a gradual rise in unemployment levels under Conservative and Republican governments and, conversely, a gradual decline in unemployment levels during Labour and Democratic administrations. If a partisan change in government, occurring, for example, at time n, was sustained indefinitely (e.g., $G_t = +1$ for all $t \ge n$), the unemployment rate would eventually fluctuate about the steady state or equilibrium value $\beta/1-\delta$. The rate of adjustment to the new equilibrium depends on the magnitude of the dynamic parameter δ . Since we assume that the macroeconomic policies of a new government are not introduced or implemented instantaneously, the intervention term G_t is specified with a one period (quarter) delay or lag.²⁴

The British Unemployment Model. The first step in the model building process is to develop a preliminary specification of the stochastic or ARMA component of equation (1) by analyzing the sample autocorrelation and partial autocorrelation functions of the endogenous variable (i.e., unemployment).²⁵ The sample autocorrelation function 26 r_k for seasonally unadjusted quarterly observations on the Brit-

²³The cyclical or seasonal component of the model is not represented explicitly by the ARMA terms of eq. (1).

eq. (1). 24 The one quarter lag on G_t may be too short, especially for the United States. However, since the intervention function allows U to respond gradually to shifts in G, this is not an important problem.

²⁵The ARMA model building process is systematically reviewed in Hibbs, "On Analyzing Policy Interventions," and developed in great detail by Box and Jenkins, *Time Series Analysis*.

²⁶Sample autocorrelations are simply the correlations between observations separated k periods in time and are given by:

$$r_k = \frac{\sum (U_t - \overline{U}_t) (U_{t-k} - \overline{U}_t)}{\sum (U_t - \overline{U}_t)^2} \quad r = 1, 2, \dots$$

Thus r_1 denotes the correlation between U_t and U_{t-1} ; r_2 denotes the correlation between U_t and U_{t-2} ; and so on.

ish unemployment rate over the 1948(1) to 1972(4) period is graphed in Figure 4.²⁷ The sample autocorrelations decay steadily as the lag k increases, which indicates that a low-order autoregressive process is compatible with the British unemployment observations. Since the partial autocorrelations (which are not reported here) are insignificant for k > 1, we tentatively entertain a first order autoregressive specification:

$$U_t = \phi_1 U_{t-1} + e_t$$
, or (2)
 $(1-\phi_1 L)U_t = e_t$.

Figure 5 presents the sample autocorrelations of the residuals $\hat{e_t}$, that is the autocorrelations of the transformed data $U_t - \phi_1 U_{t-1}$. The autocorrelations exhibit distinct peaks every fourth quarter—at k = 4, 8, 12, 16...—which suggests a strong seasonal dependence between unemployment rates of the same quarter in different years. This depen-

27The British unemployment data (wholly unemployed as a percentage of the civilian labor force) were obtained from the *Ministry of Labour Gazette*, various issues. In view of the unprecedented exogenously imposed economic crisis facing advanced industrial societies since 1973, the time series analyses are intentionally not taken beyond the fourth quarter of 1972.

dence comes as no surprise, since it is well known that unemployment is influenced by seasonal factors and the British data were not available in seasonally adjusted form. The seasonal dependence identified in Figure 5 shows no tendency to die out as the lag k increases, and therefore, four-quarter, seasonal differencing is called for. Hence we propose the model:

$$(1-L^4)e_t = \theta_0 + a_t$$
, or (3)
$$e_t = \frac{\theta_0 + a_t}{(1-L^4)}.$$

Substituting (3) into (2) yields the following expression for the stochastic component of the general intervention scheme given in (1):

$$(1-\phi_1 L)U_t = \frac{\theta_0 + a_t}{(1-L^4)}, \text{ or}$$

$$U_t = \frac{\theta_0 + a_t}{(1-L^4)(1-\phi_1 L)}.$$
(4)

Adjoining (4) to the intervention function proposed in (1) to represent the hypothesized net impact of partisan changes in government on the unemployment level, we arrive at the equation:

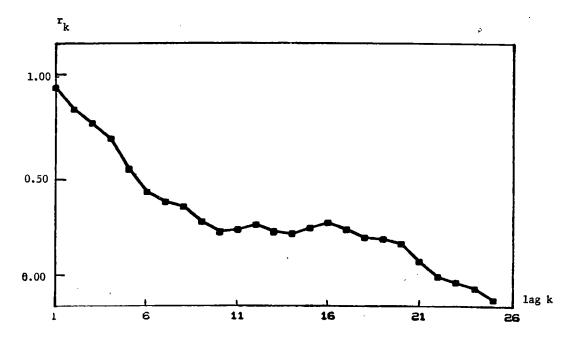


Figure 4. Sample Autocorrelation Function of the British Unemployment Rate Data, 1948(1)-1972(4)

$$U_{t} = \frac{\beta}{1 - \delta L} G_{t-1} + \frac{\theta_{0} + a_{t}}{(1 - L^{4})(1 - \phi_{1} L)}$$
 (5)

where: U_t = the percentage of the civilian labor force wholly unemployed in Great Britain quarterly 1948(1)-1972(4);

G_t = +1 during Labour governments; -1 during Conservative governments:

and all other terms are as previously defined.

A second intervention term should be added to the British unemployment model in order to take account of an important change in the British unemployment compensation scheme which was initiated in October 1966. Until 1966, the unemployed in Great Britain received a relatively flat-rate benefit that was not tied to previous earnings. The change in the unemployment system initiated by the Labour government in 1966 provided for an "earnings-related supplement" equal to about one-third of the unemployed person's previous average weekly earnings between £9 and £30. This represented a substantial increase in benefits for most wage

earning groups.²⁸ As a result, unemployed workers were under less financial pressure to accept unattractive jobs and presumably spent more time in searching for new employment. It is therefore widely believed that the new compensation scheme increased the rate and duration of unemployment.²⁹ Thus we define a

²⁸For example, it is estimated that the earnings-related benefits increased the unemployment income of a typical married male worker with two children from about 40 percent to 60 percent of average employment income. See OECD, Manpower Policy in the United Kingdom (Paris: OECD Publications, 1970).

²⁹Unfortunately the picture is complicated by the fact that a number of other macroeconomic policy changes were implemented during the 1965–67 period. These policy changes are reviewed by Bowers et al., in "The Change in the Relationship Between Unemployment and Earnings Increases: A Review of Some Possible Explanations," National Institute Economic Review (November 1970), 44–63. However, the survey-based analysis of D. MacKay and G. Reid in "Redundancy, Unemployment and Manpower Policy," Economic Journal (December 1972), 1256–72, leaves little doubt that the new compensation law had a significant effect on the duration (and thus the rate) of unemployment. Also see the discussion by M. Feldstein, "The Economics of the New Unemployment," Public Interest, 33 (Fall 1973), 3–42.

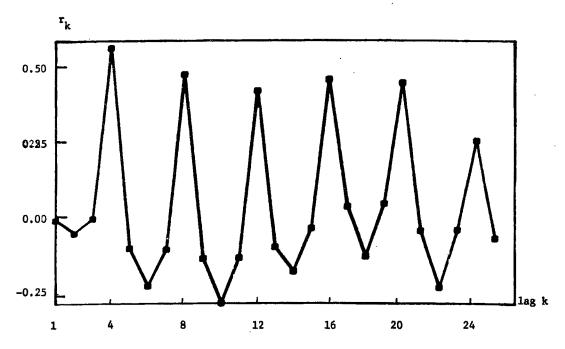


Figure 5. Sample Autocorrelation Function of the Transformed British Unemployment Rate Data $(1-\phi_1L)$ U₁, 1948(1)-1972(4)

new variable C_t taking a value of 0 prior to 1966(4) and a value of +1 otherwise, and specify the revised model:

$$U_{t} = \frac{\beta_{1}}{1 - \delta_{1}L} G_{t-1} + \frac{\beta_{2}}{1 - \delta_{2}L} C_{t} + \frac{\theta_{0} + a_{t}}{(1 - L^{4})(1 - \phi_{1}L)}$$
(6)

The revised model in (6) allows the introduction of the new unemployment compensation system as well as unrelated interparty differences in macroeconomic policy to alter gradually the level of British unemployment.

Table 2 reports the estimation results for equation (6). ³⁰ All coefficients (except the constant or trend term θ_0) are substantially larger than their estimated standard errors and therefore are significant by conventional statistical criteria. Before considering the implications of these estimates, let us first evaluate the adequacy of the fitted model. Figure 6 shows the actual and predicted levels of the unemployment time series. ³¹ The predicted unem-

30 The models in this section were estimated with Kent D. Wall's ERSF program, which provides Full Information Maximum Likelihood estimates of Rational Distributed Lag Structural Form equations. Details are given in Wall, "FIML Estimation of Rational Distributed Lag Structural Form Models," Working Paper No. 77 (Cambridge: National Bureau of Economic Research, Inc., March, 1975).

³¹The predicted level data are obtained by summing the predicted four-quarter difference series, i.e.,

$$\hat{U_t} = U_0 + \sum_{t} (1 - L^4) \hat{U_t}.$$

The summation operator Σ is the inverse of the difference operator (1-L) in the same way that integration is the inverse of differentiation in continuous time problems.

ployment observations track the actual data quite well, which of course is expected in view of the highly significant parameter estimates and small residual variance reported in Table 2. Diagnostic checks applied to the residuals provide more convincing evidence of the model's adequacy. Figure 7 presents the residual autocorrelations $r_k(\hat{a_t})$ for lags 1 through 25. The autocorrelations exhibit no systematic patterns and, except for k=4, fall within the approximate ± 2 standard deviation limits. The mean of the residuals is $\bar{a}=.0000003$ and the estimated standard error $\sigma \frac{2}{a}=.023$. The sample evidence strongly suggests therefore that the a_t are independently distributed random variates with zero means.

Returning to the parameter estimates in Table 2, interest centers on the intervention coefficients $\hat{\beta}$ and $\hat{\delta}$. The coefficients associated with the unemployment compensation dummy variable C_t ($\hat{\beta}_2$, $\hat{\delta}_2$) indicate that the additional unemployment benefits available since October 1966 produced a net increase of about 0.86 percent in the equilibrium level of unemployment, that is,

$$+\frac{\hat{\beta}_2}{1-\hat{\delta}_2}=\frac{+.511}{1-.407}=0.86.$$

In view of the fact that the dynamic response parameter δ_2 = .407, the steady state effect of

³²The lag 4 autocorrelation is of course significant and therefore the model might be improved by specifying $a_t = (1 - \theta_4 L^4) v_t$ where the v_t are $N(0, \sigma_v^2)$. Since the k=4 autocorrelation was essentially induced by the seasonal differencing (which overcompensates for the four-quarter seasonal dependency), and we are primarily interested in predicting the level unemployment series, modification of the model in this way is not advantageous.

Table 2. Estimation Results for the British Unemployment Rate Model (Eq. 6)

	Parameter Estimates	Standard Errors
G_{t-1}	$\hat{\beta}_1 =094$.035
· •	$\beta_1 =094$ $\delta_1 = +.692$.118
C_t	$\hat{\beta}_2 = +.511$.115
•	$\hat{\beta}_2 = +.511$ $\delta_2 = +.407$.228
Trend (4 quarter)	$\hat{\theta_0}$ = + .002	.023
Autoregressive	$\hat{\phi}_1 = +.773$.071
**************************************	Residual Variance,	$R^2 = .95^8$
	$\hat{\sigma}_a^2 = .045$	

 $^{^3}$ The R^2 reported here pertains to the level data rather than to the four-quarter difference data. The four-quarter difference R^2 is .85.

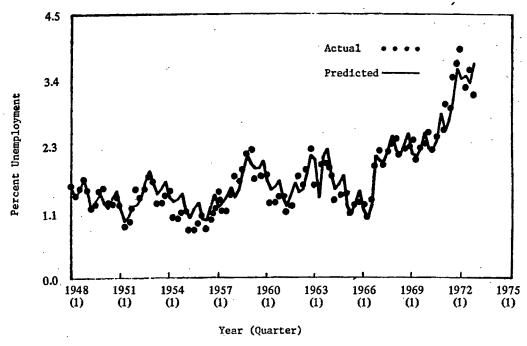


Figure 6. Actual and Predicted Values from the British Unemployment Rate Model (Eq. 6)

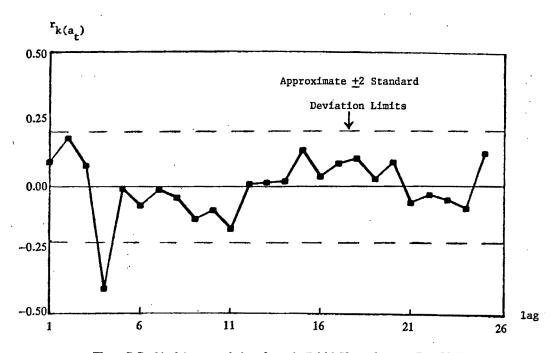


Figure 7. Residual Autocorrelations from the British Unemployment Rate Model

0.86 percent was fully realized rather quickly—after only four or five quarters. However, effects associated with the C_t term, although sizable, are only of incidental interest in this study.

More important for our purposes are the maximum likelihood estimates of β_1 and δ_1 which clearly support our initial proposition concerning the impact of partisan change on the British unemployment rate. Net of the effects attributed to the new unemployment compensation law, and independent of trends, seasonal dependencies, and stochastic fluctuation in the time series, the unemployment rate appears to be driven downward during the tenure of Labour governments and to move upward during periods of Conservative rule. The estimated steady state effects are ± 0.31 percent, that is:

$$\pm \frac{\hat{\beta}_1}{1 - \hat{\delta}_1} = \pm \frac{.094}{1 - .692} = \pm 0.31,$$

which implies a difference of about 0.62 percent between the equilibrium unemployment levels associated with Labour and Conservative governments. Holding fixed the C_t variable and the stochastic ARMA terms in the model, we see that the expression U_t =

$$\frac{\hat{\beta}_1}{1 - \hat{\delta}_1 L} G_{t-1} \text{ implies } U_t = \hat{\delta}_1 U_{t-1} + \hat{\beta}_1 G_{t-1},$$
 which upon repeated substitution gives:

$$U_{t} = \hat{\delta}_{1}^{t} U_{0} + \hat{\beta}_{1} \sum_{i=0}^{t-1} \hat{\delta}_{1}^{i} G_{t-1-i}.$$
 (7)

Imposing the arbitrary initial condition $U_0=0$ and applying the coefficient estimates $\hat{\beta}_1=-.094$, $\delta_1=.692$, we obtain the dynamic time paths of the unemployment rate that can be attributed to Labour and Conservative macroeconomic policies by simulating (7) for G_t held at +1 and -1, respectively. Figure 8 depicts the unemployment time paths for regimes of 20 quarters (5 years) duration. Notice that the steady state values of ± 0.31 percent are fully realized after about 16 quarters or 4 years.

An interparty difference of just over one-half of one percent in government-induced unemployment levels may seem small by American standards, but, if evaluated against Great Britain's average postwar unemployment rate of 1.67 percent, it is by no means trivial. Applied to the British civilian labor force, which has averaged 24.1 million workers during the postwar period, the effects graphed in Figure 8 translate into about 149,000 jobs. Since British unemployment data are compiled by the regis-

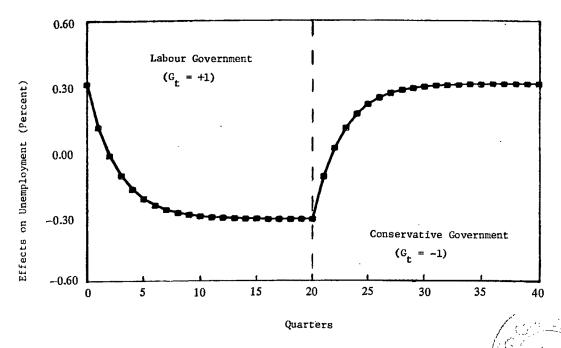


Figure 8. Simulated Net Effects of Labour and Conservative Governments on the Unemployment Rate

tration method, the measured unemployment rate tends to be biased downward relative to that of the United States, which is based on labor force survey data. Adjusting the British data to the American definition therefore permits more accurate comparisons to be made with the U.S. experience. Myers estimates the adjustment factor to be 1.51.³³ Applying this to the British data yields an interparty steady state difference of 0.94 percent, or about 226,000 jobs.

Nonetheless, the estimated effect of Labour versus Conservative macroeconomic policies on the equilibrium level of unemployment is perhaps smaller than one might have anticipated from the earlier discussion of left-to-right cleavages regarding various economic goals. Indeed the ideological distance between the Labour and the Conservative parties on the full employment issue is undoubtedly not as great as that implied by the general scheme introduced previously in Table 1. Throughout the postwar period the Conservatives have made great efforts to disassociate themselves from the mass

33R. J. Meyers, "The Unemployment Problem: What We Can Learn from European Experience," in *Measuring Employment and Unemployment* by the Joint Economic Committee of the U.S. Congress (Washington, D.C.: Government Printing Office, 1963).

unemployment of the 1930s by repeatedly emphasizing their commitment to the full employment goal, although in practice it was sometimes viewed as necessary to induce increases in unemployment in order to fight inflation. However, it should be recognized that, unlike the United States, Great Britain is very much an open economy and the macroeconomic policies of both Labour and Conservative governments have been severely constrained by the necessity of maintaining a satisfactory external trade balance. Political authorities of both parties had to insure that the country did not inflate at a rate exceeding that of its principal trading partners in order to maintain the competitiveness of British exports in world markets. In view of the international economic constraints facing all British governments, the estimated interparty difference of 0.62 percent (0.94 percent adjusted to U.S. concepts) does not appear quite as modest in magnitude.

The U.S. Unemployment Model. The impact of Democratic versus Republican administrations on the U.S. unemployment rate is also estimated by developing an ARMA-intervention model. The model building procedure is the same as that outlined in the course of the British analysis. Figure 9 shows the sample autocorrelation function for seasonally adjusted

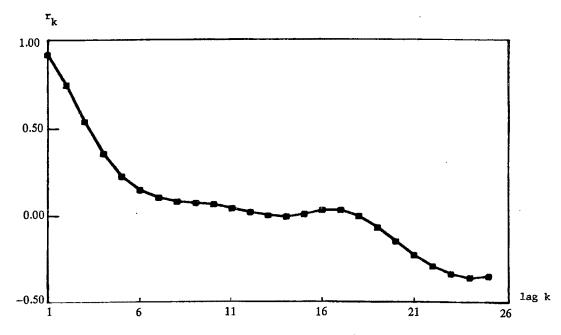


Figure 9. Sample Autocorrelation Function of the U.S. Unemployment Rate Data, 1948(1)-1972(4)

quarterly observations on the U.S. employment rate over the period 1948(1)-1972(4). The autocorrelations exhibit mild oscillations and decay as the lag k increases—properties which are characteristic of a low-order autoregressive process. Partial autocorrelations (which are not reported here) are significant for $k \leq 2$, and therefore we propose a second-order process for the stochastic component of the intervention model: 34

$$U_{t} = \theta_{0} + \phi_{1} U_{t-1} + \phi_{2} U_{t-2} + a_{t}, \text{ or }$$

$$U_{t} = \frac{\theta_{0} + a_{t}}{1 - \phi_{1} L - \phi_{2} L^{2}}.$$
(8)

Adjoining (8) to the intervention function introduced previously in (1) yields the estimating equation:

$$U_{t} = \frac{\beta}{1 - \delta L} G_{t-1} + \frac{\theta_{0} + a_{t}}{1 - \phi_{1} L - \phi_{2} L^{2}}$$
 (9)

where: U_t = the percentage of the civilian labor force unemployed in the U.S. quarterly 1948(1)-1972(4);

 G_t = +1 during Democratic administrations, -1 during Republican administrations;

and other terms are as previously defined.

In its present form, the model in equation (9) is unlikely to provide a very good estimate

34Equation (8) is nearly identical to the model developed by C. R. Nelson for quarterly U.S. unemployment data over the period 1948(1)–1966(4). Nelson's model, incidentally, outperformed the MIT-FRB-Penn econometric model in short-term forecasting experiments. "The Predictive Performance of the FRB-MIT-PENN Model of the U.S. Economy," American Economic Review (1972), 902–17.

of the net effect of Democratic versus Republican macroeconomic policies on the U.S. unemployment rate. An important omitted variable, which is not likely to be captured by the autoregressive terms in the model, is American intervention in the Korean and Vietnamese civil wars. The enormous fiscal stimulus to the domestic economy (not to mention the sizable number of young men withdrawn from the civilian labor force) generated by American participation in these conflicts shows up clearly in the steadily declining unemployment rates of the war years. (The same can of course be said about the contribution of World War II to the recovery from the Great Depression.) Indeed the United States experienced its lowest postwar unemployment rates during the peaks of these wars. Since American involvement in the Korean and Vietnamese conflicts occurred during (covaried with) Democratic administrations, it is necessary to include an additional "war" term in the model in order to disentangle the party effects of interest from the war effect. Therefore we introduce an additional variable W_t taking a value of +1 during the Korean and Vietnamese wars and a value of 0 otherwise, and specify the revised model

$$U_{t} = \frac{\beta_{1}}{1 - \delta_{1}L} G_{t-1} + \frac{\beta_{2}}{1 - \delta_{2}L} W_{t} + \frac{\theta_{0} + a_{t}}{1 - \phi_{1}L - \phi_{2}L^{2}}.$$
 (10)

The specification of the W_t term in (10) is identical to that of the G_t term, except that the war variable appears without a delay or lag. The revised model therefore allows the economic stimuli accompanying American intervention in Korea and Vietnam as well as non-war-related interparty differences in macroeconomic policy to alter gradually the level of unemployment.

Estimation results for the U.S. unemployment model of equation (10) are presented in

Table 3. Estimation Results for the U.S. Unemployment Rate Model (Eq. 10)

•	Parameter Estimates	Standard Errors
G_{t-1}	$\beta_1 =071$.020
•	$\beta_1 =071$ $\delta_1 = +.974$.017
W_t	$ \hat{\beta}_2 =179 \hat{\delta}_2 = +.513 $.145
•	$\delta_2^2 = +.513$.320
Autoregressive	$ \hat{\phi}_1 = +1.49 \hat{\phi}_2 =718 $.072
	$\widehat{\phi_2} =718$.071
	Residual Variance,	$R^2 = .94$
	$\hat{\sigma}_a^2 = .085$	

Table $3.^{35}$ The coefficient estimates associated with the administration term G_{t-1} ($\hat{\beta}_1, \hat{\delta}_1$) and the estimates of the autoregressive parameters ($\hat{\phi}_1, \hat{\phi}_2$) are substantially larger than their respective standard errors and thus easily satisfy the usual criteria of statistical significance. However, the coefficients associated with the war term W_t ($\hat{\beta}_2\hat{\delta}_2$), although larger than their respective standard errors, are not significant by conventional standards, and therefore we cannot place very much confidence in these parameter estimates. 36 In view of the collinearity

³⁵Since the unemployment data did not exhibit a trend over the observation period, all variables were deviated from their means and the model was estimated without a constant term. θ_0 therefore does not appear in Table 3.

36The t ratio of $\hat{\beta}_2 = 1.23$ and of $\hat{\delta}_2 = 1.60$; both are insignificant at the .05 level. Computation of the implied dynamic response of the unemployment rate to American involvement in the Korean and Vietnamese civil wars is therefore problematic. Robert Solow has suggested to me that since the effects of both the war term and the administration term work through the actual tax, expenditure, and monetary actions of the government, the model might be better specified by constraining $\delta_1 = \delta_2$. However, estimates obtained by imposing this constraint did not alter the results reported in Table 3 and graphed below in Figure 12 appreciably: the war coefficient remained insignificant, $\delta_1 = \hat{\delta}_2 = .969$, and $\hat{\beta}_1 = .091$.

between W_t and G_t noted earlier, it is not surprising that $\hat{\beta}_2$ and $\hat{\delta}_2$ exhibit relatively large variances. However, we are primarily interested in securing an unbiased estimate of the net response of the unemployment rate to interadministration differences in macroeconomic policy, and hence the war term should be retained in the model in order to insure that the administration effect is not confounded with the war effect.

The actual and predicted values of the unemployment time series are graphed in Figure 10. The fitted values track the actual data very closely and errors do not appear to exhibit any systematic pattern. The residual autocorrelations reported in Figure 11 confirm this observation. Except for k=8, all of the $r_k(\hat{a}_t)$ fall within ± 2 standard deviations from zero, suggesting that the a_t are independently distributed random variates. Finally, the average of the residuals is $\overline{a}=-.034$ and the estimated standard error is $\sigma \frac{2}{a}=.030$, which indicates that the residual mean is not significantly different from zero.

37The negative residual autocorrelation at k=8 ($r_8(\hat{a}_t)=-.253$) indicates that there is a modest, negative two-year (8 quarter) dependency between U.S. unemployment rates. This is compatible with the

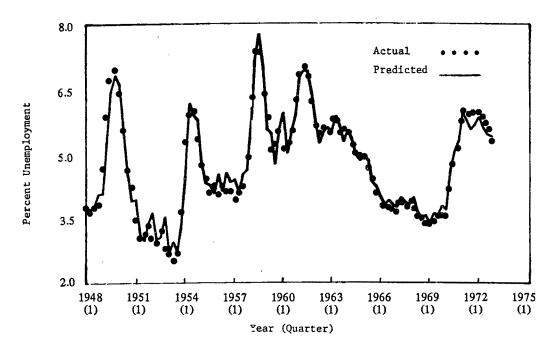


Figure 10. Actual and Predicted Values from the U.S. Unemployment Rate Model (Eq. 10)

Having established the overall adequacy of the model, we focus on the substantive implications of the administration parameters $\hat{\beta}_1$ and $\hat{\delta}_1$. The estimates reported in Table 3 give strong support to the basic hypothesis: Democratic administrations appear to engineer downward movements in the U.S. unemployment level, whereas the reverse is true of Republican administrations. The estimation results indicate that the steady state effects are on the order of ± 2.73 percent, that is:

political-electoral business cycle argument of Nordhaus, Tufte and others, in which unemployment tends to fall before Presidential elections and to rise thereafter in response to administration efforts to engineer favorable economic conditions just prior to elections and to postpone austerity measures until after elections are safely over. [Nordhaus, "The Political Business Cycle;" and Edward Tufte, Elections and Economics: Macroeconomics Under Conditions of Politi-cal Competition (Princeton, N.J.: Princeton University Press, forthcoming).] If this pattern was strong and more or less uniform across four-year presidential administrations, we should observe a sizable negative autocorrelation at k = 8 (two-year intervals) and a positive autocorrelation at k = 16 (four-year intervals). Although the focus of this study is on long-run patterns in macroeconomic policies and outcomes that distinguish left- and right-wing regimes, attempts were made to build an electoral unemployment cycle of this sort into the model. However, elaborations of the model along these lines did not yield significant results.

$$\pm \frac{\hat{\beta}_1}{1 - \hat{\delta}_1} = \pm \frac{.071}{1 - .974} = \pm 2.73,$$

which implies an interadministration difference of about 5.46 percent in the long-run, equilibrium level of unemployment. In view of the fact that the (seasonally adjusted) U.S. unemployment rate has varied between 2.6 percent and 7.4 percent during the 1948 to 1972 period, an interadministration difference of this magnitude is simply not plausible. Note, however, that this is a steady state figure, that is, it gives the implied, net difference in unemployment levels if one and then the other party were to govern nationally for an indefinitely long period of time. Since the dynamic adjustment parameter δ_1 is estimated to be .974, convergence to equilibrium is very slow and would not be fully realized until a given party had held office for more than 100 quarters or 25 years.³⁸ However, neither political party in the

 38 This is readily confirmed by evaluating the expression $\hat{\beta}_1 \sum\limits_{i=0}^{\infty} \hat{\delta}_1^i \, G_{t-1-i}$ over the index i for fixed G_t . Unlike the British results, which implied convergence to steady state after only 16 quarters, the U.S. steady state is not reached until the index i is taken to well over 100 quarters.

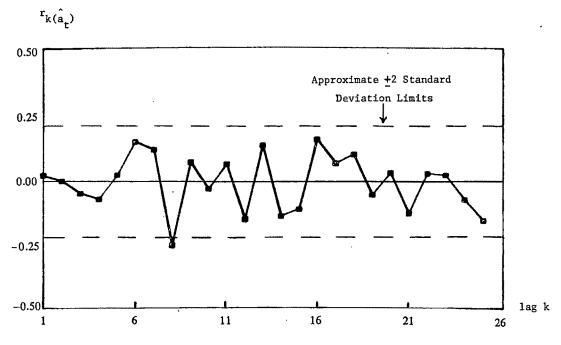


Figure 11. Residual Autocorrelations from the U.S. Unemployment Rate Model

United States has held the presidency for more than two terms in succession during the postwar period, and therefore it is sensible to restrict the interpretation of the estimation results to 32 quarters or 8 years.

Figure 12 shows the dynamic time paths of the unemployment rate implied by the G_t component of the model for Democratic and Republican administrations, respectively. ³⁹ Notice that after 32 quarters (i.e., two presidential administrations) the estimated administration effects are on the order of ± 1.18 percent and hence the interadministration difference in government induced unemployment levels is about 2.36 percent. This estimate is of course much more compatible with the postwar U.S. experience than the long-run, steady state difference

³⁹The results graphed in Figure 12 were obtained in the same way as described earlier for the British case, i.e., by simulating

$$U_{t} = \hat{\delta}_{1}^{t} U_{0} + \hat{\beta}_{1} \sum_{i=0}^{t-1} \hat{\delta}_{1}^{i} G_{t-1-i}$$

for G_T held at +1 and then -1 over regimes of 32 quarters (8 years).

of 5.46 percent reported earlier. A comparison of the U.S. results in Table 3 and Figure 12 to the corresponding results for Great Britain in Table 2 and Figure 8 also indicates that the ultimate impact of an administration on the rate of unemployment accumulates much more slowly in the United States than in Great Britain. In other words, the results suggest that the effects of government macroeconomic policies on the unemployment rate are processed much more quickly through the British system than through the American system. These inferences are entirely reasonable in view of the fact that the political and economic environment facing macroeconomic policy makers in the United States is considerably more decentralized and heterogeneous than that facing macroeconomic policy makers in the parliamentary system of Great Britain.

Discussion

The estimated interparty difference of 2.36 percent in the unemployment performance of Democratic versus Republican administrations is perhaps best illustrated historically by con-

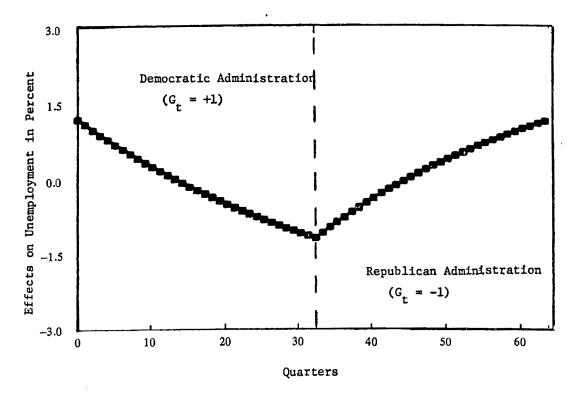


Figure 12. Simulated Net Effects of Democratic and Republican Administrations on the Unemployment Rate

trasting briefly the macroeconomic policies of the Eisenhower, Kennedy-Johnson and Nixon administrations. The principal economic goals of the Eisenhower administrations were a balanced federal budget and a reduction in the rate of inflation.⁴⁰ Despite repeated contractions in aggregate economic activity (the "Eisenhower recessions"), full employment and economic expansion never became primary goals. Indeed the emphasis on price stability and a balanced budget was so great that federal expenditures were actually decreased during the 1953–54 recession and budget outlays only barely exceeded receipts during the recession years of 1957–58 and 1960.⁴¹

The excessive caution exercised by the Eisenhower administrations in dealing with recession and the great weight placed upon price stability were of course roundly attacked by liberal Keynesian economists, organized labor, and others. In his memoirs of this period Eisenhower responded to such criticism by noting that "critics overlooked the inflationary psychology which prevailed during the midfifties and which I thought it necessary to defeat.... The anti-inflation battle is neverending, though I fear that in 1959 the public was apathetic, at least uninformed, regarding this issue."42 The consequence of this neverending battle against inflation was an economy that was chronically in stagnation and an unemployment rate that regularly exceeded six percent.

The Kennedy-Johnson administrations' posture toward recession and unemployment stands in sharp contrast to Eisenhower's. The most significant manifestation of the greater importance attached to full employment and

⁴⁰See, for example, O. Eckstein, "Economic Policy in the United States," in *Economic Policy in Our Time*, Vol. II, ed. O. Eckstein et al. (Amsterdam, North Holland, 1964), pp. 1–88; and especially H. Stein, *The Fiscal Revolution in America* (Chicago: University of Chicago Press, 1969), Ch. 11–14.

41Many analysts argue that Eisenhower's fiscal policies not only did little to combat the economic contradictions of the period but were also important causes of the 1957-58 and 1960-61 recessions. See W. Lewis, Federal Fiscal Policy in the Postwar Recessions (Washington, D.C.: The Brookings Institution, 1962); and Stein, The Fiscal Revolution in America.

⁴²D. Eisenhower, *Waging Peace*, 1956-61 (Garden City, N.Y.: Doubleday, 1965), pp. 461, 462.

economic expansion by these Democratic administrations was the 1964 tax cut. First proposed publicly by Kennedy in June 1962, introduced in Congress in January 1963, and signed into law by Johnson in February 1964, the Revenue Act of 1964 injected a ten billion dollar fiscal stimulus into a sagging economy. This represented a clear break with the budget balancing ideology of previous Republican administrations (although the rhetoric of the balanced budget lingered on) and, in view of the economic outlook at the time and the historical periodicity of U.S. recessions, undoubtedly helped prevent a serious economic contraction in 1964-65 and thereby contributed to the prolongation of the longest expansion in postwar U.S. history. Johnson defended government initiatives on the employment front by arguing that "the number 1 in priority today is more jobs. This is our dominant domestic problem and we have to face it head-on."43

The basic economic priorities associated with the Eisenhower era were reestablished during the Nixon and Ford administrations. Although Nixon-Ford macroeconomic policies were more interventionist than those of earlier Republican administrations, high employment once again was sacrificed for the sake of restraining inflation. It is generally agreed that the 1970-71 recession was deliberately induced by the Nixon administration to check inflation, though this policy was later jettisoned in an attempt to stimulate a pre-election boom. In most respects the short-lived Ford administration was a replay of the Eisenhower years. The macroeconomic game-plan called for running the economy at considerable "slack" to reverse "inflationary expectations," and repeated attempts by the Democratic Congress to pass measures promoting a more rapid economic expansion were vigorously opposed.

Macroeconomic outcomes, then, are not altogether endogenous to the economy, but obviously are influenced to a significant extent by long- and short-term political choices. The real winners of elections are perhaps best determined by examining the policy consequences of partisan change rather than by simply tallying the votes.

⁴³Cited in F. R. Dulles, Labor in America (New York: Crowell 1966), p. 394.

A Critique of "Democratic Administration" and Its Supporting Ideation*

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This paper analyzes Vincent Ostrom's major work, The Intellectual Crisis in American Public Administration, which he offers as providing paradigmatic direction for public administration and political science. The analysis urges caution as to that theory's status, especially from five analytic perspectives. Basically, attention is directed at the methodology or mode of inquiry associated with Ostrom's grounding of his argument in public choice theory, with special attention to the role of values. The adequacy of major assumptions of Ostrom's argument as descriptions of reality also is evaluated. Moreover, the critical lack of content in several key concepts is established. In addition, the analysis shows how opposite and simultaneous courses of action are implied by the argument. Finally, attention is directed at how Ostrom's argument can lead to unexpected consequences, even some that are opposite those effects Ostrom intends.

In the ongoing ferment about leading ideas to guide public administration, attention has focused on Vincent Ostrom's effort to make "democratic administration" the paradigm of the 1970s. This essay acknowledges Ostrom's centrality, and also provides some of the critical treatment that his ideas require. In dealing with Ostrom's "democratic administration," this critique necessarily refers to the broad ideational infrastructure of public choice theory on which Ostrom relies.

Given the critical tone of much of what follows, five points of agreement with Ostrom deserve highlighting:

- 1. It is inadequate to separate sharply politics and administration, as serious students have acknowledged for several decades.
- 2. Monocratic bureaucracy deserves criticism, both empirically and normatively, as a huge literature testifies.
- 3. Individuals should have a broader role in influencing public policy and its administration, as both members and clients of public agencies.

*This paper is one reflection of a substantial history of exchanges of ideas and papers that the author has had with a colleague, Dr. Keith Baker. Other reflections appear in Baker's works: "Public Choice Theory: Some Important Assumptions and Public Policy Implications," in Public Administration, ed. Robert T. Golembiewski, Frank Gibson, and Geoffrey Y. Cornog, 3rd ed. (Chicago: Rand McNally, 1976), pp. 42–60; and "The Search for a New Paradigm in Public Administration," paper delivered at the Annual Meeting, Southern Political Science Association, New Orleans, La., November 1974. The author warmly acknowledges that interchange.

¹Vincent Ostrom's *The Intellectual Crisis in American Public Administration* (University, Ala.: University of Alabama Press, 1973) is the central source.

- 4. Greater administrative responsiveness to clients' perceptions of needs, as well as greater productivity in providing public services, are to be desired and worked for.
- 5. Economic modes of thought and analysis can be powerful aids to enlightened choice making.

The basic concern here is more with Ostrom's argumentation than with his conclusions, as the three parts of this analysis reveal. Two early sections briefly introduce Ostrom's view of the major contenders for the dominant paradigm to guide the study and practice of public administration, with special attention to how these contenders deal with such central questions as determining the scale of public delivery systems. A long argument then provides a detailed analysis of the ideation supporting democratic administration, that ideation being seen as basically inadequate from five central perspectives.

Two Contenders for the Reigning Paradigm

Ostrom has two goals; thus he seeks to explain the "intellectual crisis" of public administration by pitting against one another two broad and contrasting paradigms—"paradigm" meaning roughly the set of guiding ideas which detail "significant" problems, how these problems are to be analyzed, and in what priority they are to be dealt with. One paradigm is the cause of the crisis, and the second constitutes the cure. Simply, Ostrom argues, supplant one of those paradigms, root and branch, by the other.

The first or "traditional" paradigm-clearly recognizable as close kin to the monocratic

bureaucracy of Max Weber-has a "top, down" bias and a long-lived influence. Its overall image, in Ostrom's telling, is an overflowing font of sovereignty, authority being concentrated at high levels but allowed to trickle variously down the hierarchy of state enterprises. More specifically, the traditional paradigm postulates that there will be a single dominant center of power in any system of government. Consequently, all modern governments face the same challenge: developing a unitary hierarchy under a single executive, which leads a corps of trained and politically neutral professionals.² These notions, patently consistent with a naive separation of politics and administration, provided essential guidance for a half-century or more to both students and practitioners of public administration, Ostrom advises.

Ostrom offers a contrasting, alternative, and (to him) more useful paradigm, "democratic administration." That alternative has a "bottom, up" and heterogeneous character, in contrast with the ordered symmetry of a hierarchy. To illustrate, democratic administration rests on such propositions as the following:³

- 1. Perfection in the hierarchical ordering of a professionally trained public service accountable to a *single* center of power will reduce the capability of a large administrative system to respond to diverse preferences among citizens for many different public goods and services and also will reduce its ability to cope with diverse environmental conditions.
- 2. Perfection in a hierarchical organization accountable to a *single* center of power will *not* maximize efficiency as measured by least cost expended in time, effort, and resources.
- 3. There are two necessary conditions for maintaining a stable political order which can advance human welfare under rapidly changing conditions: fragmentation of authority among diverse decision centers with multiple veto capabilities in any one jurisdiction; and the development of multiple, overlapping jurisdictions of widely different scales.

Criteria Central to Democratic Administration: Determining the Scale of Public Delivery Systems

Even the sparse illustrations above clearly imply that democratic administration must deal with major problems that are superficially finessed by the traditional paradigm. Note only

two questions which Ostrom's alternative must face: What should be the scale of public service delivery systems? How should they be differentiated and coordinated?

Such questions are not central ones for the traditional paradigm, which simply focuses on approaching the ideal of one increasingly integral authority structure. Both the implied theoretical and practical urgencies, then, center around reducing any extant autonomy of component government units so as to increase the reality of a "chain of command." Hence this answer to the key issue of how big a public service delivery system should be: as big as necessary to create a unitary organization under a single head. This is a prescription for monolithic giantism, clearly.

The literature on organizations is in fact far from being as monolithic as Ostrom usually insists, which oversight at once makes his analytic task simpler in a trivial sense and more difficult in more central senses. Beyond the core agreement about initial centralization, opinions of those supporting the traditional paradigm tend to fall into two camps. One camp maintains that effective centralization must precede effective decentralization. Its proponents presume that acting on the urgencies above is only a necessary prelude to dealing with the issues of fine-tuning the system, of determining the scale of component units as well as of defining a rationalized system of differentiated/integrated subsystems. In more extreme versions of the traditional paradigm, however, the achievement of the totally rationalized and centralized system is seen as the goal rather than as a prelude to some newly reachable goals.

Ostrom fixates on the latter interpretation, which constitutes an easier but simplified analytical target. Had he acknowledged the former interpretation more consistently, Ostrom's argument could have differed profoundly from what it actually came to be. Rather than a radical, either/or opposition of paradigms, for example, his argument could have then been framed in developmental terms, as evolutionary rather than as revolutionary. This would have been more faithful to the history of the relevant literatures.

Ostrom rejects the traditional paradigm, however, and does not acknowledge the crucial distinction above. In contrast, he offers four criteria which deal with the complex problems of scale in governmental units in an uncomplicated way. Basically, his goal is to define suitably a "package" for the boundaries of a unit of government such that those outside the

²Ibid., esp. pp. 28-29.

³Ibid., esp. pp. 111-12.

boundaries are excluded from the use of the public goods or services provided by that unit. The criteria are sketched in Table 1.

A Critical Analysis of the Ideation Underlying Democratic Administration

Democratic administration has received little critical analysis,⁴ despite the burgeoning attention devoted to the "public choice" context of which it is both creator and product. To be sure, Ostrom does note that the above criteria "are sometimes in conflict." And he does allow that when the criteria are not applied adequately, the "package" for the production and provision of public goods by the governmental unit whose boundaries are being drawn will be

⁴For exceptions to the lack of critical attention, see Thomas R. De Gregori, "Caveat Emptor: A Critique of the Emerging Paradigm of Public Choice," Administration and Society, 6 (August 1974), 205–28; and Keith Baker, "Public Choice Theory: Some Important Assumptions and Public Policy Implications," in Public Administration, ed. Robert T. Golembiewski, Frank Gibson, and Geoffrey Y. Cornog, 3rd ed. (Chicago: Rand McNally, 1976), esp. pp. 42–60.

faulty. This will result in "externalities [which] spill over upon neighboring communities."⁵

The critical analysis here is more ambitious than Ostrom's brief qualifications. By way of introduction, the following argument seeks to establish a broader indictment of Ostrom's argument and its underlying ideation, particularly on these issues:

- The methodology underlying democratic administration is awkward.
- 2. Major assumptions in Ostrom's argument are at least improbable descriptors and may be seriously incongruent with reality.
- Critical content or meaning is lacking in major concepts underlying democratic administration.
- Opposite and simultaneous courses of action are implied by central aspects of Ostrom's system of thought.

⁵Vincent Ostrom, Charles M. Tiebout, and Robert Warren, "The Organization of Government in Metropolitan Areas," *American Political Science Review*, 55 (December 1961), 835.

Table 1. Criteria for Determining the Scale of Public Delivery Systems^a

- 1. Control. The broad "boundary conditions" of a political jurisdiction should be so defined as to "include the relevant sets of events to be controlled." Such relevant events are not uniformly distributed in space or time—whether they involve a watershed basin or patterns of human interaction—so these boundaries can only be defined "with more or less precision." Too narrow boundaries disable a public agency and likely will result in a transfer of that agency's function to a governmental unit "scaled to meet the criterion of control more adequately."
- 2. Efficiency. "The most efficient solution would require the modification of boundary conditions so as to assure a producer of public goods the most favorable economy of scale, as well as effective control."
- 3. Political Representation. This criterion requires "the inclusion of the appropriate political interests within [each boundary's] decision-making arrangements." The issues are complex, but the ideal solution—assuming "criteria of responsibility and accountability consonant with democratic theory"—would require that "three boundaries be coterminous." These boundaries are:
 - a.) the scale of formal organization, or the size of the government unit providing some public good;
 - b.) the public, consisting of those affected by the provision of that good;
 - c.) the political community, or those "who are actually taken into account in deciding whether and how to provide" that public good.
- 4. Local Self-Determination. The application of the three preceding criteria to local governments in the United States is usually to be "subordinated to considerations of self-determination"; that is, application is usually to "be controlled by the decisions of the citizenry in the local community."

The basic assumption is that public goods can be appropriately "packaged" or "internalized," so that those outside the boundaries of a local unit of government can be excluded from the public goods produced by that unit. Looked at from another perspective, the assumption is that the "purely 'municipal' affairs of a local jurisdiction, presumably, do not create problems for other political communities." Relatedly, also, this fourth criterion implies that the local citizenry are both capable of and motivated toward holding public officials accountable, in ways that do not create major problems for other political communities.

^aSource: Vincent Ostrom, Charles M. Tiebout, and Robert Warren, "The Organization of Government in Metropolitan Areas," American Political Science Review, 55 (December 1961), esp. 835-37.

5. Democratic administration can lead to unexpected practical consequences, sometimes the opposites of those intended.

Methodological Concerns. The central discrepancies in Ostrom's position are methodological and constitute slippery targets because his usages (and the usages of those public choice theorists on which Ostrom explicitly relies) are so alluring as to invite incautious acceptance. This section will briefly discuss several of these methodological concerns, and subsequent sections will add substance to this lean argument.

Basically, the public choice position rests upon a methodology that emphasizes a closedsystem circularity, while it permits incautious building upon assumptions that are often suspect. These propensities to circularity and incaution raise serious concerns about the argument built on such foundations. The circularity is often explicit in the key questions for inquiry, as in: "When will a society composed of free and rational utility-maximizing individuals choose to undertake action collectively rather than privately?" It comes as no surprise to learn that collective action will occur when (enough?) individuals realize that they can increase their "utility" by so organizing.6 The line of argumentation is tendentious because "free and rational utility-maximizing individuals" could hardly have done it any other way. Worse still, rather than identifying or describing or predicting, this approach permits only a posteriori classification, as in: Aha, there is a collective action; ergo, someone's utility has been maximized. The lack of caution in argument will be illustrated at several points below, especially in the case of assumptions that conflict with reality.

Four points provide further perspective on this closed-system and convenient methodology. First, prominent public choice theorists seek to have the best of all analytical worlds in casually shifting the base of their arguments. Consider the central notion of "methodological individualism"—on which the public choice argument is typically predicated and which Ostrom tells us he accepts. The concept seeks to explain collective decision making as a simple summation of the individual decisions made by separate and distinct persons; and it is deliberately contrasted with "normative indivi-

dualism," which Buchanan and Tullock stress is rooted in the "explicit acceptance of certain value criteria." Perhaps the intent is to represent the public choice argument as one of science rather than of personal bias. Whatever the intent, Buchanan and Tullock are not sufficiently restrained by their own tethers. For example, they also announce early on that they "are not directly interested in what the State or a State actually is, but propose to define quite specifically, yet quite briefly, what we think a State ought to be." De Gregori arches an analytic eyebrow: "Methodological or normative?" he asks rhetorically of this elastic aspect of the authors' system of thought. 10

Second, related major concerns about Ostrom's argument derive from the assumptive and derivative nature of the core ideational infrastructure of the theory of public choice. Numerous evidences and consequences of this assumptive and derivative character will be developed later, but one elementary point is patent even now: the character of the assumptions is vital because it predetermines the range of derivations, given nimble minds and consistent logic. The cruciality of these assumptions does not get sufficient respect in the ideational infrastructure on which Ostrom relies. Why this neglect exists is puzzling, for some of those assumptions are at least controversial, if not in fact denied by some of those involved in the technical development of public choice theory. 11 So flexible is Ostrom's underlying methodology, indeed, that such controversiality or denial is taken in full stride, without apparent conceptual pause. His methodology, in fact, often has a heads-I-win. tails-you-lose quality. When pushed on the issue of the "real-world inapplicability" of one of his assumptions, or about a conflict between assumptions, illustratively, Ostrom only allows that "it might be necessary" to make different assumptions as to the "prevailing human condition." 12

Norton Long goes for what he sees as the jugular vein of this kind of reliance on those types of assumptions. They may not mirror reality and, hence derivations from them are highly suspect. Referring to two well-known public choice theorists, for example, Long

⁶James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor, Mich.: University of Michigan Press, 1962), esp. pp. 43–48.

⁷Ostrom, esp. pp. 50-51.

⁸Buchanan and Tullock, p. vii.

⁹Ibid., p. 3.

¹⁰De Gregori, p. 208.

¹¹William A. Niskanen, Jr., Bureaucracy and Representative Government (Chicago: Aldine-Atherton, 1971), esp. p. 218.

¹²Ostrom, p. 52,

concludes that they "can argue with elegant and impeccable logic about unicorns. The cogency of their logic, however, does not demonstrate the existence of unicorns." ¹³

Third, and in equal measure, Ostrom's argument also induces concern about the "direction" of its methodology, as well as that of its ideational infrastructure. The basic thrust is from assumptions about reality to prescriptions about how the real world should be. This directionality makes the public choice theorist more central than the testing of his theories in the real world and provides no defense at all against the human bias to see in reality what we are predisposed to see. Building complex ideational systems on assumptions about reality is hazardous, that is, especially in the absence of major motivation to test those assumptions early in the game. This motivation to test reality characterizes neither Ostrom nor the public choice literature, in the main. Indeed, one careful observer regards "the virtual absence of empirical concern as a major weakness in the collective choice literature."14 Hence it is unclear how much public choice theory reflects the world or ideological concerns; it is moot as to what degree its theoretic agreements mirror reality or the life experiences, beliefs, or preferences of the theorists. The only certainty is that Ostrom takes no great pains to safeguard his theory against being an analytical Rorschach.

Of course, one may build a theory based on any assumptions that one fancies, for example, a theory of architecture based on the assumption that the only building materials are spaghetti and gum drops. And even very bizarre assumptions can prove useful. Whether that occurs, however, depends not so much on the assumptions as on how the derivative theory is used. One could regard such theory as hypotheses-to-be-tested, as some theorists of public choice are careful to do. 15 Such testing can do multiple duty: it might provide evidence as to whether the assumptions underlying the theory are more or less correct; it might suggest that the original assumptions are so simplistic as to generate theory that ill suits reality; and so on. Alternatively, one could treat such derivative theory as the basis for making action plans for

¹³Norton Long, *Journal of Politics*, 36 (August 1974), 804.

intervening in reality or (worse still) for describing how things should be. This is a very dangerous business indeed. Ostrom's methodological approach encourages the latter treatment of theory as the-end-of-the-road rather than as hypotheses-to-be-tested.

Fourth, neither Ostrom nor public choice theory provides an acceptable place for values. Indeed, in concepts critical to the ideational structure, the value judgment is made that almost all value judgments cannot be made or should not be made. The purpose is to avoid making subjective, nonempirical judgments; but the approach also seeks to legitimate that avoidance via subjective, nonempirical judgments. That does not wash.

The present point has a paradoxical quality. That is, Ostrom's democratic administration is value loaded in critical senses, of which both writer and readers could hardly be unaware. And critics like De Gregori and Baker are quick to emphasize the metavalues of the public choice system: a "conservatism," a clear preference for those exercising economic power as contrasted with electoral or political power, a superior status for preferences reflected in markets compared to those expressed electorally or politically, and so on. 16

When theoretical push comes to shove, however, the public choice approach variously declares a value judgment that is intended to exclude almost all other value judgments. Consider the Pareto criterion or optimum, which is subscribed to by many public choice theorists, and which proposes that a change is desirable if it makes at least one person better off in that person's own judgment while leaving not even a single person worse off in his or her own judgment. Proponents of the criterion argue that it has two basic attractions: it is ethically appropriate; and the decisions made consistent with it are potentially verifiable by empirical analysis alone. The underlying assumptions are those of logical positivism, which considers as scientifically meaningful only those statements whose truth or falsity can be demonstrated by empirical test, at least in principle. Superficially, the Pareto criterion nicely fits this requirement. As Merewitz and Sosnick explain, "If one adopts the Pareto criterion and then states that a change is desirable, the statement becomes equivalent to two other statements" which, at least in principle, can be empirically verified or rejected: that the change moves some persons

¹⁴Kenneth A. Shepsle, "Theories of Collective Choice," in *Political Science Annual*, Vol. 5, ed. Cornelius P. Cotter (Indianapolis, Ind.: Bobbs-Merrill, 1974), p. 4.

¹⁵For example, Niskanen, esp. p. 8.

¹⁶De Gregori, esp. pp. 207 and 217-19; and Baker, pp. 42-60.

1.36

to more preferred positions while disadvantaging no one; and that the person making the statement approves the change.¹⁷

This line of argument walks away from most or all issues that are morally or ethically troublesome or that are likely to be encountered in practice. To explain, the Pareto criterion in effect (and one supposes, by design) seeks to limit attention to these three values: that it is desirable to let people have what they want; that it is undesirable to impose on people anything that they do not want; and that it is legitimate to neglect all else save that which people want or do not want.18 Most of the interesting issues in choice, and many of the significant ones, are thereby neglected. Hence what seems an unexceptionable criterion may have profound consequences, not all of them desirable for those unwilling to make the value judgment that almost all value judgments cannot or should not be made. Merewitz and Sosnick see "doubtful merit" in the position especially "if the people involved are ill-informed (as with hazardous products or complicated medical care), if the people are incompetent (as with youngsters, oldsters, and morons), or if they have lost self-control (as with addicts, drunks, and the bereaved). Moreover, what the affected persons want may not be everything that matters to the person passing judgment. He may be concerned about national interests, about accurate reporting, about due process of law, ... and so on."19

The Pareto criterion gains internal consistency only at the cost of defining out of its domain many issues of practical moment, if not all of them. For example, it does not deal with the class of situations in which some individuals would be better off and some worse off if an action were taken. The putative reason for the exclusion is that such situations require some evaluation of the relative worth or value of what specifically is gained or lost, which evaluation is not possible on empirical grounds. Hence exclusion of a massive class of situations is the price paid for the convenient finesse of value issues, except for the three detailed above. In addition, the domain of the Pareto criterion is even further narrowed by the requirement that the affected persons and their reactions are known with certainty. The narrowing is especially severe in those cases where persons not yet born will be affected, or when the effects are probabilistic tendencies whose actual impact is dependent upon complex conditions which are themselves uncertain.²⁰ Patently, this means that the Pareto criterion will apply to very few issues of public policy. It conceptually avoids value judgments by practically avoiding most classes of significant choice.

Not all public choice theorists accept the full conceptual baggage of the Pareto criterion, ²¹ but Ostrom is not one of those who succeeds in resisting the allures of the extreme simplification of the central issue of values. There is a basic irony here, for Ostrom rejects the notion of any separation of politics from administration and urges that the latter be disciplined by the former. But he otherwise accepts an ideational network that would in effect remove from politics so much of its essence, that tumultuous clash of values inherent in the effort to define the role of the good citizen in the just state.

In Ostrom, the clearest evidence of the simplification of values is the insistent emphasis on consumer preference or utility as sovereign, as in the concept of "methodological invidualism" or "individualistic choice."22 De Gregori apparently appreciates fully how "preference" or "utility" have become value surrogates in public choice theory, by way of such conceptual inventions as the Pareto criterion. Hence he hammers hard at the fact that Ostrom and the public choice theorists-by their insistence that only market decisions provide acceptable data and appropriate "freedom"-in effect consider one set of tastes or preferences as clearly superior to others such as those reflected in elections. De Gregori sees no theoretical justification for that superiority and, moreover, is troubled by the lack of associated specificity. He asks: "Freedom from what? Or freedom for what?"23

De Gregori insists on seeing the full array of values that provide specific content for generic terms like "tastes," "preferences," "freedom," and so on, that is, so priorities may be determined should tastes conflict. He is at once profoundly correct in this insistence, and yet it is irrelevant for public choice theorists. Their

¹⁷Leonard Merewitz and Steven H. Sosnick, The Budget's New Clothes (Chicago: Markham, 1971), pp. 79, 80.

¹⁸Ibid., p. 79n.

¹⁹Ibid., p. 79n.

²⁰Ibid., p. 80n.

²¹Illustratively, the Kaldor-Hicks and Little criteriahave been developed to loosen the conceptual limits of the Pareto criterion. See J. R. Hicks, "The Rehabilitation of Consumer's Surplus," *Review of Economic* Studies, 8 (1940–1941), 108–16.

²²Ostrom, esp. pp. 60-64.

²³De Gregori, p. 220.

methodology and its derivative theory define what he insists upon as not relevant for purposes of analysis and—given some incaution—as nonexistent, period. Recall this central feature of the public choice argument: that almost all value judgments cannot or should not be made. As mentioned above, the three value judgments that can or should be made from the public choice position are: that it is desirable to let people have what they want; that it is undesirable to impose on people anything they do not want; and that it is legitimate to neglect all else save that which people want or do not want.

Probably Improbable Assumptions. Concern with Ostrom's argument is especially appropriate because some of his central assumptions are at least probably improbable, which his methodology will fail to highlight.

The point is clear in Ostrom's central commitment to "methodological individualism." Essentially, Ostrom proposes a model of men which seeks to explain public policies in terms of individual rational decisions which act as an unseen hand guiding those market forces that determine the supply of public goods and the demand for them. This basic commitment, in turn, requires a host of definitions/assumptions about how men behave. Thus decisions are considered rational so long as they contribute to the attainment of specific goals or objectives, which are taken to be mutually consistent as well as transitive so that if people prefer decision A to C and B to C, they also will prefer A to C. In sum, the assumptions/definitions are that decision makers also will act consistently with: their own self-interest, rationality, an appropriate quality of available information, law and order, and the choice of a maximizing strategy, a least-cost way of achieving preferences.²⁴

It is tempting to be picky with such necessary underpinnings of "methodological incividualism." For example, what if the actual expressions of "self-interest" conflict with the assumption of "law and order"? In the "absence of any law and order assumption," Ostrom can only advise, "it might be necessary" to make another assumption as to "the prevailing human condition." ²⁵

The admission that it "might be necessary" to make other assumptions is a disarming but far too mild formulation, even though on the same grounds it might also be necessary to

abandon all the other assumptions/definitions above. The prudent course would have been to make as certain as possible that different assumptions are *not* necessary because the ones used are adequate expressions of our existing knowledge of the world around us. The approach taken was apparently more convenient but also is more troublesome.

Apparent convenience and trouble seem a common pair, as the public choice theorists' common insistence on transitivity in individual decision making illustrates. Transitivity would exist in the case of voting, for example, if a voter who prefers alternative 1 to 2 and alternative 2 to 3, also prefers alternative 1 to 3

This insistence on transitivity seems a prime example of how the assumed need to preserve ideational integrity can be multiply mischievous. The following three-part argument suggests that transitivity is too restrictive a central condition to build into a theory of choice. First, psychologists have not been able to establish that transitivity is characteristic of human behavior. Oppositely, Weinstein concludes that "most empirical tests of choice behavior appear to reflect intransitive preference patterns."26 Second, the complex concurrent and multiple majorities characteristic of American politics and political parties do not encourage the posing of policy choices in ways that facilitate or even permit transitive choice.²⁷ Third, insistence on transitivity in voting typically would lead to consequences diametrically opposed to the values that public choice theorists publicly espouse, including the freedom of the individual decision maker.²⁸ We cannot here present the lengthy argument that establishes this irony, but we can conveniently quote Dahl on the central point. Dahl in 1956 noted: "Arrow shows that if there are more than two alternatives, any method for making

²⁶Arnold A. Weinstein, "Individual Preference Intransitivity," Southern Economic Journal, 34 (January 1968), 335.

For an example of the empirical literature consult Amos Tversky, "Intransitivity of Preferences," Psychological Review, 76, No. 1 (1969), 31–48. Tversky begins to specify the empirical conditions under which consistent and predictable intransitivities occur. Those conditions seem to be quite prevalent, which provides but little support for the ideation and argument underlying democratic administration.

²⁷Robert A. Dahl, A Preface to Democratic Theory (Chicago: University of Chicago Press, 1956), esp. pp. 34-50.

²⁸Kenneth J. Arrow, Social Choice and Individual Values (New York: Wiley, 1951), pp. 51-59 and

²⁴Ostrom, pp. 50-51.

²⁵Ibid., p. 52.

social decisions that insures transitivity in the decisions must necessarily be either dictated by one person or imposed against the preferences of every individual."²⁹ The last two decades have seen an extensive literature develop around the "voting paradox" but, despite qualifications of subtlety and issues in doubt, Arrow's conclusion still seems more appropriate than not.³⁰

The prudent implication is caution about accepting any derivations from such public choice assumptions, an implication reinforced by considering one central aspect of "methodological individualism." Does it intend to deny the independent existence of supraindividual collectivities like "groups"? The answer is, mostly, yes. But some public choice theorists seem to want to have it both ways, as do Buchanan and Tullock in announcing: "Throughout our analysis the word 'group' could be substituted for the word 'individual' without significantly affecting the results."31

This denial of the need for multilevel analysis is clearly convenient, but it almost certainly means a major reduction in the explanatory power of any derivative theory. Whether the answer is a straightforward denial of supraindividual collectivities, or pained efforts such as that above to suggest some kind of equivalence between "individual" and "group," reality is too recalcitrant to fit such economizing formulations. To be sure, only individuals can perceive and make decisions. But what of the common experience that the same individuals typically behave in radically different ways following deliberate manipulations of the "climates" of the successive groups to which they belong?32 Such effects clearly require a supraindividual level of description and explanation. Or better still, what of groups in which all or many members come to decisions which they personally do not prefer, or even abhor, on profoundly serious³³ as well as trivial³⁴

matters? The alternative to denying multiple levels of social organization is to presume the irrationality of mankind, or to postulate their "herd" or "crowd" quality, which names or vilifies but does not explain.

What does this catalog imply? Clearly, public choice theorists are not sensitive to such dilemmas in their ideation. Clearly, also, Ostrom would like to increase the role of the individual decision maker in public policy choices. All well and good, and many of us applaud that intent. And there is no question but that assumptions like "methodological individualism" provide conceptual support for such an intent. But it appears that too much is made of a convenient thing. Ostrom not only articulates a "should" prescribing an increased role for individual decision makers (which is proper enough), but he also accepts a theoretical infrastructure which conveniently recognizes only separate individuals and thus flies in the face of long-standing experience. As De Gregori concludes, if any theorists assume "that large numbers of people do not consider themselves members of economic and social classes [and consequently that they do not] act politically in these terms (that is collectively), [those theorists] have a sizable body of data to refute."35 Methodological sleight-of-hand does not change that stubborn fact. The neglect also may imply serious misdirection if, as many argue, the individual needs to define self largely in social interaction, and if freedom is possible only in a special kind of society. Emphasis on the separate individual will in such cases result in not asking many of the appropriate ques-

Critical Gaps in Content and Meaning. Beyond relying on improbable assumptions, Ostrom's position and its underlying public choice theory also lack critical content or meaning. Indeed, this puts the point mildly, as is manifest in the unsatisfying treatment of self-interest and efficiency in Ostrom's formulation, as well as in public choice theory in general.

Whose Self-Interest about What? Public choice theory waffles badly on the issue of self-interest, and consequently finds itself between the analytical rock and a hard place. In an assumed attempt to be comprehensive, it ends up saying little clearly or consistently. De

²⁹Dahl, p. 42n.

³⁰Shepsle, esp. pp. 31–42, provides ample documentation of this summary statement.

³¹Buchanan and Tullock, p. 9.

³²This was the case, for example, in the famous 1930 studies of group atmospheres or climates. See especially Ronald Lippitt, "Field Theory and Experiment in Social Psychology: Autocratic and Democratic Group Atmospheres," American Journal of Sociology, 45 (July 1939), 26–49.

³³ Irving Janis, Victims of Groupthink (Boston: Houghton Mifflin, 1972), deals with a number of major political and military decisions which fit this characterization.

³⁴The classic experimental demonstration is that of Solomon E. Asch, "Effects of Group Pressure Upon

the Modification and Distortion of Judgments," in Groups, Leadership, and Men, ed. Harold Guetzkow (Pittsburgh, Pa.: Carnegie Press, 1951), pp. 177-90.

³⁵De Gregori, p. 210.

Gregori's conclusion is harsher still. Referring to two central figures in public choice theory, he concludes that their writings about "self-interest contain neither a concept of self nor a concept of interest, and we are left with a logical or mechanical construct without content." 36

The basic point is easy enough to document, whatever the harshness of the overall evaluation. One thing that should be patent about "methodological individuals" is that they operate in terms of their self-interest narrowly defined. However, significant public choice theorists apparently recognize that the narrow definition of self-interest is inadequate for describing reality. In any case, some theorists take care to emphasize that their "methodological individuals" have a broad-band self-interest. Their motivation is not only "narrowly hedonistic," but it can also be (and often is) "egoistic or altruistic." 37

Ostrom accepts the broad-band notion of self-interest ³⁸ and, however motivated, that acceptance creates far greater theoretical problems than it solves. Indeed, the irony may be that there is no solution, only a dilemma. Perhaps De Gregori is correct in concluding that self-interest as a postulate is either false in its narrow form or trivial as a broad-band notion.

Both falsity and triviality can be established by considering two problems for public choice theory implied by broad-band self-interest. First, the "what" of self-interest is no longer clear, and the predictive power of public choice theory thereby suffers. De Gregori nicely makes the point by observing that, if individuals are assumed to be profit maximizers, it is then reasonable to talk "about a form of behavior in which individuals seek to maximize an empirically definable function that is called economic gain." But this advantage vanishes in broad-band self-interest. De Gregori makes the contrast vividly. "Call the motivating principle 'utility' or 'ego' or whatever, and one has a principle that can be defined only in terms of the ensuing action.... It does not explain or predict the action, it merely names it."39

The same point, indeed, was apparently clear to Buchanan and Tullock in one of the early and seminal books on public choice. Broad-

band self-interest, they recognize, permits an economic assumption of only the "barest essentials." To wit: "The economic assumption is simply that the representative or the average individual, when confronted with real choice in exchange, will choose 'more' rather than 'less'."40 This is neither a powerful assumption nor a comforting one, in light of the authors' single quotation marks around what are fundamental concepts in their system of thought. The assumption permits only such gentle a posteriori statements as the following: "When I observed an economic choice, the choice was motivated by the chooser's judgment that it provided more of something, but more of what and how much more I cannot now say." Because this statement can never be tested and hence cannot be shown to be false, it provides a delicate foundation indeed for theoretical development.

Second, broad-band self-interest obfuscates another issue that is crystal clear in narrow concepts, since whose self-interest becomes a major unknown rather than a given. To explain: The "separate individual's" self-interest is stage center in narrow concepts. In contrast, if altruism (or whatever) is allowed as a motive, the issue of whose self-interest is involved takes on complex proportions. The conceptual door is also opened to various social or collective processes, including group formation, through which individuals can act in terms of the best interests of others as well as their own. Some public choice theorists follow Sen in this regard, who explains that

the society in which a person lives, the class to which he belongs, the relation that he has with the social and economic structure of the community, are relevant to a person's choice not merely because they affect the nature of his personal interests but also because they influence his value system including his notion of "due" concern for other members of society. The insular economic man pursuing his self-interest to the exclusion of all other considerations may represent an assumption that pervades much of traditional economics, but it is not a particularly useful model for understanding problems of social choice. 41

Other theorists—including Ostrom—avoid such an admission, presumably because it undercuts the "methodological individualism" so central in public choice theory. Perhaps that is why Buchanan and Tullock go as far as—but no

³⁶Ibid., p. 209.

³⁷ Buchanan and Tullock, pp. 3, 17.

³⁸Robert L. Bish and Vincent Ostrom, *Understanding Urban Government* (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973), p. 18.

³⁹De Gregori, p. 209.

⁴⁰Buchanan and Tullock, p. 18.

⁴¹Amartya K. Sen, Collective Choice and Social Welfare (San Francisco: Holden Day, 1971), p. 6.

further than-noting that the word "group" in their analysis can be used interchangeably with the word "individual" without significantly affecting their results.⁴²

Efficiency for What Purpose? The gaps in meaning or content in Ostrom's approach are especially prominent in the second criterion in Table 1—efficiency—for determining the scale of public organizations consistent with democratic administration. The deficiencies are conveniently expressed in the context of critical questions with which Ostrom does not deal, a question, for example, such as: Efficiency for what purpose?

Ostrom's assignments of meaning to "efficiency" seem general and unsatisfying. At one point, for example, he notes that consumer preferences are central, perhaps even exclusive, in providing the meaning of efficiency. His proposition (which he italicizes) is: "Producer efficiency in the absence of consumer utility is without economic meaning." This is not analytically satisfying, however. Clearly, it runs afoul of the several considerations raised earlier in connection with the broad-band concept of self-interest. Ostrom's dictum about economic meaning has no tolerably precise meaning.

Ostrom's failure to deal effectively with the values or goals defining efficiency also impacts significantly on his broader argument, as illustrated by the lack of content or meaning in his criteria sketched in Table 1. Recall Ostrom prescribes (in part) that: "The most efficient solution would require the modification of boundary conditions so as to assure a producer of public goods and services the most favorable economy of scale, as well as effective control."44

The prescription is vague in the absence of an unambiguous answer to: Efficiency for what purposes? During World War II, for example, it was common that the most monopolistic industries were "most efficient" in a number of then-relevant senses. Thus it was easier for government officials to deal with Alcoa than with the multitudinous producers of feeds and grains, for more or less obvious reasons. Of course, Alcoa also was in a position to be more troublesome if it so chose, but wartime conditions were well-suited to concentrated control and planning of aluminum production, given

The emptiness of the phrase "the most favorable economy of scale" also can be demonstrated in less obvious ways. Consider Ostrom's contrast of two ways of viewing efficiency: "One way views efficiency as being expressed through principles of hierarchical organization," by which he refers to concepts such as "unity of command, span of control, chain of command, departmentalization by major functions, and direction by single heads of authority in subordinate units." Ostrom notes with skepticism that these principles are alleged to be universally applicable, and that this first way of viewing efficiency is direct and uncomplicated. "The greater the degree of specialization, professionalization, and linear organization in a unitary chain of command, the greater the efficiency," Ostrom correctly states the essential implication of the extreme version of the traditional paradigm he chooses to emphasize. The bigger the unitary organization, in short, the better. Ostrom adds by way of contrast: "The other way views efficiency in terms of a cost calculus," which Ostrom asserts will permit the "accomplishment of a specifiable objective at least cost; or a higher level of performance at a given cost."45

Ostrom's sentiments clearly rest with the "cost calculus," and his suspicions about efficiency through hierarchical organization are profound. But the argumentation supporting that preference is only briefly developed, and it is not convincing for several reasons. First, there is no available "cost calculus" of the kind required. Even the traditional tools and approaches for financial analysis imply serious issues and deficiencies.46 Worse still are the problems of taking into account a broader range of considerations that are typically neglected in managerial accounting and financial reports, factors such as the motivation or morale of the work force, the long-run vs. the short-run consequences of managerial actions,

acceptable combinations of patriotism and whatever. Because short-run control was facilitated, the answer to how big Alcoa ahould be was: the bigger the better, and the greater its share of the market the better. In the immediate post-war period, different considerations became prominent in public policy, and different answers consequently were appropriate. Both before and during the war, however, the central question was the same: Efficiency for what purposes?

⁴² Buchanan and Tullock, p. 9.

⁴³Ostrom, p. 62.

⁴⁴Ostrom, Tiebout, and Warren, pp. 835-37.

⁴⁵Ostrom, pp. 35, 48.

⁴⁶Abraham J. Briloff, *Unaccountable Accounting* (New York: Harper, 1972).

etc. Perhaps the most that can be said here is that such work is only a little way down the track.⁴⁷ Most chronic by far are the problems with "social accounting," which seeks to relate organizational costs to broad goals or ends.⁴⁸

Ostrom's treatment of the "cost calculus" re organization scale does some violence to central notions of public choice theory, moreover, and seems at odds with parts of his own argument. Consistent with Ostrom's emphasis on markets, "methodological individualism," and so on, it would have been seemly to argue that the scale of public organizations on the supply side is, or should be, a function of the same market forces that public choice theorists seek to energize/legitimate on the demand side. In contrast, he introduces the "cost calculus," which implies a cadre of experts, with data far beyond that available to his individual choice makers, and ostensibly with some power or authority to influence the scale of public organizations. Suddenly, Ostrom finds himself squarely facing the kind of problem that PPBS enthusiasts had promised to solve, those very centralizing, rationalist, elitist proponents from whom Ostrom otherwise seeks to distance his argument.⁴⁹ Ostrom is not successful at transcending their failure. Indeed, he basically promises a "cost calculus" rather than provides one. Ostrom is in the same boat on this issue as the conceptual enemy. The conclusions applying to PPBS efforts thus apply to Ostrom in this regard. Consider their hope to justify appropriations or expenditures for public programs. Merewitz and Sosnick quash that hope by recalling the harsh and unavoidable realities. "To justify an appropriation," they observe, "it is necessary to show that the benefits to be obtained outweigh the benefits that could be obtained by spending the same amount of money for other purposes or by cutting taxes. And this cannot be shown." They conclude: "No procedure is available that will show whether expenditures are or are not justified. The answer is a matter of subjective judgment."50

Second, as the reference to social accounting implies and as the excerpt from Merewitz and Sosnick explicitly notes, there can be no "cost calculus" for determining the appropriate scale of any organization until an answer to a key question is reasonably in hand. That neglected key question: Efficiency for what purpose?

Third, Ostrom's juxtaposition of hierarchical perfection and cost calculus constitutes a straw man. That is to say, for nearly the preceding half-century, the "principles of organization" have been attacked with great gusto, and even some effect.⁵¹ Although "contingency theorists" will argue that the principles describe the appropriate form of organization under certain technological and product-line conditions, the definite drift of research and commentary argues the appropriateness of organizing guidelines that differ significantly from the "principles." These evolving guidelines were long ago sketched in the public administration literature and also have received fulsome attention in the literatures of generic and business administration.⁵² The most common citations in Ostrom predate this work by a decade and usually far longer, with most attention going to the central Papers on the Science of Administration (1937) by Gulick and Urwick and to Simon's powerful but early (1946) critique of the "principles" literature.

There is no point in detailing here these emerging guidelines for organizing work, except to note that they constitute an alternative to the "cost calculus" which Ostrom proposes will decide the scale of organizations but on which no one can deliver.

Simultaneously Opposed Courses of Action. Still more troublesome, Ostrom's prescriptions often have the unhappy feature of recommending two or more courses of action that are potentially antithetical. Consider Table 1, in

⁴⁷Consult William J. Burns, Jr., and Don T. De Coster, eds., Accounting and Its Behavioral Implications (New York: McGraw-Hill, 1969); and Thomas Burns, ed., Behavioral Experiments in Accounting (Columbus, Ohio: College of Administrative Sciences, Ohio State University, 1972).

⁴⁸ Bertram M. Gross, The Managing of Organizations, Vol. 2 (London: Free Press of Glencoe, 1954), esp. pp. 467-501; and Meinoff Dierkes and Raymond A. Bauer, eds., Corporate Social Accounting (New York: Praeger, 1973).

⁴⁹Ostrom, esp. pp. 49-50.

⁵⁰ Merewitz and Sosnick, p. 63; emphasis added.

⁵¹Francis W. Coker, "Dogmas of Administrative Reform," American Political Science Review, 16 (August 1922), 399-411; Charles S. Hyneman, "Administrative Reorganization: An Adventure into Science and Theology," Journal of Politics, 1 (February 1939), 62-74; Earl H. Latham, "Hierarchy and Hierarics," Employment Forum, 2 (April 1947), 1-6; and Herbert A. Simon, "Proverbs of Administration," Public Administration Review, 6 (Winter 1946), 53-67.

⁵² Robert T. Golembiewski, Men, Management, and Morality (New York: McGraw-Hill, 1965); Alfred D. Chandler, Jr., Strategy and Structure (Cambridge, Mass.: The M.I.T. Press, 1962); and E. Raymond Corey and Steven H. Starr, Organization Strategy: A Marketing Approach (Boston, Mass.: Graduate School of Business Administration, Harvard University, 1971).

which the scale of public organizations is to be determined, *inter alia*, by the criteria of control and the "democratic ideal of local self-determination." There are two basic difficulties. First, Ostrom provides little or no guidance as to appropriate decisions when choices between the criteria must be made. As Baker asks:

How does one account for the scale of a public enterprise which, in the process of "internalizing" the consequences of producing or providing a public good or service under the criterion of control, exceeds the boundary conditions necessary to achieve optimal economies of scale requisite under the criterion of efficiency? The boundary conditions required for regulation purposes may not coincide with the boundary conditions necessary for obtaining optimal economies of scale. Or in attempting to meet the requisites of the criterion of political representation, how does one make coterminous the scale of formal public organization, the public affected, and the political community?⁵³

Since these choices always must be made, and since they are often fiendishly complex ones in the bargain, it is not useful to prescribe a decision rule which is mute about them.

Second, it is not clear how a kind of runaway-train effect can be avoided or stopped once it starts. If "control" takes precedence over the other criteria in Table 1, as it well might, there are no obvious limits on the scale of a public agency. The need to prescribe levels of organization compounds this problem. Assume, for example, that it is possible to define some optimum size for a federal land reclamation project in Arizona, using "cost calculus" or whatever. Still open are questions of the size of the regional suborganization of which that project may be a part, of the size of the federal cabinet-level organization of which that region may be a part, and so on. What is clearly "local" to one of these levels of aggregation may be cosmopolitan to some, and yet impossibly provincial to still others. In such a case, moreover, "control" may take on more compelling qualities than "local self-determination." This will especially be the case if one of the "economies of scale" is the ability to compete successfully for funds at the congressional level where, all things considered, being "bigger" may be useful whatever the scale of enterprise consistent with "local self-determination." In addition to monitoring or even coercing lower cadres, adding overhead levels also is motivated by many other factors, including: broader

Some Awkward Probable Consequences. Worse still, Ostrom's approach and that of the public policy theorists can have major consequences opposite those intended. Four main points will help illustrate the broader family of unanticipated consequences:

- that decentralized government is not necessarily better, more democratic, more moral;
- that the case for smaller organizations being more efficient and responsive is far from clear;
- that conditions of resource scarcity or resource affluence are critical in conditioning the choice of a shifting balance of government interventionism or of centralization/ decentralization;
- that what may be called "cycles of governance" can be distinguished as cycles to which public policy must be sensitive.

These examples of unexpected and unintended consequences imply that the ideational infrastructure of public choice is not yet ready to provide a framework for public policy.

Is Decentralization Better, More Democratic, More Moral? One nativist thrust of the public choice position is that "decentralization" is preferable to a unitary hierarchy responsible to one man. The logic underlying this basic bias of public choice theory seems clear enough. Individuals possess only "bounded rationality," which means no one decision maker can pretend to a broad knowledge of utility preferences needed to achieve maximum efficiency. Hence the preference of public choice theorists for decentralized price or market systems, because they imply fewer demands on the decision maker by reducing the scale of the system of choices to be coped with.

There is no objection to this emphasis in public choice theory, if it is not pushed overly far. Indeed, the management literature had come to a similar conclusion long before, beginning at least in the 1920s with such powerful statements as Mary Parker Follett's "The Allusion of Final Authority." Significantly, moreover, Follett's central contribution was reprinted in that seminal collection by Gulick and Urwick, Papers on the Science of Administration (1937). It is true, however, that the impact of that conclusion about executive

perspective; taking the heat attendant to a decision that should be made but which local officials fear making because it violates some powerful interests or figures; and so on through a very long list.

⁵³Baker, pp. 51-52.

overload had been only partially felt in public administration or political science, where the "imperial presidency" was reaching new heights.

Public choice theorists are not restrained in this regard, however, and neither is Ostrom's political theory. "Taste" or "preference" is the basic value surrogate in public choice theory; and decentralization of government plays much the same role in Ostrom's thought. Thus public choice theory avoids this question: Whose preferences about what? And Ostrom usually neglects a similar question: What degrees of decentralization, when, for which purposes? To be fair, there are points where Ostrom argues that a "variety of different organizational arrangements can be used to provide different public goods and services." But Ostrom is on balance not very interested in that "variety."

Decentralization is far from simple, but some brief and meaningful things may be said about it here. Primarily, public choice theorists see centralization and decentralization as opposite extremes of a continuum. More useful is a tripartite notion, such as that shown in Figure 1. Public choice theorists generally, and Ostrom specifically, neglect this distinction. One danger is that their strong preference for decentralization may result in chaotic localism which, in turn, may only make more probable the very centralization which public choice theorists say they abhor. Indeed, the extreme outcome could be a Hobbesian "war of all against all," to which centralization would be among the more benign responses. Given chaotic localism, to explain, effective powers are lodged in small units of government wherein local minorities may be poorly treated, policies can vary enormously between local constituencies, and so on. Such a consequence of championing decentralization without reference to the values to be served thereby, for example, occurred when race relations were considered a local matter. There are two probable outcomes in such cases: the need deprivation will persist over long periods of time; and superordinate units of government will centralize decision making of the issue in question, all the more decisively the longer the status quo persists. Neither outcome is desired by public choice theorists, presumably.

Neither Ostrom's nor the public choice logic seems to permit any way out. To illustrate, the distinction between the two brands of decentralization—that is, "chaotic localism" and "ef-

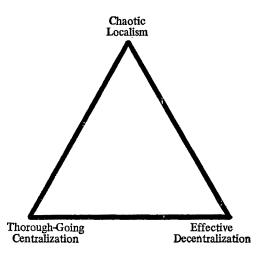


Figure 1. Schema of Forms of Delegation

fective decentralization"—clearly rests on value judgments. However, public choice theory overtly seeks to avoid most value judgments. Hence, by implication, the failure to distinguish between varieties of decentralization rests on the failure to specify values. This may similarly explain the failure to stress that centralization at times might serve important values.

This elemental sketch illustrates the conceptual difficulties about which careful observers like Fesler sought to warn those zealously committed to decentralization as a doctrinal summum bonum. Their warning stresses three points. First, the preference for centralization/ decentralization cannot be a matter of either/ or, but should be a dynamic, evolving, and oscillating process of fine-tuning institutions to current problems. Failure to recognize the point implies only trouble, if not an Alice-in-Wonderland state where even benign intentions yield unwanted effects. As Friedrich notes: "This continual change is a matter of fact; but unless it is clearly recognized and institutionally provided for, all decentralization of power . . . [is] apt to become [a] source of tension and conflict."55

Second, it is specious to link democracy, freedom, and decentralization, as Ostrom does⁵⁶ in common with much of the public choice literature. This easy marriage neglects a reality that often has been painfully obvious. As Hart notes, "Democratic processes do not necessarily result from, nor are democratic

⁵⁵ Carl J. Friedrich, Man and His Government (New York: McGraw-Hill, 1963), p. 667.

⁵⁶Ostrom, esp. pp. 75–81.

1 47. 44

ideals necessarily maximized by, decentralization. In fact, the opposite has often occurred and independent local political structures have worked against democratic ideas."57 Moreover. the linkage of decentralization with democracy and freedom equates values with means and also creates major analytical and practical difficulties. We cannot improve on Fesler's formulation: "In extreme principle [such a conceptual linkage) can mean anarchy; and in muddled practice it can be so closely associated with islands of group or community self-determination that subordination of the individual to a local autocracy may be lost from view, as can the potentially liberating influence of a national leadership committed to development of social and economic opportunities for individual achievement of personal goals."58

Third, the doctrinal approach to decentralization neglects the critical role of the central government in facilitating movement toward those goals which decentralization is intended to achieve. Briefly, decentralization implies a viable definition of the value parameters within which local governments can act without becoming so balkanized that the necessary context of unity about fundamentals is destroyed, that a "chaotic localism" comes to be and persists. Without such a "viable definition of value parameters," decentralization could equally serve revolutionary as well as reactionary politics, with the potential in extremis for polarization such as that which led to the Civil War. In Fesler's more restrained and detailed language:

National legislation, overriding local objections and implemented by national administrative action, is often required to democratize the selection of local officials, to establish viable units of local government with the size, resources, and diversity of interests that are preconditions of effective local self-government, to recruit and train skilled staff for local administration, to minimize corruption and regularize fiscal practices, and to provide grants from national revenue to help finance the impoverished communities. ⁵⁹

Patently, these interventions by the central government are not consistent with a doctrinal vision of the decentralized polity, even if their motivation (as Fesler notes) is "often to save local self-government from itself."

Are Smaller Public Organizations More Effective and More Responsive? Perhaps the most dominant emphasis in Ostrom's thought is that "smaller government" will bring more effective and responsive provision of services, and Ostrom thereby sets himself a substantial challenge. To be sure, every age seems to have its retrospective vision of an uncomplicated and pleasing "prior condition," a kind of nice sociopolitical predecessor of extant nastinesses and nuisances. Thus there was the Garden of Eden and Marx's vision of preindustrial society; more recently, the vision of a nation of "family farms" sustained many; and there is an important theme of back-to-nature in some of today's communal experimentation.

Such nativist traditions notwithstanding, the vast bulk of the literature gives small scale government no clear edge in being better, 60 more democratic, 61 or more moral, 62 (given a gentle fuzziness about what "small scale" really means); quite the opposite, in fact.

Let us begin with a debator's point or two. For one thing, Ostrom is never clear on what "small" is. Nor does it do for him to praise "smaller" more than "larger," thereby seeming to avoid the issue. For once any organization gets larger than one in which cozy face-to-face interaction is both possible and convenient, major differences in communication and influence patterns quickly develop.63 Severe problems also relate to Ostrom's very general position that the appropriate scale will vary "with the boundary condition of different fields of effects inherent in the provision of different public goods and services."64 The word "inherent" rankles. Only complex mixtures of empirical conditions and value positions will define what is an "optimal scale." There is no more an inherent optimal scale for a public agency than there is an inherent optimal family size independent of complex empirical condi-

⁵⁷David K. Hart, "Theories of Government Related to Decentralization and Citizen Participation," *Public Administration Review*, 32 (October 1972), 606.

⁵⁸James W. Fesler, "Approaches to the Understanding of Decentralization," Journal of Politics, 27 (August 1965), 549. For a balanced view deriving from extensive empirical research by one of Fesler's students, see Douglas Yates, Neighborhood Democracy (Lexington, Mass.: Lexington Books, 1973).

⁵⁹Ibid., p. 549.

⁶⁰Herbert Kaufman, The Limits of Organizational Change (University, Ala.: University of Alabama Press, 1971), pp. 101 ff.

⁶¹ Fesler, esp. pp. 539-46.

⁶²George Graham, Morality in American Politics (New York: Random House, 1952), esp. pp. 42-53.

⁶³Arnold S. Tannenbaum et al., *Hierarchy in Organizations* (San Francisco: Jossey-Bass, 1974).

⁶⁴Ostrom, p. 118; emphasis in original.

tions and especially value preferences. It all depends—within very wide limits.

But let us put aside such initial if basic concerns. What is Ostrom's argument for small (or smaller) public units? The preference in public choice theory and Ostrom for smaller units is offered almost as a matter of fact, a given beyond any serious question or qualification. Or if that is too strong, the arguments of public choice theorists like Ostrom are far from convincing, and the available empirical literature provides (at best) mixed support. 65 In the main, Ostrom refers to three basic public service areas—education, police, and fire—and provides some data about the last two in seeking to establish that smaller is better. Reviewing his evidence will be instructive.

Big city education is in a turmoil, Ostrom notes, which he attributes mostly to large and centralized school systems which are unresponsive to citizen preferences or tastes. This attribution is minimally buttressed, with an unseemly neglect of factors such as race, dislocated families, decaying center cities, plummeting tax bases and escalating public services, and so on. Not mentioned, also, is the fact that a common rationale for larger school systems rests on the perceived failure of smaller systems to do the required job.

Ostrom's contention that smaller is better in big city education is coupled with a straightforward remedy, but not a convincing or even an entirely consistent one. As for the remedy, Bish and Ostrom note that the "big city school systems are too large, and they should be broken up into smaller systems serving children in more homogeneous neighborhoods." Decen-

65For example, Harry P. Pachon and Nicholas P. Lovrich, "The Consolidation of Urban Public Services," Public Administration Review, 37 (January 1977), 38-47, provide evidence which contradicts Ostrom's position with respect to large vs. small police systems. Such studies reinforce the notion that any relevant comparisons between large vs. small public agencies are likely to be very complicated, contingent on intervening variables, and so on. This certainly has been the case with industrial and commercial organizations. For example, substantial evidence implies that total organization size is likely to be less salient than the size of component "managerial units," a managerial unit being defined as that portion of an agency monitored by some authoritative agent who can make reasonable decisions about some total "flow of work." For the conceptual argument and a review of substantial evidence, see Golembiewski, Men, Management, and Morality, pp. 113-15, 133-38, 144-47, 234-43, 268-71, and 279-83.

Size of managerial unit is not dependent on size of

Size of managerial unit is *not* dependent on size of organization, but will vary with the degree of decentralized delegation in an organization. A decentralized organization, however, could be huge in total size.

tralized school districts, they conclude, "would give center city residents control over their own schools such as that presently enjoyed by suburban residents." The remedy has some troublesome features, however. For example, research suggests that voter turnout is lowest in the smallest jurisdictions, hence smaller scale may generate effects counter to those Ostrom hopes for and expects. This contrary consequence is no doubt due to many factors, including the media's regional and national focus.

Decentralization of schools-whether by enhanced delegation of authority within a system or by making several smaller systems out of one larger one, a distinction Ostrom does not make consistently-does not clearly affect such factors as media biases or rates of participation in campaigns. And such factors need direct confrontation by those presenting decentralization as a more or less unqualified good. Moreover, Ostrom's remedy is basically inconsistent with the massive Coleman report which-despite its own internal limitations-convincingly and persistently traces educational outcomes to the quality of the home and of the immediate environment of students.68 Finally, Ostrom does not follow his own advice in some particulars, at the cost of possible inconsistency. "Accompanying decentralization would be expanded area-wide or state-wide financing, Bish and Ostrom note, "so that poorer districts would be able to provide a greater level of support for education. Wealthier districts would be free to tax at higher rates and provide more educational inputs above that general level."69 Bish and Ostrom do not deal with possible inconsistencies arising between area-wide or state-wide financing and greater local control over schools.

Ostrom's case against centralized police service is similar, 70 but is presented with more supporting data. Three empirical studies are referred to, and their results indicate that services of larger police departments (in the small sample used) cost more per capita, 71 and

⁶⁶Bish and Ostrom, p. 38. See also pp. 36-37.

⁶⁷ James A. Reidel, "Citizen Participation: Myths and Reality," Public Administration Review, 32 (May 1972), 211-20.

⁶⁸ James S. Coleman et al., Equality of Educational Opportunity (Washington, D.C.: National Center for Educational Statistics, 1966).

⁶⁹ Bish and Ostrom, p. 38.

⁷⁰Ibid., pp. 41-46.

⁷¹Elinor Ostrom et al., Community Organization and the Provision of Police Services (Beverly Hills,

that residents of small, independent communities seem more satisfied with the service their police departments provide. 72 These empirical efforts are to be applauded and encouraged, but they do not yield unequivocal interpretation. The central problem is that the underlying cost/benefit analysis is quite incomplete. Larger police departments studied did cost more per capita in two of the three cases, but no one knows whether or not they yielded commensurate benefits. It does seem that residents or larger jurisdictions have less favorable attitudes toward their police but, as important as this measure may be, it clearly falls far short of a complete measure of benefits. Calculating these benefits would be very difficult, but that alone can permit specific comparisons of the kind required. Moreover, it is anybody's guess as to how stable such attitudes are, how deeply they are rooted in experience and reliable information, and to what degree individual judgments of police efficiency/effectiveness are made in terms of values or goals that relate to the full range of police work. Such differences should be important in political debate and dialogue.

The evidence about fire services is limited to one study, which supports a conclusion opposite the one advertised: smaller is not clearly better. To explain, Bish and Ostrom cite Ahlbrandt's study⁷³ of fire protection in Scottsdale, Arizona, Ahlbrandt provides important evidence supporting the efficacy of unorthodox arrangements, for Scottsdale's fire services are provided by a business firm which seems to do an innovative and economic job when its costs are compared to reasonable estimates of what the costs would have been if the service were provided by a traditional fire department. Ostrom proposes that this is due to the fact that the firm can be unbuckled by Scottsdale if it does poorly, a pressure for performance not felt so directly by a traditional fire department. Perhaps this is so, although one could imagine alternative scenarios in other similar cases. Sweetheart arrangements between a firm and local politicians are possible, for example. Such possibilities aside, moreover, note that at least part of the firm's advantage comes from the fact that it serves not only Scittsdale, but "several other cities and unincorporated areas in Arizona."74 Smaller is not necessarily better, that is to say, and it is incautious to compare the firm's costs with those of a traditional fire department restricted to Scottsdale. Moreover, one could argue that the fire service was efficient precisely because it followed traditional principles in the sense of serving multiple cities, even as Scottsdale chose not to respect the principles by opting against including a municipal fire-fighting service in its organizational structure.

Indeed, when all is said and done, large scale is not the only (or perhaps even the central) issue for Ostrom. Thus in big city education he himself identifies three "causes of problems... professionalization, large scale, and uniformity." At other points, he observes that the "combination of professionalization and unionization has [made] organized teachers... relatively invulnerable to the directives of elected officials." 76

There is a wicked probable paradox in all this: that the emphasis on smaller scale not only may fail to achieve Ostrom's goals, but that it also may be counterproductive in his own terms. The point is meant in several senses. Even if we grant-though it is neither obvious nor necessarily true—that the development of narrow professionalization, unionization, and uniformity are causally related to size of school systems, it is doubtful whether those conditions would disappear if the size of (let us say) school systems were reduced. Will the scale or policies of professional associations or unions, especially at state or national levels, be very much affected by such reductions? Not likely. The more probable consequence, in fact, is a substantial increase in the power of professional associations, unions, and government bureaucracies vis-à-vis the elected or appointed heads of the now smaller school systems. This is precisely opposite Ostrom's stated intentions. of course. Similarly, it is not clear that smaller scale implies less "uniformity," especially if by that term Ostrom excludes nonuniformity of the chaotic localism variety. Indeed, some evidence suggests that a substantial take-off size is required for many diversified educational

Calif.: Sage Publications, 1973); and Elinor Ostrom and Gordon Whitaker, "Black Citizens and the Police" (paper prepared for the Annual Meeting of the American Political Science Association, Chicago, 1971).

⁷²S. T. IsHak, "Consumers' Perceptions of Police Performance: Consolidation vs. Deconcentration" (Ph.D. Dissertation, Indiana University, 1972).

⁷³Roger S. Ahlbrandt, Jr., Municipal Fire Protection Services (Beverly Hills, Calif.: Sage Publications, 1973).

⁷⁴Bish and Ostrom, p. 47.

⁷⁵Ibid., pp. 40–41.

⁷⁶Ibid., p. 36.

programs, 77 such as those for exceptional children, for organization development in schools, for various forms of experimentation with curricula, forms of governance, and so on. Finally, "uniformity" need not always be an evil to be avoided, as in determined consensus about the pursuit of high quality educational outcomes.

The rigorous comparison of larger vs. smaller governmental units is extremely difficult, so it is understandable that Ostrom provides no convincing comparisons. But it is inadequate to argue, as Ostrom basically does, that smaller is preferable to larger in governmental units because, inter alia, the latter lack a market with its "competitive force," thus are less likely than businesses to be aware of "diseconomies of scale," and hence will grow large rather than wisely or well. That may be so, although some would argue that the business of government is different enough to preclude strict comparison. But it would have been useful to do more than proclaim the point, as in explaining why an organization like AT&T-which lacks a market in Ostrom's sense-is not only mammoth but effective as well as innovative. Much might be learned from such obvious cases. Or perhaps Ostrom could have explained the substantial differences in the effectiveness of similar-sized units within federal agencies like the Social Security Administration, since neither those units nor SSA possess markets. Such analyses would be critical in developing a full-fledged theory of the covariants of the effectiveness of public organizations, which is what we need.

In contrast to recommending such complex and demanding questions for analysis, Ostrom basically presumes what is at best a very complicated proof, and without acknowledging that "business effectiveness" does not necessarily equal "government effectiveness." And he is content with such general statements as "opportunities for improvements in efficiency through increased size will have been exhausted for many services in communities that reach a size of 50-150,000 population."79 Among the signs he reads as portents of institutional failure

77It is also correct that larger urban schools do pose more challenging targets for change, in part because of the organizing concepts prevalent in them, as Ostrom argues. See Richard A. Schmuck and Matthew B. Miles, eds., Organization Development in Schools (Palo Alto, Calif.: National Press Books, 1971), esp. pp. 236-37.

of massive proportions, and which he attributes to large size, 80 Ostrom notes that streets "can no longer be kept clean in many areas" of New York City and that Los Angeles school administrators "can no longer cope with cockroaches."

Is Noninterventionism Clearly More Appropriate When Resources Are Growing Scarcer? Perhaps the most subtle snare in the public choice argument inheres in its assumption of relative resource affluence and in the related prescription for governmental nonintervention. Despite a variety of conceptual and practical attractions, the public choice argument seems well designed to generate awkward unintended consequences, even opposite those desired. This could happen because that argument basically prescribes a decision making pattern appropriate to an extractive or exploitative phase of economic development where great or even apparently limitless resources are there for the

The proof of this broad contention requires some stage setting. Consider the methodological individual intent on "individualistic choice." As Ostrom explains:

Individualistic choice occurs whenever each person is free to decide for himself in the pursuit of his own interest. Individualistic choice is characteristic of the market and occurs whenever the only requirement is the willing consent of those individuals who freely agree or contract with one another to exchange some good or undertake some action. 81

This multiple higgling and piggling is crucial, for Pareto optimality can be achieved only through market dynamics. A Pareto optimum is defined to exist after some transaction if and only if, in their own judgments, at least one party of the transaction is better off and not even a single party is worse off as a consequence. The essential argument is not complex. A Pareto optimum is the ideal outcome; hence a market is indispensable and thus also is the minimum of government intervention the preferred strategy.

The summary does not do full justice to this central position of the public choice argument, but it does suffice to illustrate several of its related weaknesses. Four of these weaknesses will be introduced here, by way of brief illustration of the far broader family.

First, Pareto optimality seems increasingly unapproachable in the world as we are coming

⁷⁸Ostrom, p. 59.

⁷⁹ Ibid., p. 119; emphasis added.

⁸⁰Ibid., p. 121.

⁸¹ Ibid., p. 56.

to know it: with potentially chronic energy deficits compounded by OPEC policy; apparently shrinking reserves of some commonly used raw materials; with that "revolution of rising expectations" of so many more than ever expecting so much more than before in terms of wages, goods, and public programs; and with the demands of some efforts (e.g., at pollution abatement) being at cross-purposes with historic expectations about reasonable prices or costs, return on investment, levels of employment, uses of energy, etc. That is to say, a Pareto optimum implies a sufficiency of resources, if not an overabundance. Otherwise, a Pareto optimum would be unlikely or impossible, and individuals pursuing it might succeed only in impoverishing one another. For if resources are scarce-and especially if they are growing scarcer and/or more expensive quickly-log-rolling or market dynamics will typically result in many or even all parties to a transaction being worse off than before. Thus Shepsle notes that the only cases in which the Pareto rule applies are "free of conflict," which hardly constitutes "a very political situation."82 Similarly, van Dalen concludes that the rule is a "fair-weather friend" which has the limiting quality of applying only "when times are good, very good."83

The point can be approached from another angle, with implications far beyond the Pareto optimum. The politics of bargaining-incrementalism require a definite "morality of restraint," with restraint being defined in terms of the delicate balance between expectations that are realistic or even modest in light of existing resources. As Gawthrop notes, "Self-control, moderation, and circumspection are the cardinal virtues of prudential politics," and these qualities cannot be limited only to professionals. Broadly, the required condition is that bargaining-incrementalist "expressions would be-indeed, must be-made within the given limits of the rules of the game."84 Present conditions-more individuals and group than ever before making greater and more insistent demands for resources that are not being increased rapidly enough, and are even being depleted-are precisely those which encourage

testing, or breaking, those "given limits" of self-control, moderation, and circumspection. Individualistic choice and its associated Pareto optimum thus add fuel to what is already a roaring blaze of newly heightened expectations, which the times may little need.

In fact, Ostrom recognizes a similar special case, and he graphically sketches the probable consequences of an unrelieved emphasis on individualistic choice. His special case is a "common-property resource with a renewable yield or supply," such as a water supply. Individualistic choice, Ostrom informs the reader, "has great advantages in reducing the costs of entrepreneurship so long as supply exceeds demand." The "so long as" is crucial. When demand exceeds supply, and as each individual pursues narrow self-interest, so will the supply diminish and so also will the social costs of the community of users increase. Should voluntary actions to limit demand be agreed to by some, this would only encourage others to pursue a hold-out strategy and thus to profit disproportionately from the limited usage by others. Bad can easily lead to worse, Ostrom advises; not only will the community of users suffer a loss, but other more tragic consequences may be expected. "Individuals in weak economic positions will be forced out," Ostrom notes. "The neighborhood effects which are generated may include poverty, deprivations, threats, and even violence."85

A second weakness is that public choice theory's strong preference for decentralization also is ill-suited to growing-and even less to chronic-resource scarcity. Allocations of relatively plentiful resources among people with moderate, even if growing, expectations as to what they have a legitimate claim on constitute a tame class of problems, and it is to this class that public choice theory applies more directly. In contrast, really scarce resources will (as Baker notes) "require more direct policy responses to the question of who gets what,"86 which implies critical value issues and the differential weighting of contending tastes and preferences that exist, that might come to exist, or whose development should be encouraged.

Third, a pursuit of Pareto optimality implies a very restricted range of reactions to problem situations. Consider the case of the polluter, as developed by De Gregori. He explains that a Pareto optimum could be achieved only if the victims of the pollution "voluntarily assessed

⁸²Shepsle, p. 30.

⁸³Hendrik van Dalen, "Some Theoretical Perspectives on Government's Role in Consumer Protection" (paper prepared for the Southwestern Public Administration Meeting, Phoenix, Arizona, Nov. 12, 1975), p.

⁸⁴Louis C. Gawthrop, The Administration Process and Democratic Theory (Boston: Houghton Mifflin, 1970), pp. 439, 439-40.

⁸⁵Ostrom, pp. 56-58. 86Baker, p. 59.

themselves and bribed the polluter not to pollute or to pollute less." De Gregori explains:

All actions would have been voluntary, and consequently everyone would have acted so as to make their condition better, not worse. Any solution that required the polluter to pollute less would not fit Pareto because the polluter would be worse off, because he would have either had to absorb the cost directly or, in passing the increased costs on, lower the quantity demand of his product and therefore lower his profit.⁸⁷

There are paradoxes in such "solutions." For example, this approach to alleviating pollution would be likely to aggravate it. For if you bribe a polluter to pollute less or not at all, it may encourage him initially to pollute a great deal in order to increase the bribe. Indeed, the improbability of such voluntary action implies that the probable action in such cases will be no action, at least until matters get very bad.

Fourth, the theme of government nonintervention is not meant lightly in public choice theory, as in the Pareto optimum. Baker concludes that the theory's basically noninterventionist approach to making public policy is based "upon the perspective of the representative individual in combination with a stated preference for decentralized markets as the best means of producing and providing public goods and services." The theory, Baker notes, prescribes "unfettered forces of a laissez-faire market for the purpose of determining the substance, scope and direction of public policy."88

Some public choice zealots push noninterventionism even further, much further. Consider government interventions intended to get manufacturers to produce safe products and to accept liability. Some public choicers argue that even such interventionism is ill advised, since (in McKean's words) the "only thing that would happen to the consumer's position is that he would be denied the opportunity of taking the risk." The underlying logic is instructive. McKean explains that "some consumers, especially ... the poor," would prefer the risk rather than the greater cost of a safer product. Any shift toward safer products, he concludes, would then work "to the detriment of the poor."89

The bias of public choice theory toward noninterventionism at least bypasses major questions historically at the heart of the debate over what constitutes the just state. These include: Are all "preferences" or "tastes" to be evaluated equally? Does the state direct attention to the consequences for others when some act on their own preferences? What is to be done when one segment of the citizenry comes to feel that their preferences conflict in serious ways with the preferences of others? Etc., etc.

An extended comparison will develop the point that something of substance is lost in this bypass, which derives basically from the simplification of values at the center of the public choice argument. In public choice theory, a kind of "unseen hand" is said to be generated by individual choices which determine what public goods and services are provided. What is to be done when some historical policy consequences have developed in unattractive or undesirable ways, as in racism or sexism or thalidomide or whatever? Baker worries that public choice theory would be impotent in doing much about such issues, or perhaps not even recognize them. More broadly still, Baker concludes that public choice theory "allows, if not encourages, racial, sexual and other forms of discrimination." If no distinctions are to be made between preference patterns, it appears that the theory provides no way of distinguishing (let us say) a preference for "my kind of soap" from a preference for "my kind of people," with all the prejudicial possibilities implied thereby.90

Do "Cycles of Governance" Make a Difference? Another major source of probable but unintended consequences for public choice theory inheres in its broad assumption that there is a one-best approach to governance, which in turn presumes a kind of standard set of governmental issues or challenges.

Granting the convenience of this assumption, it appears to be seriously inadequate. In Kaufman's terms, for example, it is necessary at a minimum to distinguish three major emphases in our public administration history: (1) representativeness; (2) politically neutral competence; and (3) executive leadership. "None of these values was ever totally neglected in any of our past modifications of governmental design," Kaufman notes, "but each enjoyed greater emphasis than the others in different peri-

⁸⁷De Gregori, p. 210.

⁸⁸Baker, p. 54.

⁸⁹Roland McKean, "Product Liability," Quarterly Journal of Economics, 84 (November 1970), 623. Most public choice theorists incline to somewhat more government intervention, as in providing information about product safety. See W. Y. Oi, "The Economics

of Product Safety," Bell Journal of Economics and Management Science, 5 (Spring 1973), 26.

⁹⁰Baker, p. 55.

ods." And that emphasis on one value at a time, in effect, allows discontents to develop in areas poorly served by some one value. As Kaufman explains: "At different points in time, enough people (not necessarily a numerical majority) will be persuaded by one or another of these discontents to support remedial action-increased representativeness, better and politically neutral bureaucracies, or stronger chief executives, as the case may be." The condition is far from static. Fixation on one of the classes of discontent allows other discontents to accumulate and, sooner rather than later, this motivates a major change in emphasis. As Kaufman concludes, "no totally stable solution has yet been devised. So the constant shift in emphasis goes on."91

These shifts in the cycle of governance are responses to broadly felt changes in central clusters of problems. Approaches and structures sensitive to one cluster of problems are likely to be unresponsive-if not seriously counterproductive-to the second or third clusters. Adaptive slippage is likely to increase under two conditions: when the tasks of governance are seen as homogeneous and constant over time; and when zealous allegiance is given to approaches and structures appropriate for only one of the three cycles of governance.

To translate, Ostrom's prescriptions seem intended to emphasize representativeness as a central value. But those prescriptions are variously out of potential kilter with the goal of a competent public service and more seriously at odds with the central value of executive leadership.

Zealous commitment to public choice theory, that is to say, may prove too much of a good thing as conditions change, which they almost certainly shall. It is well and good, consequently, for public choice theorists to propose for argument and debate that the American public economy was intended to avoid concentrations of power, that it has prospered because of that basic structural bias, and that this bias should continue where it exists, be reinstituted where backsliding has occurred, and be inaugurated wherever it has not existed. But it is awkward to advocate the bias without conditions, as awkward as it is to argue that reliance on markets or decentralization is always utopian and unrealistic. The awkwardness has two roots: Sensitive adjustment to changing conditions may be complicated, if not precluded, by the emphasis on a structuring bias, and, relatedly, participants may become so intense about either/or arguments that the goals of attack/defend get more attention than truth seeking and problem solving. Waldo nicely captures the essence of both awkward consequences. He worries about an outcome like that in classical tragedy, "wherein all the protagonists are 'right', but tragedy ensues." Waldo urges that we seek to avoid, "from the conflict of two legitimate principles, a denouement that could be tragic for all."92

Conclusions

The preceding analysis should suffice to support three major points.

- 1. Sufficient methodological inelegancies encourage great caution in accepting Ostrom's democratic administration and the ideational infrastructure on which it rests.
- 2. Moreover, sufficient reason exists to look critically at any prescriptions for action derived from public choice theory. Not only are a number of its major assumptions either questionable or clearly inadequate as empirical descriptions of existing conditions, but also it seems that public choice theory is likely to generate major unanticipated consequences, some of them directly contrary to intended consequences.
- 3. Finally, this critical survey does not support the either/or contraposition attempted by Ostrom. Indeed, Ostrom himself at times comes close to advocating a contingency approach. Thus he observes that, "It is the exigencies of national defense that peculiarly require a system of one-man rule inherent in the prerogatives of the commander in chiefnot the exitencies of community assistance programs, rural development, poverty, or the public security of neighborhood streets." ^{9 3} But Ostrom does not go on to detail the classes of exigencies to which various approaches/structures could be shown to be variously appropriate by a range of empirical tests. This would have directed attention away from polarizing and polemical formulations, and would have encouraged describing the conditions under which differing approaches to providing public goods and services would lead to which specific consequences. This would have put the emphasis where it belongs, in short.

⁹¹ Herbert Kaufman, "Administrative Decentralization and Political Power," *Public Administration Review*, 29 (January 1969), 3 and 4.

⁹²Dwight Waldo, ed., Public Administration in a Time of Turbulence (Scranton, Pa.: Chandler, 1971), pp. 259-60.
93Ostrom, p. 125.

Some Problems in Doing Political Theory: A Response to Golembiewski's "Critique"

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Robert T. Golembiewski's "A Critique of 'Democratic Administration' and Its Supporting Ideation" serves a useful purpose. It challenges some assumptions of public choice theory as well as some of my arguments about democratic administration as an alternative to bureaucratic administration. Golembiewski's "critique" is representative of various criticisms that have been made of public choice theory. A response to each point raised is not feasible in this essay. I shall respond only to major issues.

Several issues are raised that are central to the task of doing political theory. These include the general issue of methodological individualism, the related assumptions about self-interest and preference orderings, and the place of values, efficiency, and Pareto optimality. These issues are, in turn, imbedded in a more general problem: that of using language as a tool for theoretical inquiry and testing the usefulness of different conceptual languages for generating inferences and researchable hypotheses.

The language problem gives rise to a serious potential for misunderstanding. Many conclusions that Golembiewski attributes to me, for example, are not my conclusions and do not follow from the conceptual language that I use. In translating some of my arguments into his language, Golembiewski says something different than I have said. In some cases the virtual antithesis is asserted. In other cases, targets of convenience in public choice theory or economic theory more generally are used to condemn by association without critically examining the relevant issues in my own work. These problems will become apparent in the

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course of this essay. Before turning to some methodological issues in doing political theory, I need first to establish the context for Golembiewski's "critique" and my response.

The Context

Golembiewski refers to three works that I have authored or co-authored. His primary reference is to The Intellectual Crisis in American Public Administration, published in 1973. There, he focuses almost exclusively upon the third chapter-"The Work of the Contemporary Political Economists"-and, within that chapter, upon the two pages concerned with a model of man. His Table 1 is derived from "The Organization of Government in Metropolitan Areas: A Theoretical Inquiry," co-authored with Charles M. Tiebout and Robert Warren and published in the December 1961 issue of the American Political Science Review. The third reference is to Understanding Urban Government: Metropolitan Reform Reconsidered, co-authored with Robert Bish and published in 1973.

The only reference Golembiewski makes to the chapter on "Democratic Administration" in the Intellectual Crisis is a footnote to an assertion that "it is specious to link democracy, freedom, and decentralization as Ostrom does in common with much of the public choice literature" (1977:1500). That chapter focused largely upon formulations advanced by Alexander Hamilton and James Madison in The Federalist, and by Alexis de Tocqueville in Democracy in America. Golembiewski identifies the supporting "ideation" for democratic administration exclusively with public choice theory. Instead, I observed:

The work of Hamilton and Madison and of Tocqueville involved the articulation of a theory of democratic administration when measured in terms of the criteria specified by Max Weber. The American experiment, based upon a theory of democratic administration, can thus be viewed as a turning point in pioneering a new course of human development. Democratic administration, through a system of overlapping

jurisdictions and fragmentation of authority, acquired a stable form which provides an alternative structure [i.e., to bureaucratic administration] for the organization of public administration (V. Ostrom, 1974:97–98).

My intellectual debt to the work of contemporary political economists or public choice theorists is substantial. Only after several years of collaborating with political economists was I able to reformulate self-consciously my own conceptual language to a point where I could effectively apply economic reasoning to problems of political organization. Much to my surprise, I found upon rereading many seventeenth, eighteenth, and nineteenth century classics in political theory that a similar language and mode of reasoning was used by Alexander Hamilton and James Madison in The Federalist and by Tocqueville, Hobbes, Locke, Rousseau, Hume, and Smith among others. Many traditional political theorists were political economists who used economic reasoning to think through problems of political organization. They saw people as using a cost calculus to choose from among alternative possibilities.

The Political Theory of a Compound Republic, published in 1971, was my effort to expound the basic theoretical argument in The Federalist, on the assumption that its authors used economic reasoning to analyze problems of constitutional choice confronting the American people in the 1787 to 1789 period. Indeed, I am persuaded that the intellectual developments inherent in a theory of federalism and constitutional rule are as fundamental to the potential development of a political science as Adam Smith's Wealth of Nations was to the development of economics. The only difference is that many political scientists, dedicated to the application of natural science methods to the study of artifactual phenomena, have failed to recognize a major intellectual development of Copernican proportions in the history of political thought.2

¹I do not accept the logical positivists' position, contrary to Golembiewski's allegation. Rather, I assume, with Thomas Hobbes, that commonwealths or other forms of organization are artifacts. As artifacts created by human design, organizations necessarily entail consideration of both fact and value. A value-free political science is, in my judgment, an impossibility. Empirical evidence is, however, pertinent to knowing the performance characteristics of an artifact.

²Rousseau, Montesquieu, Locke, Hume, and others anticipated elements in this development; and Proudhon recognized its fundamental importance when he wrote, "The twentieth century will usher in an age of federations, or else humanity will fall back into purgatory for another thousand years" (Simon,

The failure to recognize this fundamental development in political thought can be illustrated by the modern reading of Federalist No. 10. Madison's conception of "a republican remedy for the disease most incident to republican government" lay in "the extent and proper structure of the Union" (Hamilton, Jay, and Madison, n.d.:62). This assertion has been widely translated to refer to the "extended republic," not the "compound republic." The qualification pertaining to proper structure has been ignored. If this contention about the extent of a republic were true, we should expect the Soviet Union to be a less tyrannical republic than the United States.

The puzzle addressed in the Intellectual Crisis is: Why have most American students of public administration in developing their conceptual tools ignored the theory inherent in federalism and constitutional rule and opted for a theory of bureaucracy organized in accordance with what Max Weber has called the "monocratic" principle? (Rheinstein, 1967:349-50.) This puzzle is explained as involving a fundamental paradigmatic choice by early students of political science and public administration when these subjects were first being developed as professional "scientific" disciplines. Woodrow Wilson, as a young and influential political scientist who set the course for the study of public administration, quite explicitly rejected the formulations of Alexander Hamilton and James Madison as "literary theories" and "paper pictures" that did not reflect the "realities" of American politics (Wilson, 1956:30-31).

Public choice theory, with its emphasis upon the nature of goods, added a critical element to contemporary political analysis. Institutional arrangements as one set of variables need always to be related to particular types of goods and services: choice procedures need to vary with the type of goods. The decision-making arrangements characteristic of markets, for example, will predictably fail to cope with the

^{1973:30).} Europe is today fashioning a new community of concurrent regimes, while America builds a new imperialism.

³The bias against federalism and constitutional rule is reflected in the assumption that the primary source of institutional failure in the American political system is associated with overlapping jurisdictions and fragmentation of authority. Federalism necessarily entails overlapping jurisdictions; and constitutional rule necessarily entails fragmentation of authority. The municipal reform and administrative reorganization movement sought to eliminate overlapping jurisdictions and fragmentation of authority.

provision of public goods and the management of common-property resources. An explicit theory of public goods assumed that such goods come in many different sizes and forms and gave a new conceptual impetus to developing a theory of public administration fully consistent with a theory of federalism (Olson, 1969; V. Ostrom, 1969; Tullock, 1969).

Tocqueville's powerful analysis of the system of "decentralized" administration found in America in contrast to the system of centralized bureaucratic administration that he analyzed in The Old Regime and the French Revolution reinforced my own views. Tocqueville would insist that there is an essential association between democracy, what he calls "decentralized administration," and freedom or liberty. Thus, the supporting "ideation" for my thesis regarding democratic administration derives as much or more from Thomas Hobbes, David Hume, Alexander Hamilton, James Madison, Alexis de Tocqueville, and Max Weber as from the work of contemporary political economists or public choice theorists. Each has made fundamental contributions to my consideration of democratic administration as an alternative to bureaucratic administration.5

Some Methodological Issues in Doing Theory

The enterprise of doing theory poses a number of difficulties or problems. Golembiewski seems to assume that the human animal can directly perceive and know "reality" as such. If this were possible, there would be no need for theory. Instead we are forced to rely upon language as a tool for reasoning about and knowing something of the human potential and the universe in which we live. The theoretical enterprise is, therefore, subject to severe limitations. Recourse to theory or "general ideas" as Tocqueville expressed it, is "no proof of the strength, but rather of the insufficiency of the human intellect; for there are in nature no beings exactly alike, no things precisely identical, no rules indiscriminately and alike appli-

cable to several objects at once" (1945:II, 13). Tocqueville recognized that the use of language "always cause[s] the mind to lose as much in accuracy as it gains in comprehensiveness" (1945:II, 13). The use of words or terms in a language of discourse always implies simplification. Human thought as mediated through language systems can never comprehend "reality," only simplifications of "reality." We see the shadows in the cave, not "reality" itself.

While recognizing these severe limitations inherent in doing theory, we still have the problem of how do we orient ourselves to the subject of our inquiry? Where do we begin? What elements do we take into account? What constructs do we use to develop different units and levels of analysis? How do we use basic terms to reason through solutions to problems? How do we anticipate the probable course of events that is likely to follow if theoretical conceptions are acted upon? The usefulness of conceptual language systems can be tested by treating inferences as hypotheses and determining which explanation best holds in anticipating the future course of events. Theory is not an end in itself but a tool that enables human beings to use processes of symbol manipulation and reasoning to solve problems and cope with the exigencies of life.

As a point of departure, I assume that the subject matter of political inquiry is the allocation, exercise, and control of decision-making capabilities among people in human societies.6 Decisions are ordered by reference to rules. Rules are artifacts devised by human beings to create order and predictability in human relationships and to enhance the well-being of those who share in communities of rule-ordered relationships. The order, predictability, and well-being shared in common are public goods-i.e., goods that are subject to joint use where separate individuals cannot be effectively excluded. Since rules are not self-promulgating and self-enforcing, human beings must rely upon the agency of some of their fellow creatures to formulate and enforce rules in relation to the decisions that are taken by others. Serious puzzles about the relationship of rules, rulers, and ruled arise in all human societies and in all forms of human organiza-

⁴Tocqueville in using the term "decentralized administration" refers to the townships, counties, and other local instrumentalities. State legislatures were the relevant centers where control was exercised by reference to the general provisions of state law. Tocqueville's reference is to the American republics—i.e., the states. He has only one chapter that considers the institutions of the national government.

⁵Among earlier twentieth century scholars who made important contributions are John R. Commons, John Dewey, and Mary Parker Follett.

⁶I thus view the major concern in political science to be with institutional variables. I would distinguish this from economics where the subject matter is concerned with the production, exchange, and consumption of goods and services. If similar modes of reasoning can be applied to both types of problems, we have a net gain in developing consistency across the social sciences.

tion. Studies in political theory are broadly concerned with the structure of rule-ordered relationships and the implications that follow from variously structured rule systems.

A basic methodological problem in doing political theory is the task of simultaneously taking several elements into account. Public choice theory always requires that attention be given to (1) individuals or other units of analysis, (2) the institutions that order relationships among individuals or other units of analysis, and (3) the nature of the goods or events that are involved. Any one of these elements may vary. A change in the characteristics of any one element may alter conclusions. In a sense, public choice theory can be viewed as a contingency theory where each statement about institutions, for example, is contingent upon stipulated conditions about the nature of the good or service involved.

Administrative or organization theory, by contrast, usually treats institutional aspects without giving simultaneous attention to the nature of the good or service involved. Instead, a general nomenclature that refers to values, objectives, or goals is used without a taxonomy for distinguishing variable types. Golembiewski fails to discern any predictive power in public choice theory because he fails to give simultaneous attention both to institutional arrangements and to the nature of the goods. Given a common-property resource and free or unconstrained access by many individual users, it is easy enough to predict what Garret Hardin has called the "tragedy of the commons" without having to rely upon crude tautological afterthe-fact justifications (Golembiewski, 1977: 1496).

Any effort to do political theory requires that certain assumptions or stipulations be made about the basic elements to be used in thinking through or solving problems of political organization. It is these assumptions that Golembiewski finds objectionable. Consideration will be given, in turn, to methodological individualism, self-interest, the ordering of preferences, and values, efficiency, and Pareto optimality.

Methodological Individualism. When I rely upon methodological individualism, I assume that individuals are the basic units of analysis in doing political theory. One begins by taking account of some of the essential characteristics of human beings. It is individuals who perceive, think, evaluate, choose, and act. Organizations are nothing more than aggregations of individuals to realize some joint advantage or common good. The basic approach in using "method-

ological individualism" is to take the perspective of representative individuals and think through the implications that follow when they are confronted with the opportunities and constraints inherent in a set of decision rules and in view of the potential payoffs associated with characteristic goods.

Thomas Hobbes is a methodological individualist when he assumes that man is both the "matter" and the "artificer" of commonwealths (Hobbes, 1960:5). The first task for Hobbes in explaining the nature of commonwealths is to treat human nature, as he does in the first 16 chapters of Leviathan. Alexander Hamilton insists that the design and construction of a Federal government must be based upon an individualistic conception of political experience. A Federal government, Hamilton says, "must carry its agency to the persons of the citizens" (Hamilton, Jay, and Madison, n.d.:98). Hamilton further suggests that justice cannot be done unless the actions of government relate to persons in their individual capacities. To apply sanctions to collectivities involves guilt by association.

Golembiewski concedes the essential point in methodological individualism when he observes: "To be sure, only individuals can perceive and make decisions" (1977:1495). If this is so, he is required to take account of individuals as basic units of analysis in his efforts to build a theory of organization. Failure to discipline oneself to take explicit account of individuals as basic units of analysis leads many political and administrative analysts to take on the perspective of omniscient observers. Then they assume that all collective goal-oriented behavior is a rational means-end calculus where people can be ignored. Methodological individualism is quite different from "individualistic choice" despite Golembiewski's identifying the two as equivalent (1977:1493). Individualistic choice refers to decision rules where each individual is free to decide for himself. Methodological individualism can be used to analyze behavior even in the absence of any authority on the part of an individual to decide a course of action. As an individual, I am, for example, not competent to make the decision to involve the United States in a war. War is not a matter of individualistic choice. My response to collective decisions, however, will be affected by my individual calculations.

Using the individual as a basic *unit* of analysis does not mean that one is confined to that *level* of analysis.⁷ The task is to explain

⁷This point is correctly emphasized by Phillip M.

why and how individuals aggregate themselves into collectivities and associations of varying sorts and complexities. I expect aggregation rules to affect the nature of the organization or collectivity that is created. These rules structure the choices and behavioral characteristics of the individuals who function in an organization or collectivity. Aggregation rules are rarely simple summations of individual decisions made by separate and distinct persons. Instead, aggregation rules are those used in an organization to authorize joint action and constrain individual decisions accordingly. In turn, interorganizational relationships result from the internal structure of different organizations and the patterns of interaction among organizations. These are also structured by reference to explicit or implicit rules. Most forms of collective behavior are affected by at least two sets of rules: those governing relationships within collectivities and those governing relationships among collectivities.

In Chapter 3 on "The Work of the Contemporary Political Economists" I drew upon Mancur Olson to demonstrate that "unrestricted individualistic choice in relation to common-property resources or public goods can generate destructive competition where the greater the individual effort the worse off people become." I then showed how individuals can escape this type of prisoners' dilemma by constituting a collectivity that replaces individualistic choice with collective decision rules of less than unanimity (Buchanan and

Gregg (1974) in an essay on "Units and Levels of Analysis: A Problem of Policy Analysis in Federal Systems." Gregg contends that much contemporary research in comparative policy analysis fails to give proper attention to different units and levels of analysis. Gregg states a number of hypotheses derived, in part, from work criticized by Golembiewski that applies to the multiorganizational level of analysis (pp. 79-80).

⁸This is not a special case which indicates the "unrelieved emphasis on individualistic choice" (Golembiewski, 1977:1505) in dealing with all potential resources or goods and services. Rather, it is a paradigm case where we would expect market failure and where the failure of exclusion would require recourse to nonmarket (public) organization. Golembiewski apparently fails to understand the distinction between paradigms as ways of thinking and decision structures as ways of ordering activities or doing things. One paradigm does not entail recourse to only one type of decision structure. Public choice theory is a way of thinking that can have reference to all types of decision structures as a key variable among its analytical elements. The preoccupation in public choice theory is not with markets but with public decision-making arrangements: therefore, public choice.

Tullock, 1962). This solution can be reiterated to derive a federal solution (V. Ostrom, 1973). The system of administration that results can only be understood as a system of multiorganizational arrangements. Much of my own work with its emphasis upon polycentricity (1972), public-service industries (1968, 1971a), intergovernmental relations (V. Ostrom and E. Ostrom, 1965), and federalism (1969) more generally has been preoccupied with the multiorganizational level of analysis and not with the individual level of analysis. Yet, individuals remain the basic units of analysis.

Failure to recognize that Ostrom, Tiebout, and Warren is primarily concerned with the organization of government in metropolitan areas as a polycentric order leads Golembiewski and his colleague Keith Baker into serious confusion. The criteria specified in that article for considering problems of scale in public organization were used to compare two different models of public organization: gargantua and a polycentric system. We argued that the conflict inherent in scale criteria can be resolved in gargantua if "field" and "area" organizations were created to "recognize the variety of smaller sets of publics that may exist within its boundaries" (1961:837). These conflicts can also be resolved in a polycentric system by separating the production of a public good or service from its provision in the sense of arranging for its joint consumption. Diverse criteria can then be taken into account in the multiorganizational structure of a polycentric order. Baker's puzzle can be resolved if he shifts his focus from a single organization to a polycentric order. He recognizes that the level of analysis is not directed at individuals per se. But he focuses exclusively upon the single-organization level of analysis that he identifies as "a public enterprise" (Golembiewski, 1977:1499; my emphasis added). He does not recognize the existence of a multiorganizational level of analysis.

Methodological individualism is only the beginning point; other units and levels of analysis are built upon that foundation.

Self-Interest. In taking the perspective of methodological individualism, I assume that any individual has preferences. These preferences will systematically affect the decisions the individual makes. Preferences are assumed to vary among individuals. In the absence of an ability to read one another's minds, I further assume that each person has limited information about others' preferences except as they provide information or engage in transactions that reveal their preferences.

1 19

The assumption of self-interest combined with the other restrictive assumptions related to the nature of private goods and the structure of a competitive market can be used to derive tightly reasoned inferences about how individuals in the aggregate will behave. This assumption, in combination with a less restrictive assumption about the nature of other goods and other institutional arrangements, does not permit one to derive as rigorous inferences as is possible with the perfect market model. Even less rigorous applications, however, save one from the error of assuming that humans can be perfect automata in the sense of being perfectly obedient servants in a bureaucracy. In using the assumption of self-interest, I would never suggest, for example, that human organizations or institutions can be subject to "fine-tuning" as Golembiewski does (1977:1489, 1500).

The problem of using the assumption of self-interest to think through inferences about how individuals will behave in light of different conditions can be illustrated by Hobbes' analysis. Hobbes uses a narrow conception of selfinterest: individuals will seek their own physical preservation. He demonstrates, however, that unconstrained or unlimited pursuit of selfinterest in a world of scarce resources will lead to a state where each individual is at war with every other individual. Because of the interaction that occurs among individuals, individuals find that instead of realizing their own preservation, as they would prefer, they are each threatened with their own extinction. Hobbes conjectures that individuals who find themselves confronting such a puzzle will then resort to reason and think through the conditions-the moral precepts-that will enable them to realize a state of peace rather than war. Peace is valued not as an altruistic good, but as a condition that enables individuals to better preserve their own lives and pursue their own good while others do so, too. Hobbes bases his analysis upon implications that follow from recognizing the essential capabilities and desires of other individuals. He assumes that individuals will be prepared to order their preferences to realize the benefits of peace so long as others do so, too. This argument lays the foundations for Hobbes' theory of the state.

Tocqueville recognizes much the same point when he refers to "self-interest, rightly understood." In the revolutionary era that marked the decline of aristocracy and monarchy, Tocqueville observed that the poor man "adopted the doctrine of self-interest as the rule of his actions without understanding the science that puts it to use; and his selfishness is no less blind than was formerly his devotion to

others" (1945:I, 11). Blind or unlimited pursuit of self-interest will lead to tragic consequences. Self-interest rightly understood depends upon the enlightenment of a right understanding. Learning occurs, and self-interest becomes enlightened. I assume that this right understanding for Tocqueville is consistent with the moral precepts contained in Hobbes' laws of nature. Enlightened self-interest can still be a useful assumption so long as the relevant choice situation is made explicit—i.e., the rule structures and the nature of the goods are specified.

In economic reasoning, the law and order assumption serves as a proxy for Hobbes' more elaborate argument and Tocqueville's right understanding. Hobbes himself makes a similar assumption for assessing relationships within a commonwealth when he uses law as the appropriate measure of justice and propriety. Selfinterest constrained by law creates a presumption that each individual will take account of the interests of others to the extent that is consistent with the moral requisites of a legal system and can be enforced as positive law.9 If crime and the criminal justice system are the subject of theoretical analysis, it would be foolhardy to treat the law and order assumption as anything more than a contingency to be taken into account in cost calculations.

Relying upon a law and order assumption in a theory of constitutional choice presents some obvious limitations. The task is that of creating a lawful order. The essential problem in the design of a self-governing democratic system of government, as David Hume, for example, saw it, is how to devise rule structures so that it is in the interest "even of bad men, to act for the

⁹In a one-paragraph discussion of the law and order assumption in the Intellectual Crisis, I conclude the paragraph in the following way: "In the absence of any law and order assumptions it might be necessary to assume a Hobbesian state of war as the prevailing human condition" (1974:52). The words in italics are used in quotations by Golembiewski in the following statement: "When pushed on the issue of the 'real-world inapplicability' of one of his assumptions, or about a conflict between assumptions, illustratively, Ostrom only allows that 'it might be necessary' to make different assumptions as to the 'prevailing human condition'" (1977:1491, cf. 1494). I had no sense of being pushed by anyone when I wrote the above statement; I have never used the term "real-world inapplicability"; and Golembiewski's remark, in addition to its all-too-typical misleading use of quotations, seems meaningless on its face. Although I do not myself know for certain what the human condition would be in the complete absence of civil order, Hobbes' argument, that it would be a war of each man against every man, is sufficiently compelling that it cannot be dismissed lightly. For my own part, I consider the argument fundamentally sound.

public good" (Aiken, 1948:296). Not any structure will do. There must be checks and potential controls so that officials can monitor one another and citizens can lawfully resist the usurpation of authority by officials. A theory of constitutional choice for a self-governing democratic society needs to be grounded in a right understanding that can be used by selfinterested individuals both to design political institutions and to assess their performance. This is the way that values get built into societies as human artifacts; and human beings can be said to govern themselves by institutions of their own choosing. These problems are treated in The Political Theory of a Compound Republic where I explicitly state how normative considerations enter into the design of political institutions.

Theory is never spun out of an assumption of self-interest alone. The self-interest assumption is useful only when we analyze how hypothetical individuals might confront choices in specifiable situations defined by reference to rule structures and the potential payoffs inherent in particular types of goods and services.

Ordering of Preferences. In using the perspective of methodological individualism, it is necessary to take a stand on whether one assumes that individuals are essentially rational or irrational in their behavior. I make a simple one-sentence assertion in *Intellectual Crisis*: "Rationality is usually defined as the ability to rank all known alternatives available to the individual in a consistent manner" (V. Ostrom, 1974:51). One cannot speak of individuals as being able to make up their own minds without assuming some capacity to order preferences in a consistent way: mindless individuals are not rational.

I did not make the argument regarding transitivity though many economists would take that position. This assumption is especially important for those economists who assume that preferences can be translated into utiles and all utiles can then be calculated on a master dial called utility.

I have reservations about this formulation. I believe that W. R. Ashby in *Design for a Brain* formulated a better solution where "essential variables" or "values" (Ashby, 1960:41-42) can be ordered by a configuration of readings on multiple dials that have reference to areas of acceptability and limits rather than to cardinal numbers. ¹⁰ However, I would still assume that

individuals can order their preferences in a consistent way. The precise nature of this consistency I do not know, though it seems all but certain that it is not a simple transitivity principle.

Golembiewski goes on to confuse the problem of preference orderings as they apply to the choices that *individuals* make with the Arrow problem regarding *social choice*. Arrow demonstrates that it is not possible to make a simple summation of individual preferences and derive a collective choice¹¹ that will meet what Arrow poses as a set of simple conditions for a rational collective choice. Arrow's impossibility theorem is not addressed to the issue of preference orderings at the individual level, but to the problem of social or collective choice.

In considering the problem of preference orderings, I make explicit reference to the problem of information and the problem of learning. When all possibilities are not known, the unknown possibilities cannot be consistently ordered in an unequivocal way. Uncertainty exists. Once uncertainty is postulated, I believe that it is necessary to introduce an assumption about learning. This led me to conclude: "Where learning occurs, the assumption of rationality may also have to be modified to allow for a reordering of preferences as the individual learns more about the opportunity costs inherent in different alternatives" (1974:51). Since learning occurs in a context that involves time, we can still assume that individuals are able to order preferences within limited time horizons.

Values, Efficiency, and Pareto Optimality. I assume, following Hobbes, that all human choices involve two sets of calculations. The first set is grounded in positive knowledge and pertains to the calculation of the probable consequences that are associated with alternative courses of action. The other set of calculations involves a weighing of the alternative possibilities and a selection of a course of action from the larger set of possibilities. I presume that the weighing and selection process involves evaluation or a consideration of values.

In doing positive theory it is necessary to handle the value problem by stipulation. The usual maximization postulate is simply a stipu-

¹⁰ Ashby's formulation can also be applied to the maximization problem. It is not necessary to maxi-

mize multiple values; multiple values can also be dealt with in relation to areas of acceptability and limits.

¹¹ The phraseology here is chosen to reflect Golembiewski's explanation of methodological individualism (1977:1491) rather than the language that Arrow uses in Social Choice and Individual Values.

lation to the effect that an actor will choose those alternatives that will yield the greatest net benefit. This can be stated equivalently in a language that speaks of least-cost in foregone opportunities; or it can be stated loosely as Madison states it when he speaks of choosing the greater good or the lesser evil. Positive theory always includes such a meta-normative element.

A different type of intellectual venture is involved when the question is posed as to what criteria should be used to evaluate or guide decisions—i.e., to weigh and select from alternative possibilities. ¹² This type of venture can be extended to conceptualize different variations for dealing with a particular type of criterion. Evaluative criteria can be used both to inform the process of design in the creation of artifacts and to assess performance. We might, thus, use evaluative criteria to assess the performance of two different systems of organization quite apart from having used evaluative criteria in the design of organizational arrangements.

In Intellectual Crisis and much of my other work I have used efficiency as the relevant criterion for measuring or evaluating changes in human welfare. 13 For the last century, arguments over reorganization and reform have focused largely upon efficiency and economy. The basic definition of efficiency is specified as "the accomplishment of a specifiable objective at least cost; or, a higher level of performance at a given cost. . ." (1974:48). Many variants of this criterion can be stated. Benefit-cost analysis often relies upon the minimal criterion that benefits exceed costs. Having reference to a cost calculus, however, has nothing necessarily to do with cadres of experts. Ordinary people can make cost calculations.

In dealing with economies of scale in a technical sense, an efficient solution is one that derives the lowest average cost for producing a given type of good or service: the appropriate scale being the production level that yields the lowest average cost. In Ostrom, Tiebout, and Warren, we were concerned that economies of scale on the production size be treated as one of the relevant criteria for considering scale problems for the organization of governments

in metropolitan areas. This is not inconsistent with the basic definition stated above.

Much of the literature in the traditional theory of public administration treats efficiency as being synonomous with perfection in hierarchical organization. Woodrow Wilson assumed that perfection in hierarchical organization would also maximize efficiency as the least-cost solution to accomplish policy objectives. Basic ambiguities exist in Max Weber's analysis because of this double meaning in the use of the term "efficiency." I would argue that these two usages are not consistent with one another. Tullock's *Politics of Bureaucracy* (1965) provides the logic for such an argument.

A variant in formulating the efficiency criterion is Pareto optimality. A Pareto optimum exists when no change could occur without making someone worse off. The Pareto criterion is an "ideal" measure. Human experience will never attain that ideal. If the Pareto optimum existed no transaction would occur. A Pareto-efficient move is justified in the limiting case where someone could be made better off but no one would be made worse off. The condition for a Pareto-efficient move can be viewed as the equivalent of relying upon a decision rule of unanimity where individuals would be free to veto any action that left them worse off.

Golembiewski and those he quotes seem not to have understood the argument advanced by Buchanan and Tullock about the Pareto criterion. Buchanan and Tullock make the basic association between a Pareto-efficient move and the rule of unanimity. This is why they use the unanimity rule as the foundation for their analysis. With the introduction of their cost calculus and certain other assumptions incuding the assumption that individuals will have an equal probability of finding themselves among either winners or losers in future collective decisions, Buchanan and Tullock were able to show that it would be a Pareto-efficient move for individuals at the constitutional stage to opt for a set of decision rules that does not meet the condition of Pareto efficiency in taking collective actions. This rather brilliant piece of normative analysis apparently escaped Golembiewski's attention. Buchanan and Tullock would expect cost calculators to opt for a set of decision rules that would minimize interdependency costs rather than insist upon a rule of unanimity. Buchanan and Tullock do not expect collective decisions to be Pareto-efficient.

We are still left with the basic problem in political theory that the instruments of coercion necessary for realizing mutually productive relationships can also be used to dominate the

¹² John Rawls' Theory of Justice represents an intellectual venture of this type as does Plato's Republic.

¹³This means that I have given less explicit attention to justice and error-correction as important evaluative criteria. I assume, without having provided the demonstration, that the evaluative criteria of efficiency and justice are consistent with error-correction as an evaluative criterion.

allocation of values in a society and oppress those who are subject to such instruments of coercion. As soon as the rule of unanimity is relaxed, the opportunity arises for some to exploit others. If most significant issues of public policy do not admit to Pareto-efficient moves, we cannot be confident that public policy decisions necessarily represent improvements in human welfare. Too frequently policy decisions rather clearly appear to contribute to the erosion of human welfare. Since I expect many decisions to be grossly Pareto-inefficient, I have no grounds for accepting the status quo as optimal.

Unfortunately, in reading Golembiewski's essay I have not been able to unravel what he means by value or values. He says approvingly that "De Gregori insists on seeing the full array of values that provide specific content for generic terms like, "tastes', 'preferences', 'freedom', and so on. . ." (1977:1493). "Freedom" is not of the same logical class as "tastes" and "preferences." To provide "specific content" for terms like "tastes" and "preferences" would presumably require reference to all potential goods and services. I cannot discern whether Golembiewski's repeated reference to "values" has the same meaning as "goods."

The problem can be indicated if we refer to Lasswell and Kaplan's definition of a value as a "desired event" (Lasswell and Kaplan, 1950:16). This definition couples desire or preference with the event that is the object of that preference. Public choice theory considers the nature of goods and services—i.e., events for which people have preferences—to be a primary element in analysis. The *Intellectual Crisis* distinguishes three types: private goods, public goods, and common-property resources. 14 Criteria for evaluation and choice can be considered independently of the good, objective, or goal that is being evaluated or selected.

I am simply left puzzled by Golembiewski's allusion to "efficiency for what." Consistent application of the criterion of efficiency rightly understood will enhance human welfare, assuming that individuals are presumed to be the best judges of their own interests. But this is a simple tautology.

Golembiewski tells us that he is rankled by the word "inherent" when I indicated that "the appropriate scale [of organization] will vary 'with the boundary condition of different fields of effects inherent in the provision of different public goods and services'" (1977:1501). Goods come in different sizes and forms: therefore, they have different fields of effects. I simply mean, for example, that the use, organization, and management of a ground water basin in a metropolitan area will involve different, but not entirely independent, domain and boundary conditions from the organization and management of a flood control program. Movements of ground water involve tangibly different fields of effects from surface flood flows. One is clearly a common-property resource; the other is a potential threat. The reduction of the potential threat can be viewed as public good for the community of people that may be affected. I am at a loss to understand his unhappiness with the word "inherent" in this case.

I am also puzzlied by Golembiewski's assertion that "[public choice] theory, Baker notes, prescribes 'unfettered forces of a laissez-faire market for the purpose of determining the substance, scope and direction of public policy" (1977:1506). Both Baker and Golembiewski must realize that a public choice theorist would expect market failure in a public-good situation. It would make no sense to prescribe a "laissez-faire" market solution. Neither I nor, to my knowledge, any other writer whom he criticizes, does so. Indeed, some public choice theorists would deny the possibility of a "laissez-faire" market-i.e., a market that existed without a supporting structure of public institutional arrangements. It takes a sophisticated political system to maintain an effective, competitive market economy.

Perhaps the major conceptual innovation developed in my work with Tiebout and Warren was to recognize that the task of organizing for the collective consumption of a public good or service can be treated independently of the task of organizing for the production of a public good or service. Once a collectivity is organized to tend to the problems associated with collective consumption that collectivity can arrange for production processes to be performed in different ways including contracting with private vendors or other collectivities. 15 This buying and selling of public goods and services permits a market-like or quasi-market condition to exist, but that condition is radically different from anything that might be called a "laissezfaire" market. I personally have been careful to avoid using the term "market" or "market model" and have consistently used the terms "quasi-market" or "market-like" organization,

¹⁴A more general typology is provided in V. Ostrom and E. Ostrom, 1977.

¹⁵This argument is more fully developed in V. Ostrom and E. Ostrom, 1977.

The existence of such possibilities clearly implies that nonbureaucratic coordinating mechanisms can exist in the public sector.

Golembiewski can unquestionably find some neoclassical economists who would view the market process as the ultimate arbiter of values. The contractarian formulations from Hobbes onward treated the problem of an ultimate arbiter of values in an explicitly political or social context long before Adam Smith's formulation of market theory. Smith, a professor of moral philosophy, was as much concerned with justice as efficiency and never viewed the market process as having priority in determining social values (Billet, 1976). The implicit contractarian position with which I would associate myself is that the basic criteria for human action depend upon general agreement or consensus among the people that comprise self-governing communities. These criteria pertain to enlightenment (error correction), justice (equity or fairness), and welfare (efficiency), among others. They form the basis for stipulating the basic constitutional terms and conditions of governance so that all authority is subject to limits. People acting collectively retain the basic prerogatives of constitutional decision making in relation to the diverse collectivities in which they participate. People acting individually retain basic constitutional prerogatives to govern their own affairs. All individuals participate in multiple political communities and share in testing and evaluating the conceptions and criteria of constitutional choice that are being acted upon in the conduct of different political experiments under changing conditions. A process of inquiry, contention, debate, and deliberation shapes decisions that are subject to review and reconsideration so that no essential interests are ignored. Tyranny in the small is no more justified than in the large. The interests of the poor and the disadvantaged are as essential as the rich and powerful. The ultimate arbiter of values, for me, is the process by which conflicts are articulated, processed, and resolved in mutually enlightening and mutually productive ways rather than repressed. General agreement and consensus about the constitutional order are maintained. This process requires access to multiple decision structures reflecting diverse communities of interest where all authority is subject to limits and dominance by any single center of authority is foreclosed.

The Use of Political Theory

In considering different approaches to theory, Golembiewski characterizes public choice

theory as resting upon a methodology that "emphasizes a closed-system circularity, while it permits incautious building upon assumptions that are often suspect" (1977:1491). He pleads for assumptions that are more realistic, alleging that the "motivation to test reality characterizes neither Ostrom nor the public choice literature, in the main" (1977:1492). Rather, he contends, "Ostrom's methodological approach encourages the ... treatment of theory as the-end-of-the-road rather than as hypotheses-to-be-tested" (1977:1492). He later asserts, "Ostrom accepts the broad-band notion of self-interest and, however motivated, that acceptance creates far greater theoretical problems than it solves" (1977:1496).

Indeed, there is a good bit of closed-system circularity in public choice theory-doubtless too much in places. But a degree of "closedsystem circularity" is one of the basic characteristics associated with the rigorous use of analytic methods. In my own work I prefer to use assumptions of uncertainty, fallibility with capability for learning, and what Golembiewski calls "broad-band" self-interest in the belief that such assumptions are more realistic than assumptions of certainty, perfect information, or blind self-interest. I do so precisely because I believe that such assumptions are more useful in generating researchable hypotheses. The cost entailed is a significant loss in logical rigor. I see: little purpose in solving logical puzzles unless the intellectual effort can be used to enable human beings to cope more successfully with practical problems of organization.

The essential problem in public choice theory is to derive the implications that follow when (1) self-interested individuals choose maximizing strategies within (2) stipulated organizational arrangements when applied to (3) particular structures of events that can be viewed as yielding payoffs and having the characteristics of particular types of goods and services. Based upon such relatively simple elements, it is possible to extend a structure of inferential reasoning with the introduction of new concepts or terms to derive a wide variety of implications and conclusions that apply to different units and levels of analysis.

These chains of reasoning can be used to generate researchable hypotheses. Researchable hypotheses can also be derived from other theoretical traditions. When contradictory conclusions are reached from different theoretical traditions, it is possible to formulate the structure of inferential reasoning into competing hypotheses. Research can then be undertaken to secure evidence to test competing hypotheses (Chamberlin, 1965; McDavid, 1976).

If repeated tests of hypotheses yield results that support one mode of reasoning as against another mode of reasoning, we can have some measure of confidence in treating the one theoretical formulation as the more useful analytical tool for reasoning through solutions to problems of public policy and political organization.

The method of competing hypotheses, if he had used it, would have placed upon Golembiewski the burden of demonstrating that an alternative theory offers more "realistic" assumptions, relies upon less "broad-band" definitions, derives tighter or more rigorous inferences and conclusions, and yields hypotheses that are better supported by empirical evidence. This he does not do. Instead, he objects that I have not defined "small," for example, in a precise way (1977:1501). He wants research results that yield "unequivocal interpretations" (1977:1503). He insists upon a "complete measure of benefits" (1977:1503). Etcetera. Etcetera. Etcetera. No research in the social sciences can meet these demands including the research in Golembiewski's favorite version of organizational theory.

Anyone who has done empirical research on human communities that manifest varying patterns of organization will realize that a single effort to test an hypothesis can involve a substantial expenditure of time and effort. To extend such an effort to a series of tests under varying circumstances involves a substantial magnitude of work. Research done in limited time horizons will necessarily be selective in treating some hypotheses and neglecting others. But the suggestion that there is a virtual absence of empirical research associated with the theoretical work that I have done must appear erroneous to any serious student of the literature.

Since Ostrom, Tiebout, and Warren, the problems of scale in public organizations have stimulated considerable interest among students and colleagues. The argument is seriously misrepresented when it is stated that smaller is better. Rather the argument is that the problem of scale is related to the nature of goods. We expect different forms of public goods and common-property resources to manifest different fields of effects. Varying scales of organization will be advantageous. Mancur Olson states this conclusion in the following way:

Only if there are several levels of government

tion to the smallest local government (quoted in V. Ostrom, 1974:70).

In Ostrom, Tiebout, and Warren we explicitly argued that the publics implicated by different types of potential public goods might vary in size from neighborhood to global proportions. Contamination of the atmosphere with nuclear waste, for example, is a global problem and cannot be controlled by action only at the national level. We explicitly argued that a large-scale metropolitan unit of government—gargantua—is an appropriate scale for many public services.

The provision of harbor and airport facilities, mass transit, sanitary facilities, and imported water supplies may be most appropriately organized by gargantua. By definition, gargantua should be best able to deal with metropolitan-wide problems at the metropolitan level (1961:837).

But we anticipated that other types of services will impinge upon much smaller communities of interest. Here we would expect disadvantages to accrue when *only* large-scale units of government are available to provide services where community preferences and environmental conditions vary in relation to such services. We expect an advantage to accrue if varying sizes of governmental units can operate concurrently with one another and with substantial autonomy from one another.

These conclusions can be juxtaposed to an argument advanced in the metropolitan reform literature which contends that greater efficiency and economy will be realized by the merger and consolidation of smaller units of government in a metropolitan area into a single dominant unit of government for the area as a whole. These two arguments can be used to derive competing hypotheses.

My colleague, Elinor Ostrom, has explicitly used contrary arguments to derive competing hypotheses in which size of jurisdiction and number of jurisdictions within a metropolitan area are used as independent variables and the level of output is used as a dependent variable (E. Ostrom, 1972). In 1970, she and colleagues in the Workshop in Political Theory and Policy Analysis began a series of studies in Indianapolis, Chicago, and St. Louis that specifically examined the effects of jurisdiction size on the supply of police services to individual neighborhoods in metropolitan areas. Several

(e.g., the President's Commission on Law Enforcement and Administration of Justice and the National Advisory Commission on Criminal Justice Standards and Goals). On the other hand, public choice theory would lead one to expect substantial diseconomies of scale in responses to individual calls for service and the production of general area patrol. Hypotheses derived from two theoretical traditions predict opposite consequences of using large-scale police agencies to produce neighborhood-level police services.

In each study, one data source has been a sample survey of citizens in which citizen experiences regarding criminal victimization, calls for service, speed of response, degree of follow-up, being stopped by police, knowing someone mistreated by police, and citizen evaluations of police services were used as multiple indicators of performance. In the St. Louis study other indicators were used including the proportion of warrants issued to warrants applied for, ratings given to police departments by a sample of police officers, and activity indicators. Similar-systems research designs have been used so the socioeconomic variables and other ecological variables could be treated as parameters by selecting neighborhoods matched on these variables. The size of the police department serving similar neighborhoods was consciously varied. By using multiple indicators of performance and a similar-systems research design, comparisons can be made without a complete benefit-cost calculus that relies upon a single common denominator.16 Conclusions can be reached about grossly inappropriate scales of organization even though we cannot expect to specify a precise optimum.

16The development of indicators and measures of performance is the answer to Golembiewski's charge: "...there can be no 'cost calculus' for determining the appropriate scale of any organization until an answer to a key question is reasonably in hand. That neglected key question is: Efficiency for what purpose?" (1977:1498)

The problem is to develop appropriate indicators or measures of performance where there is not a single, homogeneous, packageable, and quantifiable product. The problem is difficult, but not so difficult that we are forced to throw up our hands and proclaim to the world that there can be no such indicators or measures of output or performance. When comprehensive data are not available for simple calculations of efficiency, it is sometimes necessary to rely upon weaker criteria for evaluating performance such as effectiveness or responsiveness (E. Ostrom, 1971, 1975a, 1975b). The task of developing operational measures and indicators is a major one in empirical research that goes much beyond the development of logical constructs in theory.

In the Indianapolis study, three small (13,500 to 16,500 population), independent jurisdictions served by their own police forces (varying from 18 to 25 officers) were compared with matched adjoining neighborhoods within the Indianapolis Police District (population 485,750) served by the Indianapolis Police Department with 1,100 full-time officers (E. Ostrom et al., 1973). The Chicago study involved a comparison of two small poor black communities in south suburban Cook County with three similar neighborhoods within the city of Chicago (E. Ostrom and Whitaker, 1974). A more comprehensive study was undertaken in St. Louis involving 44 neighborhoods served by 29 different police departments (McDavid, 1974; E. Ostrom, 1976; Parks, 1976; and Smith and E. Ostrom, 1974). When grouped into classes of small (1 to 10 officers), medium (11 to 76 officers), and large (2 departments of 440 and 2,200 officers), small departments performed better on some indicators and medium-sized departments performed better on other indicators. In no instance did the large departments have better performance measures than either the small- or medium-sized departments.17

Similar studies were replicated by Samir IsHak (1972) in Grand Rapids, Michigan, and by Bruce D. Rogers and C. McCurdy Lipsey (1974) in Nashville-Davidson County, Tennessee. The consistent finding in all of these studies is that the largest departments have the poorest performance on most indicators.

A reanalysis of NORC data from a national survey undertaken for the President's Commission on Law Enforcement and Administration of Justice indicates similar results (E. Ostrom and Parks, 1973). Had the commission analyzed its own data on the effects of police agency size, it would have found evidence to reject one of its own principal conclusions.

These findings are consistent with the hypotheses derived from the theoretical tradition within which I have been working. When

17 Again, Golembiewski concedes an essential point in dealing with size as a variable: "For once any organization gets larger than one in which cozy face-to-face interaction is both possible and convenient, major differences in communication and influence patterns quickly develop" (1977:1501). Communication and influence patterns are what politics and collective decision making are all about. The larger the group, the greater the loss of information and control, and the less influence exercised by any one person. This becomes especially important where users of services function as essential coproducers, as in education, welfare services, police services, etc. (V. Ostrom, 1977; Whitaker, 1976).

large-scale organizations are used to produce services that are highly sensitive to localized community conditions, they are unlikely to perform as well as small- to medium-sized agencies serving similar areas. If all local police services were consolidated, we would expect to see the quality of some of these services deteriorate, to see costs rise, or to see both poorer service and increased costs.

Pachon and Lovrich (1977) using data from the Survey Research Center, report similar findings for the Detroit and Cleveland areas. However, they argue that, when controls for socioeconomic variables are introduced into their analysis, the basic relationships are reduced or reversed. A careful reading of their footnotes reveals that they rely upon data derived from such small samples and use such questionable statistical techniques that substantial doubt exists about the validity of their statistical analysis.

We do not expect to find that smaller is better as a general rule. Rather, we expect advantage to accrue from diverse scales where critical attention needs to be given to the type of service involved. Response to calls for service and community police patrol, for example, involve quite different scale problems than metropolitan highway patrol, radio communication, detention facilities, crime laboratories, etc. We would expect units serving larger areas to derive an advantage in supplying such services. But, a public-service industry composed of a large number of units operating at several different levels might be expected to supply such services as efficiently or more efficiently than a public-service industry composed of a single dominant agency serving a comparable area.18 The scale problem can easily become a monopoly problem (Bish and Warren, 1972; Savas, 1974). These speculations become more hypotheses for research at the interorganizational or industry level.

18Golembiewski contends that "some evidence suggests that a substantial take-off size is required for many diversified educational programs. . ." (1977: 1503-04). He seems to proceed on the assumption that diversity in service mix can be attained only through the comprehensive school. Special services can be supplied through specialized agencies serving any given area. Several agencies acting jointly as an alternative to one agency rendering comprehensive services. Which type yields the better service as measured by performance indicators is an empirical question worth investigating. My colleague Herbert Kiesling informs me that New York State relies upon special Boards of Cooperative Educational Services (BOCES) that supply supplementary services on a contractual basis so that small districts can procure diversified educational programs for their students.

Elinor Ostrom, Roger B. Parks, and Gordon P. Whitaker (1974), and other colleagues in the Workshop in Political Theory and Policy Analysis have developed methodologies for rigorously defining and quantitatively measuring structural variations in the interorganizational structures of public-service industries, including a description of organizational structures for police industries in 80 metropolitan areas (1977a, 1977b). Studies concerned with measures of performance for police agencies operating in differently structured local police industries are currently being undertaken.

A variant upon the industry-structure problem is to ascertain whether contracting for services can derive advantages over the production strategy of relying exclusively upon a municipal department to render a local service. Contracting or traditional departmental service can be conceived of as institutional variables. Roger Ahlbrandt's Municipal Fire Protection Services (1973) tests a hypothesis that compares contract service with that supplied by traditional fire departments. Werner Pommerehne and Bruno Frey (1977) have done a comparable study of residential refuse collection in 103 Swiss cities. The evidence in both cases supports the conclusions that private production under contractual arrangements is more efficient.¹⁹ But, as Pommerehne and Frey emphasize, this result can be expected to hold only as long as public policy maintains competitive pressures and constrains tendencies toward collusion among private suppliers. E. S. Savas reports similar findings in a major study of solid waste disposal in American cities (Savas, 1976).

This is only a small fraction of the empirical research that has relevance for the competing hypotheses which can be derived from public choice theory and from the more traditional theory of public administration. The implications of a public choice approach for studying the provision of public services has led a number of other scholars to pursue research on a wide variety of organizational conditions and settings (Baden and Stroup, 1972; Bish, 1971; Bish et al., 1975; Boschken, 1977; Hardin and Baden, 1977; Hawkins, 1976; Loveman, 1976; Martin and Wagner, 1978; Niskanen and Levy,

19Comparison across different types of institutional arrangements for supplying equivalent services under comparable circumstances was, for example, inherent in the idea of using the TVA as a "yardstick" for measuring the performance of private electric utility companies. Private producers can also be used as a "yardstick" for measuring the performance of public producers.

1974; Oakerson, 1973; O'Brien, 1975; Rich, 1977; Sabetti, 1977; Sproule-Jones, 1973, 1975; Sproule-Jones and Hart, 1973; Thomson, 1977; Warren, 1964, 1966; and Weschler, 1968.) Indeed, since 1961, proposals for two-tier solutions have been advanced among the traditional advocates of metropolitan reform. But the usual rhetoric about "overlapping local units causing a confusing maze" indicates that the two-tier solution is a politically expedient one without being appropriately grounded in theory (C.E.D., 1970:10).

Golembiewski's New Centrism

In his discourse on centralization, decentralization, and chaotic localism²⁰ it is interesting to note that Golembiewski relies exclusively upon a language that turns upon the concept of a unitary centralization rather than on federalism or polycentricity. He simply ignores the concepts of federalism and polycentricity which I consider to be the central thrust of my own work. In doing so, he fails to see my interest in variety (1977:1500). Instead, he adopts the conceptual language of Woodrow Wilson and ignores the conceptual language of The Federalist. I prefer to associate myself with the work of Daniel Elazar (1971), Charles Lindblom (1965), Martin Landau (1973), and Aaron Wildavsky (1976) who treat federalism as fundamentally different from centralization and decentralization. There are mechanisms of partisan mutual adjustment, cooperation, and conflict resolution for noncentral coordination of relationships in federal systems that cannot properly be characterized as centralization and decentralization (V. Ostrom, 1976c). As Wildavsky puts it: "Federalism requires mutuality, not hierarchy, multiple rather than single causation, a sharing instead of a monopoly of power" (1976:95).

Golembiewski seems to associate himself with the "camp [that] maintains that effective centralization must precede effective decentralization" (1977:1489). If I read him correctly, he suggests that only after first attaining effective centralization is it possible to determine "the scale of component units" and define "a rationalized system of differentiated/integrated subsystems" (1977:1489). A rational social

20I am amused that Golembiewski does not even perceive the possibility that his typology might include something called "chaotic centralization." The problem has been commented upon many times where central decision makers are the source of continuing surprises and disruptions in society. Uganda might be identified as an extreme contemporary case.

order apparently depends upon the exercise of monopoly power by an omniscient observer or body of omniscient observers who can assign people to their proper places and functions. The natives must be kept in their place, or chaotic localism will reign supreme.

Golembiewski approvingly quotes Fesler to the effect that "national legislation, overriding local objections and implemented by national administrative action" is necessary to (1) "democratize the selection of local officials," (2) "establish viable units of local government with the size, resources, and diversity of interests that are preconditions of effective local self-government," (3) "recruit and train skilled staff for local administration," (4) "minimize corruption and regularize fiscal practices," and (5) "provide grants from national revenue to help finance the impoverished communities" (1977:1501; my emphasis). The role of the states has apparently been eliminated in this new centrism. Democracy means that those who control national legislation and administration know what is good for the people. All legitimate interests can and must be defined in a national context.

National legislation is clearly appropriate for dealing with problems of racial and sexual discrimination and for dealing with a wide range of problems other than the organization of local government. Public choice theory provides no justification for encouraging "racial, sexual, and other forms of discrimination" as Baker presumably alleges and Golembiewski reiterates (1977:1506).

In Golembiewski's new centrism, national authorities have full competence to make constitutional decisions about the general structure of local government. National authorities are to control the allocation of power in society. He has forsaken the logically necessary conditions for the maintenance of a system of government where the conduct of officials can be limited by a system of enforceable constitutional law. He has taken us back to Hobbes' solution where those who exercise sovereign prerogatives at the center of government are the source of the law, are above the law, and cannot be held accountable to law. Sovereign authorities reign supreme and the prosperity of the people, to paraphrase Hobbes, depends upon their obedience and concord not upon their form of government.

The revolutionary intellectual development of Copernican proportions that occurred in America between 1776 and 1789 was the formulation of a theory of constitutional choice where it was possible for people to create, through processes of constitutional decision making, a system of government in which

all officials and all persons exercise an authority that is subject to the effective limits of an enforceable system of constitutional law (V. Ostrom, 1976a, 1976b). This theory of constitutional choice can be reiterated to allow for numerous units of government and several levels of government. All are constrained by positive rules of constitutional law. All people share in multiple communities of interest with access to concurrent governments in a compound republic where no one government exercises a monopoly over the legitimate use of force in society. Citizens maintain open public realms where their freedom of speech, assembly, and voluntary actions cannot be impaired by those who would destroy local government to save it from the vulgar influences of people.

I see no evidence in Golembiewski's "critique" that he has any better understanding of public choice theory than Woodrow Wilson had of the political theory expounded in *The Federalist*. Until Golembiewski learns the rudiments of economic reasoning used by Hobbes, Hume, Hamilton, Madison, and Tocqueville among others, he will understand neither public choice theory nor the theory of constitutional choice that was used to design the American political system. Instead he will use his lack of understanding to project false images as though these theories were nothing more than Rorschach ink blots.

Unfortunately, this is a problem that applies to all of us. The generality of the problem is indicated in a recent paper by Larry D. Spence where he writes:

The political writings in the history of Western civilization have become a series of Rorschach ink blots on which contemporary political theorists can project their aspirations and their values (Spence, 1977:12).

I simply do not know how we can solve this dilemma of potential misunderstanding other than to assume that we are all fallible creatures who can only hope to correct false images through an effective dialogue with those with whom we disagree. When we comprehend one another's arguments in a way that is consistent with the author's meaning, we then have the possibility of formulating competing hypotheses rather than competing misstatements. Unless we can test competing hypotheses, we can never sort out the wheat from the chaff. Until we have grounds for discarding some ideas, all ideas will have equal merit. Political science will be an evolutionary accumulation of everything. I find that to be a dismal prospect. Instead, the task of doing political theory should be one of

arraying arguments so that disciplined choices can be made from among contending arguments rightly understood and competing hypotheses properly tested. The criterion of error-correction should guide those choices. That is what it means to speak of a "discipline" of political science.

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Observations on "Doing Political Theory": A Rejoinder

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Although rejoinders to responses normally do not interest me, Ostrom's comments broaden the issues for analysis, while they highlight aspects of what my critique had to say about *The Intellectual Crisis*.

Two Preliminary Points

Let me draw early attention to two issuesone narrow, the other central and global, A definitional difference may help explain why Ostrom takes the absence of the word "federalism" in the critique as evidence of my desire to eliminate it, or worse, an argument recapped in the final section below. Ostrom urges distinguishing federalism and decentralization (e.g., p. 1521), so as to distance his argument from the critique's discussion of an over-reliance on decentralization. I see a federal pattern as a combination of both centralized and decentralized modes, with rules for variously blending them.² The critique argues, in effect, that different problems may require greater or lesser blends of decentralized with centralized modes of governance and administration. Federalism provides a useful institutional framework for such blendings.

Now for a central problem. Many of Ostrom's usages set up straw-men, irritate more than inform, attribute extreme positions to me by remarking that I "apparently" or "seem to" take them, and use easy victory over such an extreme position as a kind of proof of its polar opposite. Ostrom raises all four concerns when he observes: "Golembiewski seems to assume that the human animal can directly perceive and know 'reality' as such (p. 1510). The nuances of that charge play variously on my mind, because I never even knew anyone who would take such a position, and now I "seem" to be one. However, the truism that we "know" only through our concepts, theories, perceptions, and so on, does not constitute a license

to develop empirical theory based on whatever assumptions happen to strike one's fancy.³

Relatedly. Ostrom's intellectual agendas often elude me. Consider this example (p. 1513): "In using the assumption of self-interest, I would never suggest, for example, that human organization or institutions can be subject to 'fine-tuning' as Golembiewski does." Does that mean that organizations and institutions, in Ostrom's usage, are only "individuals in the aggregate" and hence-by definition-only individual self-interest is involved? If so, that contradicts much that we know about organizations, as well as Ostrom's observations (e.g., pp. 1511-12 and 1518 that structural variations can generate important differences in outcomes. Or does Ostrom intend only that self-interest saves "one from the error of assuming that humans can be perfect automata in the sense of being perfectly obedient servants in a bureaucracy" (p. 1513)? If so, many other constructs do that easy job-consider only the venerable notion of "informal organization" and its implied view of humans as essentially social beings-and without the problems attendant upon self-interest. Moreover, I would have expected Ostrom's self-interested individual to fine-tune organizations and institutions, perpetually and insistently. And if Ostrom does not believe that organizations and institutions "can be subject" to stimuli for adjustments to the needs and conditions of those affected, that undercuts the main thrust of The Intellectual Crisis-that is, "to advance and serve the interests of ... individual persons."4

Three Central Perspectives

Beyond these preliminary concerns, Ostrom's response suggests that several points at issue can be sharpened from three perspectives.

¹All page references in the text of this rejoinder refer either to my critique or to Ostrom's response.

²This usage follows Peter F. Drucker, Concept of the Corporation (New York: Day, 1946 and 1972).

³Robert T. Golembiewski, Keith Billingsley, and Samuel Yeager, "Measuring Change and Persistence in Human Affairs," *Journal of Applied Behavioral Science*, 12 (June 1976), 133–57.

⁴Vincent Ostrom, The Intellectual Crisis in American Public Administration (University, Ala.: University of Alabama Press), p. 131.

Perspective 1. Some Points of Agreement Differently Viewed. How basic agreements get treated provides useful perspective on any discussion of differences. Let me develop four points briefly to illustrate Ostrom's style of argumentation.

First, Ostrom correctly notes (p. 1508) that the critique places emphasis on "public choice theory," while he urges that he derives at least as much intellectual guidance from Hobbes, Hume, and others (p. 1510). Ostrom expresses concern that the critique thus fails to do justice, lbut it merely follows his lead. That is, Ostrom acknowledges his initial reliance on public choice theory as the model for reformulating his "conceptual language to a point where I could effectively apply economic reasoning to problems of political organization" (p. 1509). And he points to his discovery of similar "language" and "a mode of reasoning" in various writers about politics (p. 1509). A fair reading of this intellectual indebtedness certainly places public choice theory strategically in Ostrom's thought, I conclude. Hence the critique in effect retraces Ostrom's path of intellectual development in analyzing the ideational basis for his reformulation, especially to draw attention to problems with its "mode of reasoning" or methodology, from which Ostrom can hardly allege immunity.

Second, both Ostrom and my critique (p. 1508) agree on the grave limitations of traditional organization theory, in different but complementary ways. Ostrom emphasizes incompatibilities between the theory and important parts of our political heritage; and my earlier intra-organizational emphasis established that incompatibilities exist between that theory and several tenets of a "Judeo-Christian ethic," often resulting in costly effects on productivity and quality of working life.⁵

Ostrom does not provide an effective alternative to the traditional theory of organization, however, a point that can be developed in terms of how and why the bureuacratic model gained and maintained acceptance. In sum, spiraling economic development fueled the reputation of the traditional theory for getting results; empirical research but slowly established the common negative effects of the bureaucratic model on output and satisfaction; few if any cogent structural alternatives existed, especially those generated in the public sector, until the last 20 or 30 years; and even when alternatives became relatively clear, no technology existed until

recently for facilitating changeover from the traditional structure. Revealingly, analogs of bureaucracy have been with us at least since biblical days; and persuasive learning processes even today acquaint middle-class children at age eight or so with its rudiments which they apply consistently in choice situations.⁶

The tradition critical of bureaucracy needed two key elements to give it real force: the development of a methodology for empirical inquiry, and a heightened sense of the value and ethical consequences of different modes of organizing.

The critique argues that The Intellectual Crisis neither provides nor supports these key elements. To review, Ostrom neglects extant empirical research relevant to alternative structural forms. Moreover, the methodology underlying his work is variously inconsistent with or unsupportive of empirical research. Notably, the method underlying Ostrom's work has the same character as that underlying the bureaucratic model-derivations from assumptions about nature and from preferred conditions, which assumptions and preferences can be seriously challenged when made explicit. Finally, problems concerning both methodology and values make it improbable that Ostrom's work will encourage, let alone generate, an "applied science" necessary to provide efficient guidance for bringing alternative structural arrangements to life.

I acknowledge the several publications cited by Ostrom (pp. 1518-20), some of which were available when the critique was submitted for review in 1975. Review of subsequent efforts does not encourage any change in my judgment as to the likelihood of *The Intellectual Crisis* inducing a tradition of cumulative empirical research. The section below on Ostrom's way of "doing theory" presents detail on the point.

Third—although the critique nowhere so notes, nor does its argument require the note—I happen to believe that our "federal system" provides a serviceable framework for governance. To paraphrase Churchill, it is the most faulty basic form, except for all the others devised by humankind. Ostrom comes to a very different conclusion about my beliefs for rea-

⁵Robert T. Golembiewski, Men, Management, and Morality (New York: McGraw Hill, 1965).

⁶Herbert Wilcox, "The Culture Trait of Hierarchy in Middle Class Children," Public Administration Review, 28 (May 1968), 222–35.

⁷See critique, fn. 51, for a brief list of a half-century of major criticisms of the traditional theory. Most attention was fixated on supporting what became the "imperial presidency," although there were periodic bursts of attention to "grass roots administration" and experiments such as TVA.

sons I do not find convincing, as the last section of this rejoinder details. Here let me make a limited point only to reinforce what should be apparent: the critique can reject Ostrom's assumptions, methodology, and aspects of this argumentation while it need not reject federalism. Ostrom sees an identity between them, I suppose, even though linkages between political institutions and Ostrom's assumptions/reasoning often strike me as quite loose, even casual. In any case, arguments favoring the federal system can rest on bases very different from those Ostrom advances. Briefly, to illustrate, one can argue that the basic mode of expressing the human spirit-good or bad or indifferent-is through association, in "factions" or "groups" or whatever, whose bases can be kaleidoscopic. Hence a multi-tiered and polycentric system with easy egress/access, and with some extensiveness and heterogeneity-is prescribed as a practical way of institutionalizing geographical and functional barriers to the spread of mischief via association, while also permitting (even encouraging) individual and especially sub-national experiments which will help distinguish humbug from the locally appropriate as well as from the systemically useful or necessary. The contrasts with Ostrom should be patent: factions as well as individuals, eclectic and self-interested decision-making, collective attitudes or values as counter-weights to individual preferences, multiple social testing as constraints on individualistic choice, and so on.

Fourth, Ostrom's response unequivocably (e.g., fn. 1) urges the importance of values, and the critique could not agree more. Any prescriptive organization theory must be rooted in specific values. And so must "efficiency," "freedon," and a whole panoply of similar notions.

But problems remain. Consider (1) that Ostrom's response cites the need for a valueloaded political science; (2) that he uses the term in The Intellectual Crisis; and (3) that "value" is found often in his response. I follow Ostrom's usages; and I believe the critique is clear enough about values. Yet he pleads inability to "unravel" (p. 1516) what I mean by "values," and in that way dismisses a number of significant points. More troublesome still, the public choice literature is shown by the critique to have serious problems associated with values, and Ostrom at once ties his work to that theory while his response also seeks to distance itself from public choice theory in that fundamental regard. Yet again in his response, Ostrom notes (p. 1514): "In doing positive theory it is necessary to handle the value problem by stipulation." This is precisely the general point raised by the critique-which discusses the inadequacy of the implicit "stipulation" underlying public choice theory—and does not square with the crucial role of values Ostrom acknowledges in his response.

Finally, my concern on the value issue is heightened by Ostrom's way of dealing with value-loaded issues, as in his expressed puzzlement about this question: Efficiency for what? Ostrom proposes that "Consistent application of the criterion of efficiency rightly understood will enhance human welfare, assuming that individuals are presumed to be the best judges of their own interests" (my emphasis, p. 1516). Then Ostrom adds: "But this is a simple tautology" (p. 1516). Precisely, Ostrom himself correctly identifies the character of his argument. My comments about values are directed to the profound issues involved in what "efficiency rightly understood" means in collective settings.

Perspective 2: Some Basic Disagreements About "Doing Theory." Ostrom's intent to provide a comprehensive paradigm—and his way of "doing theory"—also play prominent roles in his response. They shed light on some issues in serious contention that go beyond the scope of the critique.

Three points sketch my position as to Ostrom's paradigmatic goal. First, no firm basis exists for comprehensive paradigms in the social sciences. The 1960s temporarily forecast a brave new world in this particular, but even Kuhn—who inspired so many, including Ostrom—observes that: "It remains an open question what parts of social science have yet required such paradigms at all." Paradigms are something to be worked toward, not posited and worked from, at our present level of development.

Second, Ostrom fails to distinguish between types of theory and their features. This increases my doubt that Ostrom's approach could resolve our paradigmatic problems, even if that were a feasible way to go today. Briefly, if Ostrom's work dealt with in the critique were consistently envisioned and presented as normative theory—as an expression of preferences and values-no major problems would exist. However, Ostrom presents his work in broader terms (p. 1510), as what I call a goal-based, empirical theory, that is, as a statement of how to approach some desired end-states via knowledge of empirical regularities. Such theory must be anchored both normatively in an explicit set of values, as well as empirically in a network of mid-level theories that relate tested

⁸Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 15.

concepts and operations suitable for approximating the critical dimensions of reality.

Ostrom's approach as goal-based, empirical theory has serious difficulties at both anchoring points. Thus he does not deal specifically with his basic goals or values, except where he uses generic "preferences" as surrogates for specific values. This is awkward, especially because the number of goal-based, empirical theories is limited only by the alternative sets of goals or values that can be envisioned. Moroever, Ostrom leap-frogs to general theory, with hardly a mention of the available networks of mid-level theory relevant to organizations that touch on leadership, atmosphere or climate, specific structural properties, and so on through a very long list. Basically, Ostrom fills lacunae of knowledge with a "mode of reasoning" that emphasizes assumptions or stipulations. Both of these are required in any theoretical enterprise, as Ostrom notes (p. 1511). Just as clearly, too much can be made of even the most useful things. The critique argues that Ostrom does precisely this, in both empirical and normative senses.

This confusion of types of theory implies the indeterminancy of tests of the general hypotheses Ostrom associates with his theory. To illustrate: Ostrom's theory is not grounded in available empirical research, so detailed theoretical linkages remain unspecified, and this implies major trouble for the commendable empirical work (pp. 1518-20) listed by Ostrom-e.g., as to whether size of public-service agencies is related to user perceptions. The available literature indicates that "size," for example, is by itself not a very useful indicator. Thus "size" is neither conceptually nor operationally simple; and it may not be a critical variable, however conceived and measured.9 Moreover, Ostrom's failure to specify numerous intervening variables or processes will very likely generate inconsistent and contradictory results in any hypothesis-testing, however extensive. Ostrom's global orientation and method cut him off from precisely such specificity on which goal-based, empirical theories must rest. Indeed, Ostrom's reaction (p. 1518) to the critique's comments about "size" implies that he is unaware of the available literature, its mixed results, and the complex linkages that must be specified before reasonably coherent predictions can be made.

In sum, Ostrom's lack of specificity under-

cuts a basic defense of his approach (e.g., pp. 1517-18) that he is developing theory which will generate hypotheses, whose test will imply the theory's adequacy. No such optimism seems appropriate. Paramountly, the issue is not the absence of empirical research inspired by Ostrom-which he says (p. 1517) is a "suggestion" in the critique. Rather, the straightforward emphasis is on the method and the theory encouraging the research, as well as on the degrees to which the theory is grounded in available empirical theory and to which the research specifies and tests intervening variables. Relatedly, Ostrom in concept ties "the problem of scale" to the "nature of goods," as Table 1 in the critique indicates. And Ostrom is clearly correct in noting (p. 1517) that his argument should not be taken to imply "that smaller is better," except where the "nature of goods" is identical or similar. The critique (pp. 1518-21) deals only with the latter case-although it is not as specific on that point as it should be-but it makes that simplification only because it relates to the literature cited by Ostrom which deals with comparisons of "similar" organizations providing "similar" goods. And here's the critical rub: in the absence of specificity about the "nature of goods" and organization characteristics, such comparisons of effects are treacherous. Police departments of similar scale, illustratively, are definitely not replicas in terms of their concepts of service, stewardship, and so on.¹⁰ Moreover, what we know about organizations with even "identical missions" is that they can and do differ profoundly in terms of output and quality of working life. 11

Third, Ostrom challenges (pp. 1517–18) that part of my obligation requires developing an alternative approach. I can comply, although not here: A forthcoming two-volume set attempts precisely that task, focusing on the theme Public Administration as A Developing Discipline. And Ostrom's The Intellectual

⁹See John R. Kimberly, "Organizational Size and the Structuralist Perspective: A Review, Critique, and Proposal," Administrative Science Quarterly, 21 (December, 1976), 571-97.

¹⁰ James Q. Wilson, Varieties of Police Behavior (Cambridge, Mass.: Harvard University Press, 1968), esp. pp. 227-77, distinguishes three "styles" of "similar" departments, relating them to demographic, political, and historical differences.

¹¹The major impact of variations in managerial style on output and satisfaction—other things being more or less equal—illustrates the present point. See James D. Thompson, "Authority and Power in 'Identical' Organizations," American Journal of Sociology, 62 (1956), 29—301.

¹²Robert T. Golembiewski, Public Administration as A Developing Discipline (New York: Marcel Dekker, 1977): Vol. 1: Perspectives on Past and Present; and Vol. 2: Organization Development as One of A Future Family of Mini-paradigms.

Crisis played a role in motivating that effort, for which I thank him.

Patently, the full argument of these two volumes can only be touched on here, but the books provide historical and empirical support for the basic proposition that the present condition of the field can be remedied by following guidelines prescribing work that:

- has both "pure" and "applied" aspects
- which as pure science is ab initio rooted in available empirical research, deals with specific processes and dynamics, and is directed by a methodology that tests increasingly comprehensive linkages of variables embodied in growing networks of theory
- which as applied science rests on an explicit set of values that prescribe the character of the "good organization"
- which has multiple analytic levels
- which includes a technology for facilitating efficient adoption of preferred structural variants
- which constitutes a reasonable "next bite" in coping with the issues of organization and governance.

In contrast to a "comprehensive paradigm," then, the two volumes array a range of thought and research regarding one mini-paradigm devoted to building experience and consciousness concerning both empirical and normative aspects of organizational analysis.

Perspective 3: "Golembiewski's New Centrism." Ostrom's argumentation in his response is nowhere better illustrated for me than when he assigns me paternity of a "new centrism," or hegemony over it. His rationale rests on my alleged self-association with those who propose the "effective centralization must precede effective decentralization."

Even acknowledging the honor of having a centrism named after me, I must decline, respectfully but nonetheless truthfully. Ostrom's generosity has no basis—either in my own words printed above, in quotations of others which I use approvingly, or in my other work.

Ostrom opens strongly in attributing to me the "new centrism," on which I am said to rely "exclusively" and whose goal is nothing less than to "keep the natives... in their place" (p. 1521). Then he goes on to observe that I only "seem to associate" myself with those centrists who stir his concern, that I only "suggest" their

nefarious doctrine, and that we are all only "apparently" devoted to "monopoly power." And Ostrom also adds: "If I read him correctly..." (p. 1521).

Well, Ostrom does not read the critique correctly. For example, the paragraph at issue (critique, p. 1509). several times signals its interest only in "those supporting the traditional paradigm," "its proponents," and so on. Somehow, Ostrom not only enlists me in those ranks, but also puts me at the head of "Golembiewski's new centrism." This goes beyond guilt by association. You are what you describe, seems the operating rule.

Such business leaves me very cautious about Ostrom's posture toward analysis or argument, and four related points reinforce that concern. First, the critique strongly denies any preoccupation on my part with centrism (pp. 1508-09, and numerous other points through p. 1519, n.).

Second, the proposition that "effective centralization must precede effective decentralization" does not inspire my confidence. Effective centralization might have such an effect, or it might lead to heavy-handed autocracy. It all depends. If forced to bet, and given only one choice, I would give odds that effective centralization would result in a continued and probably heightened centralization. In my consulting experience, an ineffective centralization is most likely to inspire attempts to decentralize. Incidentally, the version of the critique originally submitted to the Review noted about that old chestnut of a proposition which Ostrom calls the "new centrism"—in a section that made the cutting-room floor, as they say—that "the proposition is not face-valid, of course."

Third, the bulk of my published work defies such attribution. In stark opposition, that research deals with the conceptual elaboration 13 and empirical testing 14 of specific alternatives to centralized modes of organizing, as

13At the intra-organization level, see my "Civil Service and Managing Work," American Political Science Review, 56 (December, 1962), 961-73. For a current effort to tailor organization policies to one common time-bounded set of individual needs, see Golembiewski, "Mid-Life Transition and Mid-Career Change," Public Administration Review (in press).

14For example, one recent line of research seeks to establish the empirical and value advantages of an apparently-simple policy change at work, implemented in accordance with a specific set of values. See Robert T. Golembiewski, Rick Hilles, and Munro Kagno, "A Longitudinal Study of Flexi-Time Effects," Journal of Applied Behavioral Science, 10 (December, 1974), 503–32; and Golembiewski and Hilles, "Flexi-Time in a Large Firm," Monthly Labor Review, 100 (December, 1976), 65–69.

well as with a technology ¹⁵ for developing attitudes, behaviors, and skills suitable for functioning in other-than-centralized modes.

established by now; and I would argue that central action often has been required—in economic development, in matters of racial justice,

Fourth, Ostrom does not use the exact text of the critique to make his point. On p. 1501, I quote Fesler's balanced words that "National legislation, overriding local objections and implemented by national administrative action, is often required to democratize the selection of local officials, to establish viable units of local government with the size, resources, and diversity of interests that are preconditions of effective local self-government," and so on through to "help finance the impoverished communities."

People of good will may differ, of course, about the precise degree and kind of penetration of the federal government into state and local affairs. But the principle seems pretty well

15E.g., Robert T. Golembiewski, Renewing Organizations (Itasca, Ill.: F. E. Peacock, 1972); and Robert T. Golembiewski and Alan Kiepper, "MARTA: Toward an Effective, Open Giant," Public Administration Review, 36 (January, 1976), 46-60.

established by now; and I would argue that central action often has been required—in economic development, in matters of racial justice, segregation, civil rights, organized crime, and so on through a long list. Ostrom might disagree, and we could debate the issues with zest. I would enjoy that. The argumentation took a very different turn, however. Thus Fesler's qualifier "is often required" becomes "is necessary," in Ostrom's telling, and this list of astounding conclusions is then made (p. 1521).

- 1. "The role of the states has apparently been eliminated."
- "All legitimate interests can and must be defined in a national context."
- "National authorities have full competence to make constitutional decisions about the general structure of local government."
- 4. "National authorities are to control the allocation of power in society."

Good grief.

Developing Public Policy Theory: Perspectives from Empirical Research*

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There has been considerable interest in the development of theories of public policy formation, but theoretical efforts to date have not demonstrated adequate recognition of the distinctive qualities of the dependent variable as a focus of research. Facets of public policy are far more difficult to study systematically than most other phenomena investigated empirically by political scientists. Our attempt to test hypotheses with some rigor demonstrated that public policy becomes troublesome as a research focus because of inherent complexity—specifically because of the temporal nature of the process, the multiplicity of participants and of policy provisions, and the contingent nature of theoretical effects. We use examples of policy making taken from the case study literature to show concretely how such complexity makes it essentially impossible to test apparently significant hypotheses as they are presented by Lowi, Dahl, Banfield, and others. Our effort here is to enhance theoretical development by carefully specifying and clarifying the major shortcomings and pointing out the apparent directions of remedy.

The emerging discipline of public policy studies is characterized by a growing disjunction between theory and research. While there are many provocative and potentially important theories, systematic empirical research to test them has largely been lacking. Most importantly, there seems to have been little progress in the critical intermediate stage of refining and then operationalizing the important variables.

In the course of our own research—an attempt to employ aggregated case-study data in order to test quantitatively a number of

*The authors gratefully acknowledge support from the Rackham School of Graduate Studies, The University of Michigan, and the assistance of Marc Holzer and an anonymous referee.

¹By "systematic empirical research" we mean the use of quantitative data gathered from a number of instances of policy making in order to test specific hypotheses. Examples of recent work in this direction are provided by Robert K. Yin and Douglas Yates, Street-Level Governments, R-1527-NSF (Santa Monica: Rand, 1974); William A. Lucas, The Case Survey Method, R-1515-RC (Santa Monica: Rand, 1974); and Robert K. Yin and Karen A. Heald, "Using the Case Survey Method to Analyze Policy Studies," Administrative Science Quarterly, 20 (September 1975), 371-81.

important public policy hypotheses²—we experienced enormous difficulty at this intermediate stage. We attempted to translate leading hypotheses into operational variables suitable for employment in quantitative analysis. The difficulty we experienced did not lie in the question of whether the hypotheses were supported or not, but in whether they were testable or not. Although the theories seemed perfectly applicable to the few cases used by their authors to illustrate them originally, the propositions did not fit so neatly when applied to a number of examples not expressly chosen for explanation and illustration. As we attempted to deal with problems of operationalization in connection with a variety of theories and dozens of cases, we noted that many of the problems recurred. While the theories were plausible, they seemed to fall into certain traps that appear to be endemic to the systematic study of public policy. This led us to adopt the purpose of the present paper-to comment on

²For a description of that project, see our "Case Study Aggregation and Policy Theory," *Proceedings*, 1973 Annual Meeting of the American Political Science Association, New Orleans, La., September 4–8, 1973.

these peculiarities of public policy theory and describe these pitfalls, as an avenue toward both building better theory and illuminating some of the significant characteristics of public policy itself.

The plausible and provocative theories of public policy formation offered by Lowi, Banfield, Froman, Gamson, Dahl, and others, appear in general to be no more or less adequate than those in any area of political science. But the kinds of phenomena that the theories seek to explain are radically different from most of the other phenomena we study. It is this difference in the objects of analysis that is the source of the difficulty.

In brief, public policy as a focus of systematic comparative analysis is *more complex* than such phenomena as electoral votes, legislative roll calls, incidents of political violence, and elite ideologies. It is more complex on at least four counts, which we will elaborate and illustrate in the discussion to follow. These are:

- 1. The policy process takes place over time, sometimes over a long period of time. This leads to difficulty in explaining "the process" as a simple unit. Even if one attempts to explain specific outcomes, the explanatory forces invoked almost invariably involve characteristics of this long and shifting process. Two sorts of difficulty arise:
- a.) As the process proceeds over time, it can involve a large number of decision points, e.g., the decision of a subcommittee chairman, a Senate roll call, a presidential compromise, and the decision of an appellate court. The contents of each of these outputs might be called "public policy" and might be predictable by public policy theory. But we do not want theories to be oriented toward or tested upon inconclusive or tentative decisions. Nor do we want them constructed so as to predict the characteristics of the rubber-stamping process. We want somehow to focus only on "significant" outputs.
- b.) The idea of a predictive theory of public policy demands that the values of the predictors be determined at some beginning point. Such values, however, are likely to change with the unfolding of the process itself, their final status being achieved only at its termination. Many presumably predictive theories are thereby weakened substantially, and become, in final analysis, post hoc explanations.
- 2. Any given policy proposal, or "output," or "outcome" is in itself complex; it may have

several important aspects. This multiplicity can make the whole policy extremely difficult to place in any single category, as is demanded, for example, by the categorization schemes that currently abound in public policy theory.

- 3. As a focus of analysis, policy making is complicated by the presence of a large number of participants. When a characteristic of the participants becomes a variable of interest, as it often does, variation among participants with regard to that characteristic causes difficulty. The difficulty takes two forms:
- a.) Subjective. The state of the world as perceived by participants yields many important policy-analytic variables. But perceptions vary considerably, of course, depending upon the participant consulted or described.
- b.) Objective. Still more variables are generated in existing theory by "objectively" determined participant characteristics—as determined, that is, by the researcher, interviewer, casewriter, or other outside observer. Ambiguity is introduced when the heterogeneous group of all participants, or heterogeneous subcollections of participants, must be assigned a single score on such a characteristic (e.g., level of involvement, or point of access to decision makers).
- 4. Lastly, public policy as a research focus is complex because the process cannot be described by simple additive models. On the contrary, the forces interact; the impact of one depends in large measure upon the value of another.

In sum, "public policy" is almost never a single, discrete, unitary phenomenon. Indeed, the appeal of public policy studies as a focus of intellectual endeavor lies precisely in its richness; the complexity of the unit of analysis simply and appropriately reflects the fact that an action of government is rarely meaningful if conceived of as a discrete, disembodied event, and that the impacts of a single government action on society are not understood properly if taken in isolation from one another.

In this essay, then, we seek to specify in some detail the kinds of difficulties these characteristics create, illustrating them by exploring the problems arising from attempts to apply specific theoretical propositions to concrete events taken from the case-study literature. We hope that our conclusions will not only provide some assistance to developers of

³Our use of the terms "output" and "outcome". follows Ranney's definition of those terms. See his

[&]quot;The Study of Policy Content," in *Political Science* and *Public Policy*, ed. Austin Ranney (Chicago: Markheim, 1968), pp. 8-9.

public policy theory who are concerned to avoid problems of conceptualization and operational definition, but will also highlight certain important characterisites of public policy itself that have heretofore been inadequately understood or explained.

An Initial Illustration

Many of the problems arising from the complexity of using "policy" as a unit of analysis can be illustrated by the work of Theodore Lowi. No single theoretical construct has been more important to the development of public policy studies than Lowi's categorization scheme,4 yet the way in which he defines his fundamental terms is seriously weakened by the problems inherent in the unit of analysis, which make his hypotheses almost impossible to operationalize meaningfully in order to test them empirically. Since these problems of operationalization bear upon several of the challenges inherent in developing public policy theory, and since Lowi's idea is so well known and so well received, we refer to his work by way of general introduction, to be recalled briefly at several points in the more schematic discussion to follow.

The heart of Lowi's argument is that "policies determine politics." He developed, in a series of articles, a typology containing "regulatory," "distributive," "redistributive," and (subsequently) "constituency" policies and argued that policy processes will differ significantly depending on the policy type involved. As a result of those differences in process, he suggests, relationships among important concepts or variables that may be quite strong when policies of one kind are involved may be much weaker or even totally absent when other types of policies are concerned. Instead of attempting to find relationships that hold across the entire range of public policy, he argues that one should focus one's investigation within one of the four policy classifications.

Lowi generates a number of important hypotheses from his typology. For example, he predicts that congressional committees will be able to retain control of the process of coalition building on distributive amendments and that,

⁴We refer, of course, to the ideas presented in the series of three articles: "American Business, Public Policy, Case-Studies and Political Theory," World Politics, 16 (July 1964), 677–715; "Decision Making vs. Policy Making: Toward an Antidote for Technocracy," Public Administration Review, 30 (May-June 1970), 314–25; "Four Systems of Policy, Politics, and Choice," Public Administration Review, 32 (July-August 1972), 298–310.

therefore, few amendments will be offered to committee bills on the floor. He also predicts that peak associations can be expected to be more cohesive when confronted with redistributive issues that unite their memberships than when they are faced with regulatory issues that divide them. 6

The explicitness of the predictions Lowi derives from his policy classification is not matched, however, by a similar explicitness in explaining how one can determine the correct policy type for a given policy. Since the correct determination of policy type is obviously central to the predictive ability of Lowi's theory, this limitation has crucial importance. Lowi contends that distributive decisions are made without reference to their implications for other decisions; that regulatory decisions imply a direct choice "as to who will be indulged and who will be deprived"; and that redistributive decisions involve the greatest interconnection, since they imply choices among broad classes of individuals. But the "decisions" of which Lowi is speaking are not simple and discrete; they are policy decisions. Many can be expected to contain at least some characteristics of each of Lowi's types, and there is little guidance for determining how a policy is to be classified in any but the simplest cases.

The problem of classifying a policy correctly thus becomes a central stumbling block to the empirical testing of Lowi's hypotheses. For instance, policies often have distributive programmatic characteristics but redistributive or regulatory financing mechanisms, as the case of the Chicago Transit Authority aptly illustrates. The CTA approached the state of Illinois for subsidization—a handout, precisely in the distributive tradition of the pork barrel and the pre-1960s tariff. The matter might easily have been processed and settled as a distributive issue, but the method for financing the subsidy became controversial. The proposal was even-

⁵This is why Lowi found the '60s tariff to be so different. In earlier times, tariff policy was distributive, but "The true nature of tariff in the 1960s emerges as regulatory policy.... Issues that could not be thrashed out through the 'group process' also could not be thrashed out in committee but had to pass on to Congress and the floor," Lowi, "American Business," p. 701. Lowi presented data on amendments to bills in Congress from the floor in "Decision Making vs. Policy Making," pp. 321–22.

6"If there is ever any cohesion within the peak associations, it occurs on redistributive issues, and their rhetoric suggests that they occupy themselves most of the time with these" (Lowi, "American Business," p. 707).

⁷Ibid., pp. 690-91.

tually defeated, largely because of the dispute on regulatory or redistributive concerns—the question of whose particular pockets the money was to come from. More generally, for any policy lacking explicit income transfers, there is some financing method either built in or implied that may or may not fall into the same category (in Lowi's typology) as the policy's other characteristics.

James Q. Wilson discusses this difficulty at some length. He maintains that a single policy may have aspects of each policy type, and he offers several examples. "A bill barring discrimination in public accommodations," for instance:

could be seen as a measure regulating the use of hotels and restaurants or as one redistributing a benefit (access to hotels and restaurants) from one social stratum to another.... Urban renewal programs regulate the use of land, redistribute the housing supply, and distribute benefits to certain contractors and labor unions. Monetary and fiscal policy has both regulatory and redistributionist implications depending on whether one thinks of it as simply controlling the interest rate or as benefiting creditors at the expense of debtors (or vice versa).

Lowi never specifically addresses the problem of how to classify a policy correctly when it has attributes of more than one of his policy types, but one can infer from the general tone of his articles that he does not expect classification to present a major problem. His basic argument in this respect appears to be that expectations based on past experience with similar issues objectively structure policy choices for the entire policy process. Thus he might well respond to Wilson by contending that one particular aspect of any of the policies Wilson cites can be expected to dominate the expectations of the actors in the process, so that there will be no problem in making the classification. Hotel and restaurant accommodations policy, for example, would undoubtedly evoke regulatory expectations from virtually all those connected with the policy process, based on their past experience and the past debate on similar issues.

That answer, however, is adequate only when expectations are virtually unanimous. Yet the policy process is as likely to be characterized by multiple perceptions as policies are

Variations in perception of a policy are especially likely to occur when participants in the process actively seek to redefine the issue. Lowi himself suggests that "one of the important strategies in any controversial issue is to attempt to define it in redistributive terms in order to broaden the base of opposition or support."11 In classifying a policy in which this strategy has been employed, Lowi's method will work only if the strategy has been almost a complete success (almost all perceptions have been redefined) or almost a complete failure (almost no perceptions have been redefined). If the strategy succeeds in altering some perceptions but not all, one has no guidance for deciding which perceptions will provide the basis for classification.

Take the controversy over Medicare. 12 Initially, since the proposal called for an additional tax on workers to provide benefits for the elderly, one would probably classify the policy as redistributive. The AMA, however, attempted to redefine the issue as one of government regulation of physicians and the practice of medicine. The AMA's argument undoubtedly influenced the perceptions of at least some individuals on the issue, but it certainly did not succeed in restructuring the perceptions of all. There is simply no objective way to determine which set of perceptions should be dominant in classifying a policy issue when there is substantial disagreement among the participants themselves about what is at stake.

likely to be characterized by multiple attributes. Whenever there are any significant disagreements among perceptions of a policy, the problem in classification according to Lowi's typology has simply shifted from one of determining "which aspect" to one of determining "whose perceptions." In the CTA example, it would be quite difficult to decide whose views should predominate-those concerned with mode of financing or those concerned with the costs and benefits of the transit service. Moreover, as Wilson notes, the problem becomes even more difficult in the cases of new or innovative policy, where there is little relevant past experience to structure the perceptions of any of the participants. 10

⁸Edward C. Banfield, "The Chicago Transit Authority," in *Political Influence* (New York: Free Press, 1961), pp. 91-125.

⁹ James Q. Wilson, Political Organizations (New York: Basic Books, 1973), p. 329.

¹⁰Ibid., p. 339.

¹¹Lowi, "American Business," p. 707, footnote 28.

¹²Theodore R. Marmor, "The Congress: Medicare Politics and Policy," in American Political Institutions and Public Policy, ed. Allan P. Sindler (Boston: Little, Brown, 1969), pp. 3-68.

The problems in operationalizing Lowi's hypotheses illustrate the special difficulties of theory construction and testing in the field of public policy studies. His provocative ideas cannot be meaningfully operationalized without considerable effort by the researcher to add greater specificity and precision, a process in which the researcher must often, without adequate guidance, make important assumptions about what the theory is really trying to say. As it stands, Lowi's theory is not testable because the basic concepts are not operationalizable; in order to operationalize them, researchers must make a number of guesses and assumptions that create a situation where they can no longer be sure just whose theory they are testing.

The discussion of Lowi's paradigm illustrates the obstacles one encounters in attempting to apply policy theory across many cases, each of which is a full-scale instance of the operation of a policy-making system. We will now consider somewhat more explicitly the forms of complexity enumerated earlier, after which we will conclude with a brief listing of implications.

Problems of Temporality

Multiple Outputs. Several important hypotheses become mired in ambiguity for want of a time-stopping criterion. A whole host of variables is difficult to operationalize without being arbitrary until some decision is reached as to when, over a protracted period of development and struggle, "policy is made." For example, many of the most important hypotheses attempt to predict, as a dependent variable, the success or effectiveness of employing various resources or strategies in influencing outputs. Both Gamson and Dahl have discussed this problem at length, and both suggest measuring success by comparing the output of the process with the intentions of the relevant actors. 13 The output, however, keeps changing! There are simple cases, to be sure, such as those studied in Gamson's research on fluoridation. but there are complex cases as well. In fluoridation, points of beginning and ending were generally clear; someone at some time initiated the controversy with a proposal to fluoridate the water and policy was "made" when a community voted. More frequently, though, policy making consists of an ongoing process in

and in which both outputs and intentions are continually modified.

Policy struggles are usually preceded by a background period during which intentions and opinions are being formed. Later, outputs are issued, modified, and remodified as administrators respond to continuing pressures or as losers initiate appeals to higher authorities. At times, reviews by higher authorities create real opportunities to change results; at other times the decision is merely rubber-stamped. At what points are researchers to slice into the ongoing policy process to measure the relevant intentions and outputs? How many and what sorts of appeals are to be considered before a decision process is regarded as complete? These questions are important because the measurement of such variables as the number of participants, the duration of conflict, the resources employed, etc., will be significantly altered depending on the time period over which they are measured.

An illustration can help clarify some of these difficulties. In the case study of "The Glavis-Ballinger Dispute,"14 the controversy progressed through several stages. At stake was the validity of the Cunningham claim to large tracts of public land in Alaska. At first there was an administrative determination that the claim was valid. Then Glavis, an official in the Bureau of Land Management, began a routine investigation into the validity of the claim, ultimately recommending that the land not be turned over to Cunningham. Secretary of the Interior Ballinger overruled Glavis. Glavis then appealed to President Taft, who supported Ballinger. But Forest Service Director Gifford Pinchot then helped Glavis publicize the issue and take his case to the Congress, and ultimately a law was passed which prohibited the sale of public lands in the future, but which still did not resolve the Cunningham dispute. Several years later a federal court found against Cunningham.

It may seem obvious that the court decision decided the case, but is this the most important point to examine? And what if the court had found for Cunningham? The determinative decision might then be considered to have been either that of Ballinger, or Taft, or Congress, or the court, with all of the subsequent decisions essentially ratifying the first. Similarly, one might argue that the case began at any of

zation of the sale of land; Cunningham's violation of the law in amassing his claim; Glavis's investigation; or the rejection of Glavis's recommendations. Depending upon the points chosen, a number of important variables, such as the degree of conflict in the dispute and the number of participants, would vary.

Treatment of this particular quandary not only affects decisions about the analysis of an individual case, such as the Glavis-Ballinger dispute, but might also artificially predetermine certain results of a quantitative analysis. Defining the duration of policy conflicts in such a way as to be as inclusive as possible, for instance, might well yield the finding that most important administrative decisions are determined in the courts, while in fact one suspects that there are many appeals in which there is no real hope of overruling an administrative determination-appeals that are taken largely pro forma in order to symbolically satisfy aggrieved interest groups. Another artificial result of inclusiveness would, no doubt, be the finding that congressional committees hardly ever have the final say about anything-although in fact they make an enormous amount of policysimply because their decisions are routinely passed upon by the full chamber, conference committees, and the president. On the other hand, defining the time parameters of the process too narrowly-by attempting, say, to single out each discrete decision-would force the systematic analyst to consider thousands of decisions, jamming any data set with useless, unimportant, and misleading information.

It is thus essential, both when policy hypotheses are proposed and when they are tested, to have in mind some reasonable criteria for demarcating the process temporally. The reader and writer must agree somehow on the outputs that are theoretically crucial, otherwise doubt and confusion must inevitably arise regarding the consistency of theory and data.

Postdictive Theory. Another complication of temporality arises from the annoying tendency of variables not to stand still as the process unfolds. The duration, scope, or complexity of a given policy struggle are rarely strictly determined at the outset; not only are outcomes often unpredictable, but so too is the process. Thus predictive hypotheses that fail to account for this contingency often become extremely difficult to apply to concrete cases and are often best tested—and thus rendered most meaningful—by being converted into postdictive hypotheses. But that change may seriously dilute their significance.

An example is provided by the concept of "requisite actions" suggested by Banfield in *Political Influence*. Banfield introduces the concept as a way of explaining why some policy proposals are adopted and others are not. He employs the following definitions:

Performance of a specified set of actions by specified actors, or by a specified number of, or proportion of, the actors who constitute a specified group, constitutes adoption, i.e., adoption is defined as the performance by these actors of these actions, which will be called requisite actions... An actor who can perform a requisite action has authority over the action. He may perform it or not as he likes, or, in the language to be used here, he may give or withhold it from the system of activity being concerted toward adoption of the proposal. 15

In any situation, certain actors will be controlled while others are autonomous—that is, free to withhold the performance of requisite actions. In highly centralized systems, there are few autonomous actors; in highly decentralized systems there are many. Adoption of proposals in a highly decentralized system is uncertain, according to Banfield, because it cannot be predicted whether or not autonomous actors will perform the requisite actions.

Banfield goes on to derive a number of interesting and nontrivial hypotheses from his argument. For example, he argues that, "as the number of autonomous actors in a situation increases, the probability of adoptions decreases,"16 and that "corruption will tend to increase as the distribution of authority widens."17 Banfield believes that these derived propositions can be tested: "If when testedand some of them cannot be tested by any data in this volume-these derived hypotheses prove false, doubt will be cast upon the factual premises from which they were deduced."18 But in order to test Banfield's model, it is necessary to identify and count the number of requisite actions that must be concerted in order for a policy to be adopted. And while Banfield asserts that it is uncertain that any given requisite action will be performed, it must be possible, in order for his theory to have predictive value, to at least specify what the requisite actions are before the policy struggle

The number of requisite actions may, however, change dramatically during the course of a

¹⁵Banfield, p. 309.

¹⁶Ibid., p. 318.

¹⁷Ibid., p. 322.

¹⁸Ibid., p. 308.

policy struggle, as controversy heats up; and it may become clear that some actions were in fact requisite actions only after the policy struggle is over. Take the case, Defending "The Hill" Against the Metal Houses, as an example. 19 A developer wished to erect a questionable type of housing in a lower-middle-class neighborhood. Given the legal requirements of the jurisdiction, certain zoning and building clearances would undoubtedly have to be obtained. Are there other requisite actions? How long and harsh will the decision be in the making? Can we predict the probability of adoption from our initial count of the requisite actions?

If we are to count the neighborhood residents as "an autonomous actor," the struggle could be protracted, but the residents of such neighborhoods typically do not organize to make their wishes known. Thus, predictively, the number of requisite actions would seem to be, say, two or three. As the particular case turned out, however, the residents of the neighborhood did organize, and as a resultthough not even then a clearly predictable result-a substantial number of aldermen, the mayor, the city's corporation counsel, the state board of health, and the opposition candidate for mayor all eventually became autonomous actors as well. The developer was defeated, but only after a long and emotional struggle.

The specific dynamics of the policy process, in this case at least, thus determined not only whether or not significant actors would give or withhold requisite actions, but how many requisite actions there would have to be. This is not to say that Banfield's theory is not correct as a post hoc generalization about the extent to which two interesting attributes of the policy process covary; but the implied causality in the original hypothesis becomes impossible to test empirically.

It is now clear that Lowi's central hypothesis, that characteristics of the process can be predicted from characteristics of an input (policy type) is fettered by this same difficulty. The policy type and perceptions of it by relevant actors often are determined well after the process has begun. Of course, one might predict new characteristics of the process continuously as a policy changes or is perceived to change from distributive to regulatory, regulatory back to distributive, and so forth, but this

complicates the job of comparative analysis to the point of impracticability.

The Problem of Multiplicity of Policy Aspects

A public policy is only rarely the result of a simple binary decision, or even a chain of such decisions. Even the simplest government policy is likely to spring from a complex chain of causes and relationships and to have a set of subtly interrelated consequences for the general social network. Unless there is adequate awareness of this complexity, and adequate precautions are taken to cope with it, even those theories of public policy which seem most sensible are likely to evaporate on close inspection into a cloud of ambiguity. We have already discussed at some length the problems of complexity arising from the temporal nature of policy making. The point of this and succeeding sections is that policy research would be complex even if the process were instantaneous.

Lewis Froman, for instance, hypothesizes that homogeneous communities will adopt "areal" policies, while heterogeneous communities will adopt "segmental" policies. He defines areal policies as those which affect the total population of a city simultaneously by a single action, and segmental policies as continuing policies which affect different people at different times in separate sections of the city.²⁰ In his segmental category, Froman thus seems to take cognizance of the fact that the same policy can have different impacts in different places over time. Yet that awareness is. of no help to a researcher trying to fit a policy such as, for example, "promoting industrial development," into Froman's categories. Industry may locate in a carefully zoned, narrowly circumscribed area, but (1) its pollution may affect surrounding neighborhoods; (2) it may provide jobs to residents of a much larger area; and (3) its property taxes may pay for services uniformly consumed throughout the whole jurisdiction. There is no a priori way for the researcher to determine which of its impacts are to be considered in deciding whether the policy is "areal" or "segmental."

The same problem is illustrated by Alan Altshuler's description of an intercity freeway dispute: should the freeway be built along a southerly route through a black neighborhood

¹⁹ William K. Muir, Jr., Defending "The Hill" Against the Metal Houses, ICP Case #26 (University, Ala.: University of Alabama Press, 1955).

²⁰Lewis A. Froman, Jr., "An Analysis of Public Policy in Cities," *Journal of Politics*, 29 (February 1967), 94-108.

or along a railroad line to the north?21 The route through the black neighborhood was "scientifically chosen" for the shortest and best traffic patterns. The benefits from that route certainly appeared to be areal. The highway would be used by virtually everyone, and even those who didn't use it would benefit from the generally improved transportation and commerce in the area. More importantly, state and federal assistance was available for that route, lowering the tax burden for everyone in choosing it. On the other hand, the costs of the decision were quite clearly segmental. Many families were displaced from the area immediately surrounding the construction, with little or no counseling or provision for replacement housing. The displaced residents moved primarily into nearby buildings, seriously increasing already severe congestion and suffering the noise and pollution consequences of their continued proximity to the freeway. From the point of view of the winners, then, the freeway location decision was areal; from the point of view of the losers, it was segmental. From the point of view of a researcher trying to assign the policy to one of Froman's types in order to test his hypotheses, the decision was not easily classifiable.

The difficulty we encountered above in treating Lowi's stimulating hypothesis in its illustrative application to the subsidy for the Chicago Transit Authority also fits into this category. The policy proposal took on at least two separate and salient aspects, service and financing, which made difficult its classification into one and only one category of Lowi's policy typology.

The Problem of Multiple Participants

Even if the policy-making process were both instantaneous and unitary, it would still be complex as a focus of analysis because of the multiplicity of participants involved and the towering importance of participant characteristics, both subjective and objective, as elements of policy theory.

Subjective. The perceptions of relevant actors are important determinative variables in many theories about public policy, but policy theories rarely specify whose perceptions are to be taken into account. An illustration of the necessity of specifying from whose point of view a concept is to be defined is provided by hypotheses in which the status quo is an

important concept. Gamson, for example, argues that it takes fewer resources to defend the status quo successfully than to bring about change.22 While the status quo seems to be an objective characteristic of a state of the policy process, it may often be defined only in terms of participants' perceptions, which may in fact differ substantially from one another. Take the following example: in the case study, Shooting Down the Nuclear Plane,23 all agreed that the specific purpose of an existing \$75 million appropriation was to carry out a small development program. From the Air Force's perspective, however, that appropriation was only a first step and constituted a commitment to the actual construction of a nuclear plane. To the Air Force, it was that commitment which defined the status quo. Failure to expand the program and build the plane would be a negation of the commitment and thus a serious departure from the status quo. From the perspective of the members of the congressional appropriations committees, on the other hand, the small development program represented only a limited venture; from their point of view, confining the future program to reactor development, for example, would represent no change whatever in the status quo. In a situation of this sort, how is one to evaluate Gamson's hypothesis? Each protagonist, the Air Force and the Congress, thought that it alone was defending the status quo and that the other was opposing it; there is no objective criterion on which the outside observer can base a decision as to whose perception was correct. It might well be considered sound to decree in this case that the most relevant perception of the status quo is that of the Congress. Its perception anchors the concept of "change" in Gamson's hypothesis; Congress must be changed. The generalization to be recognized, however, is that such a determination must be made on a priori theoretical grounds for each variable and each hypothesis subject to this kind of ambiguity in research.

Again, the example helps to pinpoint a problem noted in our earlier discussion of Lowi's typology. Lowi argues explicitly that the perceptions of actors determine the category into which a given policy must be classified, but he does not treat theoretically the question of whose perceptions must be dominant in the event that there are differences.

²¹Alan Altshuler, Locating the Intercity Freeway, ICP Case #88 (New York: Bobbs-Merrill, 1965).

²²Gamson, p. 63.

²³W. Henry Lambright, Shooting Down the Nuclear Plane, ICP Case #104 (New York: Bobbs-Merrill, 1967).

Objective. In many hypotheses, the independent variable seeks to describe a subjective characteristic of participants-that is, a state of the world as seen through a particular lens. When theorists fail to specify adequately whose subjectivity is to be measured, the problem described in the previous section arises. Other hypotheses, however, seek to link objective characteristics of participants-their resources, explicit attitudes, or demographic characteristics-to aspects of process or output. But those hypotheses are of little help in determining which participants the characteristics describe. It is useful to keep subjective and objective characteristics of participants distinct. Hypotheses regarding the former often are deceptively simple, because they refer explicitly to impersonal conditions (e.g., the status quo, the divisibility of the benefits), which in reality are subjective perceptions of conditions; whereas the latter tend to refer quite obviously to people (e.g., resources used, strategy used). Both, of course, are troublesome for the same basic reasons-the multiplicity and heterogeneity of participants.

In Democracy in the United States, Dahl suggests that, "how severe a conflict is depends on how much is at stake."24 Empirically, we might estimate how much is at stake ourselves as outside observers, or we might depend on the relevant actors' own views of what is to be lost or gained. Even if we elect the former, "objective" course, however, so that perceptions of actors are not relevant, we are still left with the unanswered question: What is at stake for whom? Variation is possible here, just as it is for factors such as social class or resources employed. The following concrete instance illustrates how answering that question may be critical to an empirical test of Dahl's hypothesis.

The issue in the case of *The Florida Milk Commission Changes Minimum Prices* was a proposal for the complete deregulation of milk prices, replacing a system of controls at all levels from the dairymen to the consumer.²⁵ Three different groups were primarily affected. Consumers could be expected to benefit in the short run from increased competition within the milk industry. The "big three" dairies would be hurt in the short run, since they would have to lower prices and curtail profits.

Most seriously affected of all, however, would be the independent distributors, who would not be able to compete with the "big three" and might eventually be forced out of business altogether, if past history in the market area were any guide. In the long run, the "big three" would then benefit from the reinstitution of an oligopolistic market in which they each had a larger market share. Conversely, consumers could expect the long-run outcome to be neutral at best, and possibly negative.

Dahl's hypothesis could thus be applied to this case in at least three different ways. If one looked primarily at the consumers, one would predict very little conflict; for them, the stakes were quite small. For the "big three." the stakes were large but not overwhelming. For the independents, the issue was virtually one of life or death, since they could not compete in either buying power or production efficiency if prices were deregulated. The choice of which group to focus on is thus crucial. If one takes the party with the most at stake, one would expect the conflict in this case to be quite severe; if the party with the least at stake were used, very little conflict would be expected. To take an average would similarly be to predict a relatively low or at most a moderate level of conflict. More to the point-unless one attaches a way of surmounting the problem to the original formulation of the hypothesis-the theory cannot be tested. It remains, in an important sense, incomplete.

The Problem of Interaction

Interaction among independent variables in determining an outcome is of course common in social theory and in research findings. By "interaction" we mean that the existence or strength of an effect is contingent upon some other condition or the value of some additional variable; for example, the effect of financial resources upon success in a policy struggle may depend upon the arena of decision-legislative, executive, or judicial. The phenomenon is quite obviously not peculiar to public policy. We suggest, however, as the complexity emphasized repeatedly in the foregoing discussion would indicate, that interaction is endemic to public policy-it is perhaps its most salient characteristic-yet, it is rarely recognized in theoretical offerings.

Many important theories about public policy are probably incorrect as generalizations encompassing everything that falls within anyone's image of policy making, while essentially accurate for that subset of examples which

²⁴Robert A. Dahl, Democracy in the United States, 2nd ed. (Chicago: Rand-McNally, 1972), p. 303.

²⁵Harmon Zeigler, The Florida Milk Commission Changes Minimum Prices, ICP Case #77 (University, Ala.: University of Alabama Press, 1963).

theorists implicitly take as their own definition of the universe of applicable instances. The failure to demarcate that universe is potentially a failure to recognize important statistical interaction. We will offer a specific illustration in a moment, but we note in passing that the general definition of "policy" itself may be a contingent condition upon which the applicability of a theory is meant to depend. There exists in the literature a rather astounding number and variety of suggested boundaries (or lack thereof) about the concept, "policy": all government action;²⁶ a program of goals, values, and practices;²⁷ the impacts of government activity;28 general rules to subsume future behavioral instances;29 the consequences of action and inaction;30 important government decisions;31 and "a particular object or set of objects which are intended to be affected ... [together with] a desired course of events ... a selected line of action ... a declaration of intent ... and an implementation of intent..."32 It is perhaps too much to ask at this stage that we all agree on our usage of the

26Lowi criticizes Dror for defining policy as simply any output of any decision maker in his book Public Policy Making Reexamined (San Francisco: Chandler, 1968). Dror never formally defines policy but his discussion indicates Lowi is correct. See Lowi, "Decision Making vs. Policy Making," p. 317. Thomas R. Dye defines public policy as "Whatever governments choose to do or not to do," Understanding Public Policy (Englewood Cliffs, N.J.: Prentice-Hall, 1972), p. 1.

27Harold D. Lasswell and Abraham Kaplan define policy as "a projected program of goal values and practices," Power and Society (New Haven: Yale University Press, 1970), p. 71. See also Carl J. Friedrich, Man and His Government (New York: McGraw-Hill, 1963), p. 70.

²⁸For example, Easton writes: "Arriving at a decision is the formal phase of establishing a policy; it is not the whole policy in relation to a particular problem. A legislature can devise to punish monopolists; that is the intention. But an administrator can destroy or reformulate the decision by failing either to discover offenders or prosecute them vigorously. The failure is as much a part of the policy with regard to monopoly as the formal law. When we act to implement a decision therefore we enter the second or effective phase of a policy." The Political System, 2nd ed. (New York: Knopf, 1971), p. 130.

²⁹This is our own preferred definition of policy. See our "Case Study Aggregation and Policy Theory," p. 11.

30 Dye includes the consequences of inaction as well as action, whether intended or not, in his definition of policy. Dye, p. 2.

31 Lowi suggests the need to look at only important substantive government decisions, "Decision Making vs. Policy Making," p. 317.

32 Ranney, p. 7.

term "policy," but in the absence of such agreement, it is well to recognize that hypotheses might receive more or less support if tested on all government decisions and actions or on some one of the many alternative subsets referred to as "policy" in current theoretical writings.

To illustrate the problem of unspecified interaction more specifically, we will consider hypotheses about group size as a political resource. Pluralists in general postulate that group size is an important potential resource. Murray Edelman, on the other hand, argues that large groups are more likely to be bought off with symbolic reassurances than small, well-organized groups.33 E. E. Schattschneider argued still a third position: that group size may be of relatively minor significance because the relevant group may be able to involve wider publics or disinterested government officials in a dispute.³⁴ It is easy to think of examples to illustrate the persuasiveness of each of these conflicting hypotheses, and, indeed, each author provides several. None, however, attempts to place his propositions in the context of more general theory by specifying the conditions under which they are valid. One is thus left with three opposing theories, all of which may very well be valid for a broad class of decisions, but with no clue as to when they are valid and when they are not, and why.

By way of contrast, it might be useful to offer the work of one theorist who appears to have adequately stated the conditions under which his hypotheses can be expected to be supported. In his The Logic of Collective Action, Mancur Olson takes exception to the pluralist position concerning group size as a resource, contending that large groups may be ineffective in pursuing their interests in comparison with smaller groups.³⁵ Although his hypothesis is derived from more general economic theory, Olson also relies heavily on the kind of illustrative material used by Edelman and Schattschneider. Yet Olson carefully delimits the scope of his theory by specifying the circumstances under which the behavior he predicts is most likely to occur. Large groups, for example, will be able to organize effectively when they are seeking collective goods if their members benefit disproportionately, or if they

³³Murray Edelman, *The Symbolic Uses of Politics* (Urbana: University of Illinois Press, 1974).

³⁴E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, 1960), pp. 3-77, passim.

³⁵Mancur Olson, *The Logic of Collective Action* (Cambridge, Mass.: Harvard University Press, 1965).

are able to coerce their memberships or provide members with side payments of selective goods. Because Olson makes such contingent conditions explicit, his hypothesis is testable without requiring further refinement—without the researcher having to guess at the theorist's intentions. ³⁶

Implications

We have argued from our own research experience that the systematic study of public policy is seriously complicated by the nature of the beast. "Policy" is complex-because of temporality, because of multiplicity of aspects and participants, and because of interaction. Yet complexity need not prohibit systematic empirical research. So long as it is accounted for and confronted directly, so long as hypotheses are sufficiently precise, theoretical models may adequately represent reality. We suggest that most of the lessons to be learned have to do, not so much with the conduct of data analysis, as with the formulation of hypotheses and the elaboration of theory.37 Specifically, we suggest the following:

1. Temporality: Multiple Outputs. Optimally, we should all agree on how to identify key developmental points in the policy process, such as the beginning point and the point at which it might be said that policy was indeed "made." Such agreement is undoubtedly premature. In its absence, however, systematic comparative analysis still requires that key stages be identified. In this one instance, it is the data analyst who perhaps has more to contribute than the theorist. We can learn by experience which identification criteria are reliably operationalizable and which among them

36This does not mean that Olson's theory is necessarily correct. There may be additional contingent conditions which he failed to state that would further refine or modify his theory. John Chamberlin, for example, has recently suggested some additional contingent conditions under which large groups are likely to provide large amounts of a collective good in contradiction to Olson's predictions. All we want to imply is that the concepts in Olson's theory are reasonably well specified and the theoretical relationships among concepts are stated with reasonable precision, allowing others successfully to re-examine and test them. See John Chamberlin, "Provision of Collective Goods as a Function of Group Size," American Political Science Review, 68 (June 1974), 707-16.

³⁷For illustrations of how this process can both permit the elaboration of theory and provide operationalized hypotheses for empirical testing, see Jeffrey A. Miller, "Welfare Criteria and Policy Outcomes: An Empirical Assessment" (Ph.D. dissertation, University of Michigan, 1975).

seem to yield fair tests of hypotheses. In our own work, we have found it both practical and productive to identify a "point of last significant controversy" and, somewhat less importantly, a "point of first significant controversy." The terms are almost self-explanatory, although an elaboration of their meaning and use is available elsewhere. We commend these criteria to others for consideration and trial and urge the formulation and trial of alternatives as well. Most importantly, we emphasize that objective comparisons cannot be made unless a criterion is consistently applied to each case in order to determine which of its many decision points establishes "policy."

- 2. Temporality: Postdictive Theory. One must simply be sensitive to the problem and avoid creating predictive hypotheses that fall into the postdictive trap. Specifically, one must not attempt to predict characteristics of the policy process either (a) by variables (such as number of requisite actions or resources committed to the struggle) whose value or score for a given case is not known until too late in the process itself, or (b) by variables (such as "policy type") that are offered predictively by theorists as inputs but that in actuality are characteristics of outputs. For a policy hypothesis to be predictive, the causal variable must obviously be observable and measurable at a point in time before the effect and should not be subject to significant change beyond that point of evaluation.
- 3. Multiplicity of Policy Aspects. The general implication of this problem is that we must either avoid variables that may differ significantly in value depending upon the aspect of a given policy to which they are applied, or that we must provide a means of selecting the aspect that is most relevant for the operationalization of a given variable. Two guidelines on this issue stand out in our experience to date. One is that any typology of whole policies runs a substantial risk of ambiguity at the stage of operationalization; typologies can be extremely valuable, but they should optimally be offered along with well-considered ground rules for classification. The other guideline is that a major (but not exclusive) source of ambiguity lies in the possible divergence of the benefits from the costs or the substantive from the financial aspects of a public policy. In creating hypotheses, it may be profitable for the theorist to consider whether the variables are subject to

³⁸See our "Case Study Aggregation and Policy Theory," p. 14.

differential rating depending upon whether benefits or costs form the basis of evaluation.

- 4. Multiple Participants: Subjective. The question of whether it is most important to consider a given possible state of the world as it in some sense actually or objectively exists or as it is perceived by participants in the policy process is generally given inadequate attention. If there are good theoretical or methodological grounds for choosing the latter, it is essential to consider whether different participants might perceive the status of affairs differently. If so, it is necessary to translate those grounds into guidelines on whose perceptions are to govern the scoring of that particular dimension for research purposes.
- 5. Multiple Participants: Objective. This problem is potentially the most troublesome of all, since characteristics and behaviors of participants emerge so commonly in policy theory as important variables. The theoretical considerations that necessitate testing a hypothesis comparatively-by assigning a value to each policy struggle to represent some objective characteristic or behavior of "participants"should make possible some decision as to how those characteristics should be operationalized. Theory might provide criteria for selecting one participant or homogeneous group as the basis for assigning a value to the variable, or, perhaps, criteria for aggregating the value across one or more groups of heterogeneous participants. We have had substantial success in dividing the participants in each struggle into two opposing camps and, for most participant-oriented variables, giving an aggregate score on the variable to each camp. However, this attempt at a universal method is much less satisfactory for some variables than for others, and for some cases of policy making than for others. Sometimes it does not work at all. Other schemes might be devised, but such tinkering by the data analyst is in general less desirable than explicit criteria generated within the theoretical proposition to be tested; for example: "In predicting the severity of conflict by 'how much is at stake,' the true predictor is the average of the perceived stakes across all major participants."
- 6. Interaction. It is clear that not all hypotheses can be valid for all types of policy making in all kinds of circumstances. To the extent that a theory fails to specify major conditions defining its applicability, it is inadequate theory. If the conditions of validity are specified, not only is the analyst's job made

easier and a fair test of the proposition likely to ensue, but also direct benefits result for the development of the content of policy theory itself. Theory should be parsimonious, to be sure, but not oversimplified.

Conclusion

Because of the complexities of public policy as an object of study, we may never be able to obtain hard knowledge of the policy process of the type available in the advanced physical sciences. Yet improving our understanding of policy phenomena is clearly possible, if only through advancing the conceptual sophistication of theoretical formulations.³⁹ Our own experience has convinced us that such advances will be more rapid and certain as theory encounters systematic empirical data. As the brief comments in this paper illustrate, the collision between theory and data, while perhaps frustrating at first, can have important benefits for both researchers and theorists. Those lacunae in theory, painfully identified by difficulties in operationalization, become foci for efforts at additional theorizing that, however much they may do violence to the intentions of the original theorist, can add considerably to the richness and utility of hypotheses. The necessity of separating aspects from cost aspects in theories of policy type, or the notion of "last significant controversy" in a policy process, are small, but not trivial examples. If present theoretical levels in public policy are to progress, obstacles of the kind identified here should not be viewed as roadblocks. They cannot be wished away, nor can they be evaded. But by grappling with them directly, it is possible to add to the precision and sophistication of theory while proceeding with the essential conduct of supporting empirical research.

³⁹For an illuminating discussion of the role of "hard" research in developing social theory of various types, see the following two papers by Anatol Rapoport: "Various Meanings of 'Theory'," American Political Science Review, 52 (December 1958), 972–88, and "Explanatory Power and Explanatory Appeal" (Paper prepared for the Conference on Explanatory Theory in Political Science, Department of Government, University of Texas at Austin, Feb. 19–23, 1968). Rapoport argues that because of the limitations of social science, improved conceptualization is often a more important criterion in judging good theory than predictive power. In this regard, the value of the application of hard methods in social science would not necessarily lie so much in improved prediction as in better conceptualization and the reformulation of thinking about problems.

Power Consciousness: A Comparative Analysis*

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This article, drawing on recent studies of class consciousness and powerlessness explained in individual versus system level terms, develops and analyzes an operational measure of "power consciousness." Power consciousness is defined as a person's evaluation of his or her political power position and his or her explanation of the causes of any inadequacies or advantages perceived in this position. The operational measure of power consciousness arrays responses to two items on political power satisfaction (one of them open-ended) along a dimension which ranges from satisfaction, to dissatisfaction ascribed to personal failures of the respondent, to dissatisfaction explained in terms of problems with the political system. Survey data are utilized for a comparative analysis of whites' and blacks' scores on the power consciousness measure. The following topics receive detailed attention: the place of power consciousness in the matrix of power measures; its relationship to background factors, especially level of education; its relationship to indicators of political discontent: and the impact of power consciousness on levels and styles of political behavior. The analysis is followed by suggestions for the development of future work in this area.

Introduction

With the advent of survey research, American political scientists have devoted considerable effort to measuring and assessing political efficacy. This interest in subjective feelings of political power is grounded in the demonstrated importance of efficacy as a predictor of voter turnout and other conventional forms of political activity, and in the oft reported relationship between feelings of political efficacy and satisfaction with the political system. Political efficacy is even characterized by some as a regime norm which may operate as a critical force to contain frustrations in mass

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¹There is a substantial amount of research reported in the literature. See Kenneth Prewitt, "Political Efficacy," in *International Encyclopedia of the Social Sciences*, ed. David L. Sills (New York: Crowell, Collier, and Macmillan, 1968), Vol. 12, pp. 225–28, for a concise review and bibliography.

²David Easton and Jack Dennis, "The Child's Acquisition of Regime Norms: Political Efficacy,"

societies and restrain "disillusionment with [the] norm of democracy."2

Recent literature on political efficacy is marked by questions about the equivalence, meaning and structure of measures employed by scholars working in this area.³ Converse's discussion of the widely used Michigan (SRC/CPS) political efficacy measure holds that even it should be partitioned "into at least two components, which might be more precisely labeled 'personal feelings of political competence' and 'trust in system responsiveness'." Balch makes a similar point in his analysis of the SRC/CPS efficacy measure, discussing "two separate aspects of the [sense of political efficacy] concept" and linking the trust aspect with "diffuse system support." 5

This distinction between individuals' assessments of their ability to influence government

American Political Science Review, 61 (March 1967), 38.

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3</sup>See, for example, George I. Balch, "Multiple Indicators in Survey Research: The Concept Sense of Political Efficacy," Political Methodology, 1 (Spring 1974), 1-43; Philip E. Converse, "Change in the American Electorate," in The Human Meaning of Social Change, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 334-37; and Edward N. Muller, "Cross-National Dimensions of Political Competence," American Political Science Review, 64 (September 1970), 792-809.

⁴Converse, "Change in the American Electorate," p. 334.

⁵Balch, "Multiple Indicators in Survey Research," p. 31.

and their trust in the responsiveness of the political system is analogous to the distinction often drawn in the literature on attitude theory between the cognitive and evaluative (affective) components of an attitude. The former is a probability dimension and refers to beliefs about the probability of unprobability that a particular object or relationship exists. The latter is an evaluation of the object or relationship-whether it is good or bad, satisfactory or unsatisfactory, or the like.6

The cognitive/evaluative distinction is important in the social-psychological approach to studying alienation in which subjective powerlessness is a key element. Seeman, for example, defines powerlessness in purely cognitive terms as "the expectancy or probability held by the individual that his own behavior cannot determine the occurrence of the outcomes, or reinforcements, he seeks." The evaluative (affective) component is specifically excluded from the definition because, Seeman says, if powerlessness is operationally defined "in terms of a sensed discrepancy between what is, and what is desired (unhappiness over one's control), ... then the measure of powerlessness easily becomes an index of resentment, discontent, or the like-and in the latter case, dependent variables that are also (in one way or another) measures of resentment, despair, or mistrust become unpersuasive as correlates of the alienation index."8 Another student of powerlessness, Clark, takes exactly the opposite tack and defines alienation as "the discrepancy between the power a man believes he has and what he believes he should have."9 The notion here, of course, is that the evaluative component is the very essence of alienation and that measures focusing on dissatisfaction with some institution or relationship have sufficient integrity to stand on their own in an empirical analysis.

⁶These definitions are drawn from the critique of the relevant literature found in Martin Fishbein, "A Consideration of Beliefs, and Their Role in Attitude Measurement," in Readings in Attitude Theory and Measurement, ed. Martin Fishbein (New York: John Wiley, 1967), pp. 256-66.

⁷Melvin Seeman, "On the Meaning of Alienation," American Sociological Review, 24 (December 1959), 784.

⁸Melvin Seeman, "Alienation and Engagement," in The Human Meaning, pp. 510-11. See also ibid.

⁹John P. Clark, "Measuring Alienation Within a Social System," American Sociological Review, 24 (December 1959), 849–52, and John P. Robinson and Phillip R. Shaver, Measures of Social Psychological Attitudes, Appendix B (Ann Arbor: Survey Research Center, Institute for Social Research, 1969), pp. 209-10.

Without attempting to decide which definition of alienation (powerlessness) is "correct," it is clear that measurements emphasizing either the cognitive or evaluative components are worth constructing in an effort to understand the nature and consequences of subjective powerlessness. 10 This article focuses on an evaluative aspect of subjective powerlessness called power consciousness. It is defined as a person's evaluation of his or her power position and his or her explanation of the causes of any inadequacies or advantages perceived in this position. The concept of power consciousness goes beyond the existence of dissatisfaction or satisfaction to the explanation of the situation. the answer to the question "What is going on here?"11 The why of dissatisfaction or satisfaction-the nature of the explanations given for perceived power deprivation or advantage-is central to power consciousness.

How does the concept of power consciousness resemble the related concept of class consciousness? As Portes notes, Marx, in attempting to understand why exploited groups don't always rebel against their oppressors, developed "the social psychological concept of class consciousness: recognition by the working class of the structural determinants of its situation and correct identification of the classes are major factors promoting the political struggle of the proletariat."12 Power conscious-

10A good case can be drawn from Fishbein's "A-Consideration of Beliefs" for an approach which explicitly includes an evaluative component. He argues, in brief, that "all beliefs about an object carry some implicit or explicit evaluation of the attitude object" (p. 264). The problem is that "the same belief may have totally different attitudinal significance for different individuals" (p. 263). Therefore, "independent measures of the evaluative aspects of beliefs" (p. 265) should be utilized.

Following the logic of this argument, the power consciousness indicator developed in this paper measures each individual's power satisfaction in an explicit manner. In the section below on "The Place of Power Consciousness in the Matrix of Power Measures" I will examine the relationship between familiar cognitive indicators of subjective powerlessness and power consciousness.

¹¹Robert E. Lane defines general political consciousness as the answer to this question in *Political Thinking and Consciousness* (Chicago: Markham, 1969), p. 319.

¹²Alejandro Portes, "On the Interpretation of Class Consciousness," American Journal of Sociology, 77 (September 1971), 228. See also Bertell Ollman, "Marx's Use of 'Class'," American Journal of Sociology, 73 (March 1968), 573-80, and Seymour M. Lipset, "Social Class," International Encyclopedia of the Social Sciences, Vol. 15, pp. 296-315, for reviews of the literature. of the literature. A somewhat different view is found in Georg Lukács, History and Class Consciousness

ness borrows directly from Marxist conceptions of class consciousness since it seeks to identify if people see the source of their own powerlessness in the structure of social or political life. Unlike Marx's class consciousness, however, the idea of power consciousness is independent of a theoretical structure identifying the correct way people should feel or interpret their situations. There is no "false" power consciousness for our purposes. The concept is more empirically based (although it does not preclude the superimposition of a general theory including a notion of false consciousness); this allows an exploration of the most interesting questions or propositions which derive from the idea of class consciousness without a commitment to any one theory or view of the world as correct.

The following pages analyze survey data on power consciousness collected from a sample of residents of Detroit, Michigan, and several small jurisdictions in its environs. Profiles will be drawn of the responses of blacks and whites to a question measuring satisfaction or dissatisfaction with the amount of political power possessed and to a follow-up question on the explanation given for power dissatisfaction. The follow-up question allows me to discriminate between those who blame an unsatisfactory power situation on problems with the political system and those who place the blame on adequacies or shortcomings they perceive in themselves. 13 A composite of these questions will be constructed, labeled the "power consciousness" measure, and analyzed in relationship to other variables. The following topics will receive detailed attention: the place of power consciousness in the matrix of power measures; its relationship to background factors, especially level of education; its relationship to indicators of political discontent; and the impact of power consciousness on levels and styles of political behavior. Literature relevant to an empirical analysis of power consciousness will be reviewed in each section, but, since racial contrasts are drawn throughout, a brief comment on the reasons for a comparative analysis is best made at this point.

Racial comparisons of power consciousness are of special interest here because of the developing social-psychological literature on the power explanations of blacks. Gurin et al. have pioneered in this area. They developed and analyzed a special set of four race-related items in their elaboration of Rotter's IE (Internal-External) Scale which measures an individual's expectancy for control or self-efficacy. The items were administered to a sample of black college students in the mid-1960s and produced a factor labeled "individual-system blame." As Gurin et al. noted, "Consistently choosing the internal alternative on these four items means resting the burden for failure on Negroes themselves, specifically on their lack of skill, ability, training, effort, or proper behavior. In contrast, choosing the external alternative means attributing the responsibility for failure to the social system because of lack of opportunities and racial discrimination." ¹⁴ In a reversal of what one might expect from the usual I-E findings, those who chose the external control alternatives on the race-related items and blamed the system participated more in civil rights activities. When the students perceived systemic constraining forces as opposed to internal frailties as the source of blacks' failures, they reached a level of consciousness which motivated action against the prevailing system. 15

A number of provocative questions are raised by Gurin et al.'s discussion and findings which can be answered by analyzing the measure of power consciousness developed in this study separately for whites and blacks. Do whites and blacks tend to give different explanations for power deprivation? How strong, in relative terms (i.e., with social status controlled), is the tendency for blacks to blame themselves for inadequate power possession and, for that matter, to express satisfaction with the amount of power achieved? Are there different correlates and consequences of power dissatisfaction for whites and blacks?

⁽Cambridge, Mass.: MIT Press, 1971), who defines class consciousness, p. 73, as "the sense, become conscious, of the historical role of the class." The reader should examine pp. 46–82, "Class Consciousness," at a minimum.

¹³Explanations of satisfactory power situations were not secured from respondents in the study reported here.

¹⁴Patricia Gurin, Gerald Gurin, Rosina C. Lao, and Muriel Beattie, "Internal-External Control in the Motivational Dynamics of Negro Youth," *Journal of Social Issues*, 25 (Summer 1969), 45.

¹⁵ Ibid., p. 52. As Gurin et al. say (p. 33): "Instead of depressing motivation, focusing on external forces may be motivationally healthy if it results from assessing one's chances for success against systematic and real external obstacles rather than the exigencies of an overwhelming, unpredictable fate." See also John R. Forward and Jay R. Williams, "Internal-External Control and Black Militancy," Journal of Social issues, 26 (Winter 1970), 75-92.

The Data

Data for this analysis were gathered in a survey of people 16 years of age and older who lived in housing units in the areas covered by the *Detroit City Directory*. ¹⁶ The survey went into the field in September of 1970 and the last questionnaire was completed in April 1971. A total of 538 people were interviewed, 174 blacks and 394 whites. An additional sample of 225 (104 blacks and 121 whites) was interviewed at the same time as part of a panel of Detroit area respondents started in 1967. Blacks were interviewed by blacks, whites by whites.

Power Consciousness: A Profile

The major dependent variable in this study, the power consciousness indicator, is derived from answers to two very simple questions: (1) "Do you think people like you have too little political power, or just about the right amount?"; and (2) if the respondent replies "too little political power," "Why do you have too little power?"17 The phrase "people like you" in the lead-in question is meant to stimulate respondents, in Converse's words, to think of their "location in the capability pecking order." They should be stimulated to think of themselves as having a relative location, along with others, in the power order of the political system. The intention is to minimize any idiosyncratic explanations which focus on individual inadequacies as the source

16For a complete description of the sampling procedure, see Joel D. Aberbach and Jack L. Walker, Race in the City (Boston: Little, Brown, 1973), pp. 245-52. Aside from the city of Detroit, the areas covered by the directory were Hamtramck, Highland Park, the Grosse Pointes, and Harper Woods.

17A follow-up question was asked only of those who felt they had too little power because of my fear that a probe of the answer "the right amount of power" would appear to challenge that response, damage the rapport between interviewer and interviewee, and bias other responses. The spontaneous comments given to our interviewers, however, and a follow-up question asked of those saying "right amount" on a survey administered to a small sample in Detroit, lead me to believe that these dangers are minimal. The question on the small Detroit study, incidentally, yielded answers focusing mainly on the existence of the franchise, other possibilities for participation, and general system satisfaction. My thanks to Professor Monica Blumenthal of the Survey Research Center at the University of Michigan for placing the item on her questionnaire.

¹⁸Converse, "Change in the American Electorate," p. 335. of an unsatisfactory share of political power and thereby increase confidence in the significance of those that are given.¹⁹

Table 1 presents the answers of respondents to the power consciousness questions. The similarities in the answers of the panel and cross-section respondents are quite striking.²⁰ Nevertheless, only data for the cross-sections are displayed in Table 1 since these yield the best estimates of the views of the population. Panel data will, however, be added in the remainder of the tables in order to increase the number of cases in the analysis.

The first and most obvious thing to note is that black and white respondents are about equally satisfied with the amount of political power they believe people like them possess.²¹ I will examine any differences in the sources of these positive feelings later, but for now I would like to concentrate on the reasons given for dissatisfaction with the amount of power possessed. Since the percentages of satisfied respondents are quite similar for the two racial groups, I will use the percentages in the right hand columns—that is, the dissatisfied respondents only—in describing the results.

Unusually careful coding procedures for ensuring relaible data should be employed when applying a complex code to an open-ended question. My research assistant and I consulted on the coding of each explanation given by a respondent dissatisfied with the amount of power possessed. An additional coder then was brought in who independently scored each answer. Any discrepancies were resolved. However, the number of discrepancies was quite low and I am very confident about the reliability of the variable.²²

19While the phrase "people like you" was not used in the follow-up question, most people seemed to keep it as their frame of reference in answering.

20 Fifty-nine percent of the white panel respondents and 46 percent of the black panel respondents felt they had the right amount of power. More important, the distributions of reasons given for the power dissatisfaction were remarkably congruent between each racial panel and the appropriate cross-section, especially when one takes into account the slight bias to a more highly educated group found in the panel due to the greater attrition of lower as opposed to higher education respondents.

21It is likely that these percentages have dropped since 1971 when the data were collected, but in 1966 when the same question was asked on the SRC Election Study, 55 percent of the whites and 60 percent of the blacks living in the central cities of the 12 largest SMSA's said they had the right amount of power.

22There was total agreement between the first and second codings for 82 percent of the answers by

Table 1. Power Consciousness for the Cross-Section Sample, by Race

	White C	Cross-Section	Black C	ross-Section
Power Consciousness ^a	Total	Too Little Only	Total	Too Little Only
Just about the right amount of political				
power	60%		55%	with
Too little, because of				
 Personal (individual) failure or inadequacy Lack of initiative, interest, personal inadequacies (blames people like self or self) Personal inadequacies (no blame of 	7	19%	15	33%
people like self or self)	1	3	3	7
Subtotals	8	$\frac{3}{22}$	3 18	40
 Isolated individuals Isolated individuals (no mention of group): ordinary people do not count; one person, little man does not count Isolated individuals (emphasis on need for group): individuals alone can't do much, must belong to a group to have 	3	8	3	7
influence	4	10	1	1
c. One vote alone doesn't count	i		_	_
Subtotals	8	$\frac{3}{21}$	4	8
 3. Group weakness a. My group not powerful enough: unorganized, a minority, not cohesive, etc. b. Black people not powerful enough: unorganized, not cohesive, don't hold enough offices, etc. c. Same as 3b, but specifically indicates that whites are responsible Subtotals 	1 - - 1	3 - - - 3	1 8 1 10	1 17 3 21
Political system level problems	-	_		
 a. Nominating procedures are closed, unfair b. Electoral system (method) problems c. Government leaders don't care about people, don't listen to them, ignore 	3 2	7 4	Ξ	
them	7	19	6	13
d. Government leaders corrupt	1	2	_	
 e. Vested interests, cabal, run the government f. Government (as an institution) doesn't function well, is nonresponsive, too 	3	8	5	11
large, etc.	_6	14	3_	7
Subtotals	22	54	14	31
Totals	99%b (N = 365)	100% (N = 146)	101% ^b (N = 165)	100% (N = 75)
Other	(N = 3)	(N = 2)	(N = 1)	(N=1)
Don't Know, No Answer	(N = 26)	(N = 11)	(N = 8)	(N=2)

^aQuestion: Do you think people like you have too little political power or just about the right amount? (If too little) Why do you have too little power?

bTotals do not sum to 100 percent because of rounding.

The data are arrayed along a crude dimension running from dissatisfaction explained in terms of personal failures to explanations focused on problems of the political system. The rationale for this arrangement is that each major category represents an increasing level of power consciousness culminating in a belief that people have too little political power because of faults in the institutions, personnel, or procedures of the government. Answers were coded so that higher level responses received priority. For example, a person citing a political system level problem and a group weakness problem would be coded into one of the political system categories for purposes of this analysis.

1. Too Little Power Due to Personal Inadequacies. Turning to the first major category of those who felt they had too little power (people citing personal failures or inadequacies), 33 percent of the dissatisfied black respondents and 19 percent of the dissatisfied whites said they or people like them lacked the initiative or interest to achieve the right amount of political power. The blame for this situation, most clearly believe, rests squarely on their own shoulders since it is only their personal failures which account for this state of affairs. Typical responses follow:

(black, male, age: 45) Because people like myself do not get out and let the government know what we, the little people, want from them.

(black, female, 25) People like me don't usually take advantage and do what they could. Most people don't speak out or vote when they should.

(black, female, 28) For one thing, I don't take interest in it like I should and never express my beliefs when in a group.

(white, female, 22) Well, a lot of them are like me, they don't keep up with politics. (Interviewer probe: Why?) They're just not interested.

(white, female, 69) I'm dumb. (Laughed) I'd follow it. Every time I did vote, I'd vote for the wrong one. We'd have an argument, my

whites and 79 percent of the answers by blacks. Most of the disagreements (12 percent for whites and 15 percent for blacks) were about the order of the appropriate codes (i.e., what should be coded as the primary response and what as the secondary response), not the meaning itself. In only 6 percent of the cases for each race was there complete disagreement about the meaning of a response, and these disagreements were well spread out among the code categories.

husband and I. I really don't know. So many things that I really don't understand.

(white, female, 37) It's our own fault. We don't go out and do more about it. We just sit back; guess we are the silent majority. We don't make any noise, you know, go out and march and picket. That's not for me. I've other things more important to do; not that the government isn't important, but my family and home are more important to me.

(white, male, 58) If you get involved you can have it. If you abstain then you don't have any political power. I have not extended myself.

One would have to read very deeply into these comments to catch even a glimmer of blame placed on the political system. In fact, it is difficult to discriminate these respondents from many who said they were satisfied with the amount of political power they possessed and then went on to add spontaneous comments like: "When I vote"; "As much as we're entitled to"; "If we make an effort"; "If we use it"; "If I vote, I would have enough. I don't get out anymore, much"; "If I aspired to more political power I could have it. The fact that I have no such aspirations, that I don't get out and beat the bushes, work as hard as those who are active in politics, doesn't give me any excuse to cry about my lack of power."

A small group of whites (three percent of those dissatisfied with the amount of power possessed) and a slightly larger group of blacks (seven percent) also cite personal inadequacies, but they do not blame themselves for the situation or, at least, one could only infer self-blame from their statements through a very loose interpretation:

(white, female, 78) It's my age. If I were a young lady, my point of view would be different.

(black, male, 56) I don't have the actual education.

2. Too Little Power Due to Individual Isolation. The second cluster of reasons given for unsatisfactory amounts of power all involve individual isolation. Virtually equal percentages of dissatisfied whites and blacks (eight and seven percent respectively) see the individual or ordinary person as cut off, without contacts, unable to mobilize support:

(black, female, 34) I'm just one person. I don't think my word would go too far.

(white, male, 28) The little man cannot express himself and be heard.

(white, male, 53) I guess because one person can't do nothing alone.

Another group within the isolated individuals heading also believes that people like them are isolated and unable to do much, but, unlike the respondents described above, they emphasize the need for group affiliation or assistance in order to gain adequate influence. This category is populated almost exclusively by whites²³ (ten percent versus one percent of the dissatisfied blacks):

(white, male, 22) No one individual has power. It has to be united with others to have any effect.

(white, male, 42) Because nobody represents us as an individual. There's no organized group who represents the individual vote.

(white, female, 50) I don't believe there are enough of us. (Probe: What do you mean?) People are not interested in doing anything. If you could get enough people together who were interested they could do something as a strong group.

3. Too Little Power Due to Group Weaknesses. Group weaknesses are the focal point of a third major heading used to identify responses. This category is dominated by blacks who are dissatisfied with the amount of political power blacks possess.²⁴ I have divided the black powerlessness answers into two categories. The first (17 percent) stresses that black people are not sufficiently organized, are not cohesive enough, or don't hold enough offices. It is composed about equally of those who state this as a sad fact of life at this moment and those who imply that blacks could change things if only they were sufficiently motivated. The following are typical responses of those who see this as a sad fact of life:

(black, male, 37) The trouble lies in the fact of not getting enough people organized. (Probe: How do you mean?) Not just people, but black people.

(black, female, 22) Everything we want to change isn't getting changed. The reason must be we don't have enough power—pressure—as a group.

²³Whites also comprise the small percentage of respondents who say, to quote one of our respondents, "my one vote doesn't mean much."

²⁴Without going into detail, the small number of respondents in the group weakness category who were not concerned with race mentioned a variety of groups. For example: "people like me—the minority better educated group—[who] have little influence over the majority group."

(black, male, 52) They don't have enough black people in political offices.

Respondents who imply that blacks could change things if only they were sufficiently motivated gave answers like the following:

(black, male, 37) If we were strong enough Austin²⁵ would have been in, but some of our people wouldn't vote for him and some didn't vote at all. (Probe: Anything else?) The faith in one another.

(black, female, 36) Because we have quite a few people that can vote and they don't, thus not giving the Negro much voice.

A second (three percent) and much smaller group consists of people who specifically indicate that whites are responsible for the fact that blacks do not have enough power:

(black, female, 48) [They] don't really let blacks do everything whites do.

4. Too Little Power Due to Political System Level Problems. The last major heading along the power consciousness continuum is "political system level problems." Respondents in the categories listed within it mention aspects of the political system as the source of the inadequate power they believe people like them possess. Almost twice as many whites as blacks (54 percent versus 31 percent) give explanations of this type.

Notions that nominating procedures are closed or unfair or that the electoral system does not function properly were prominent among whites, but unmentioned by blacks:²⁶

(white, male, 27) You don't have any say about who's nominated.

(white, female, 27) Because of the way the political conventions are set up. The delegates basically are people very active in politics. If you are not an active individual you have no say as to who is nominated. Also, I don't care for the electoral college; should be by direct vote of the people.

²⁵Richard Austin ran for mayor of Detroit in 1969 and lost by a very slim margin. He ran successfully for Michigan secretary of state in 1970 and is now the highest ranking black state office holder.

26The small number of people coded under isolated individuals in the category "one vote alone doesn't count" were not coded here or under 4.f. because their responses did not clearly indicate that the political system itself was at fault. It is possible that they believe this is so, but it seemed a more conservative convention to assume that they see themselves as isolated, symbolize this isolation in terms of the vote, and do not blame the situation on some problem with the political system.

[This] may not actually give the people more power, but they will feel more involved.

Relatively large percentages of whites and blacks (19 percent and 13 percent respectively) believe that they do not have the right amount of political power because government leaders are simply unresponsive—they don't care about people and don't listen to them:

(white, male, 60) What are we going to do? We got no choice. (Probe: What do you mean?) Doesn't matter who's in there, they are going to do what they want. We got nothing to say about it. We vote them in, but as soon as they get in they forget their promises; so nothing we can do.

(white, female, 67) Politicians are voted in, then they do what they want rather than what people want. All lobbyists should be eliminated. They are all paid by large interests.

(white, female, 42) We can write our letters, etc., but if our congressmen feel one course is what they want to take, they do as they please.

(black, female, 39) Normally we don't have too much of a say so when it comes to politics. The politicians, once they get elected, they do what they want to anyway.

Unresponsiveness fades into a deeper charge of corruption for some and, for others, a sense that powerful forces are firmly in control of the political system (10 percent of the whites and 11 percent of the blacks):

(white, female, 51) Because a little man doesn't stand a chance. You have to know someone in office or have a lot of money. (Probe: Why is that?) All everyone ever seems interested in anymore is money. You have to pay somebody off to get a favor.

(white, male, 29) Too many monied people who run everything. There is a lot of big money behind politics.

(black, female, 38) Because most of the people with the money seem to control the government.

A final group of respondents who give system level problems as the reason that people like them have too little political power is composed mainly of whites. They tend to see the government as a large, nonresponsive, poorly functioning institution:

(white, male, 28) I feel when I vote the government turns it around the way they

want. It is not a fair system. The Supreme Court changes a lot of votes too. They change the people's opinions.

(white, female, 34) Because our democracy is not a democracy and our government is too complex. It's a representative [government], but it's like being ruled by a group of people ... how can you possibly know them? There are too many to actually know what each [other are] doing.

Summary. Whites are only slightly more satisfied than blacks with the amount of political power they feel people like them possess. There are, however, substantial differences in the distribution of reasons that the two racial groups give for power dissatisfaction. Blacks are much more likely to believe that individual failures or inadequacies are the source of the unsatisfactory power distribution. The emphasis here is on the lack of initiative or interest of people who could achieve enough power if only they were willing to make the effort. Blacks are also more prone to cite group weaknesses when explaining why people like them have too little power. They pay substantial attention, not surprisingly, to the problems blacks face in organizing adequate pressure, but there is also some disillusionment with the efforts members of their race have put forth on their own behalf. Whites, more often than blacks, stress that people like them are isolated and alone-in need of a group affiliation or an organized effort to gain adequate attention to their interests. Most striking is the fact that 54 percent of the dissatisfied whites, as compared to 31 percent of the blacks, talk about problems with the political system. Particular attention is given to problems with the nominating or electoral systems and to the nonresponsiveness of leaders and institutions.

In the sections which follow I will analyze some of the correlates and consequences of power consciousness. The responses in Table 1 were arrayed along a crude dimension of power consciousness running from satisfaction with the amount of power possessed, to dissatisfaction ascribed to personal failures, to dissatisfaction explained in terms of problems with the institutions, personnel, or procedures of the government. The many categories and subcategories identified are important for descriptive purposes, but now that we have a better understanding of the frequencies and nuances each represent it is necessary to combine categories so that the data are in a more usable form for analysis.

The indicator of power consciousness that I

will use is presented below along with the rationale for each category:

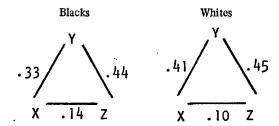
- Right amount of power: Self-explanatory.
- 2. Too little because of personal failures or inadequacies (self-blame only): This category contains only those who blame themselves or people like themselves for an unsatisfactory power situation. There is no hint in their explanations of blame attached to the political system.
- 3. Too little because of isolation or group weakness: This category is an inclusive one meant to embrace all those whose explanations hold neither themselves nor the political system directly responsible for an unsatisfactory amount of power. The category includes the small number of respondents who mention personal inadequacies, but indicate no self-blame (1.b. in Table 1) and excludes blacks who say people like them do not have enough power and then go on to indicate specifically that whites are to blame. The latter answer seems so clearly to imply blame of "the system" that I felt it would be reasonable to place it in category 4.
- 4. Too little because of problems with the political system: All explanations involving problems with the institutions, personnel, or procedures of government are included here. The one exception is described in the last sentence of paragraph three above.

The Place of Power Consciousness in the Matrix of Power Measures

In the introduction a distinction was drawn between measures of individuals' perceptions of their ability to influence government—mainly a cognitive dimension—and measures more oriented to tapping the evaluative aspects of powerlessness. The primary purpose of this section is to see how the power consciousness measure fits into the matrix of power measures. I will examine the relationships among three measures of perceived political powerlessness: One, a straightforward cognitive (probability) measure, taps the "expectancy that one's own behavior can control the occurrence of [political] rewards."²⁷ It is a modified version of the Almond-Verba "Political Competence" scale,

X = Perceived Ability to Influence Government*
Y = Amount of Political Power Held by "People Like

You"
Z = Power Consciousness



*All variables are scaled so that high scores equal high efficacy or satisfaction.

Figure 1. Relationships (Gamma) Between Power Measures, by Race

also called the "Ability to Influence Government" scale by Muller.²⁸ A second cognitive measure taps respondents' assessments of the amount of political power people in their situation possess.²⁹ A third measure, clearly evaluative, is the power consciousness indicator which measures discontent with the amount of power held.

Relationships among the three measures are depicted in Figure 1.³⁰ No assumptions about causality are made in the figure, although a simple causal model of the form $X\rightarrow Y\rightarrow Z$ fits

- ²⁸Muller, pp. 794-801. The following questions were asked in deriving the "Ability to Influence Government" index:
 - 1. Suppose a law were being considered by the Congress in Washington that you considered very unjust or harmful. What do you think you could do about it?
 - 1a. If you made an effort to change this law, how likely is it that you would succeed: very likely, somewhat likely, or not very likely?
 - 2. Suppose a law were being considered by the *[name of local government unit]* Common Council that you considered very unjust or harmful. What do you think you could do about it?
 - 2a. If you made an effort to change this law, how likely is it that you would succeed: very likely, somewhat likely, or not very likely?
- ²⁹The item is worded as follows: How much political power do you think people like you have? A great deal, some, not very much, or none?
- 30Gamma is used because it is a widely known and understood ordinal measure of association. It is highly sensitive, however, and can distort when there are extreme marginals. Therefore, the tables used to calculate the Gamma coefficients were checked for each coefficient reported in the article in order to guard against unreliable results.

²⁷Seeman, "Alienation and Engagement," p. 472.

these data reasonably well.31 Whether this simple model is correct or not,32 it is apparent that the direct link between Variable X (Perceived Ability to Influence Government) and Variable Z (Power Consciousness) is quite weak (.14 or below for both whites and blacks). Many people who are confident that they can influence governments to do the things they want are not satisfied with the amount of political power they believe people like them possess. The link between Y (the Amount of Political Power Held by "People Like You") and Variable Z (Power Consciousness) is much stronger than that between X and Z, although it is possible that the magnitude of the Gamma coefficient is an artifact of the common phrase "people like you" in both measures. The correlations among all three variables, however, are quite modest considering that all deal with aspects of political powerlessness. It is clear, therefore, that discussing the differences between measures of powerlessness that focus on the cognitive (probability) dimension and those which explicitly include an evaluative dimension is more than just a conceptual exercise. This raises the distinct possibility that power consciousness has different correlates than the measures of political efficacy usually employed by political scientists.

Correlates of Power Consciousness

Objective Social and Demographic Factors. The strong, positive relationship between high social status and a feeling of political efficacy is one of the most consistent findings in political survey work. "In fact, there is support for the proposition that advanced education is the single most important factor contributing to high efficacy." 33 and the assumption is readily

³¹If one specifies that $X \rightarrow Y \rightarrow Z$ is correct, then one would predict that $\Gamma_{XY,Z} = 0$. The relevant data for blacks and whites are as follows:

32That power satisfaction or dissatisfaction is basically the end-product of perceived ability to influence government rather than the other way around seems logical enough, although there are grounds for arguing the other way. See Muller, pp.

made that this reflects an accurate reading of the power situation. When it comes to evaluations of the amount of power people have, however, advanced education is, if anything, a predictor of power dissatisfaction.³⁴ Upper status respondents, rather than lower status respondents as one might expect from the literature on efficacy, believe that people like them do not possess enough political power. Tables 2a and 2b present the relevant data.

The relationships displayed in Table 2a between education and the full power consciousness variable is quite pronounced for blacks (Gamma = .27) and very weak for whites (Gamma = .09). As you will recall, Table 1 indicated that blacks are somewhat less likely than whites to say that people like them have the right amount of political power. Table 2a demonstrates that most of this difference can be accounted for by the unusually high level of power dissatisfaction found among college level blacks.³⁵

Table 2b presents the relationship between education and the reasons given for power dissatisfaction only (i.e., respondents believing that people like themselves have the right amount of power have been removed). A factor that was visible in Table 2a is readily apparent here: when lower education respondents, white or black, are dissatisfied with their political power positions, they are very likely to explain the problem in terms of personal inadequacies. College educated respondents, on the other hand, are the most likely to explain power dissatisfaction by citing problems with the political system. Significant differences between whites and blacks are found only among the high school educated respondents. High school educated blacks resemble grade school educated respondents in their reasons given for power dissatisfaction, while high school educated whites bear a closer resemblance to the college educated than to grade school educated respondents in their explanations of unsatisfactory amounts of power possessed. These factors (and the differing levels of formal education in the two racial communities) account for the finding in Table 1 that blacks are more likely to explain power dissatisfaction in terms of personal inadequacies than whites and

³⁴Education is not atypical of the SES variables in the Detroit study. Occupation and income are similarly related to power consciousness.

35This reinforces a picture of discontent among upper status blacks found in other analyses of the data

Table 2a. Power Consciousness by Education and Race

		Too L		***************************************		
Education	Right Amount of Power	1. Personal Inadequacies	2. Isolation or Group Weakness	3. Political System Problems	Totals	(N)
Blacks				-		
Grade school	63%	11%	17%	9%	100%	(64)
High school	50	19	18	13	100	(165)
College	39	8	17	36	100	(36)
1			Gamma = .27			
Whites						
Grade school	64	11	14	11	100	(92)
High school	58	9	12	22	101a	(247)
College	60	5	8	27	100	(143)
			Gamma = .09			

^aPercentage does not sum to 100 due to rounding.

Table 2b. Power Dissatisfaction Reasons by Education and Race

Education	1. Personal Inadequacies	2. Isolation or Group Weakness	3. Political System Problems	Totals	(N)
Blacks					
Grade school	29%	46%	25%	100%	(24)
High school	39	35	27	101 ^a	(83)
College	14	27	59	100	(22)
		Gamma = .26			
Whites					
Grade school	· 30	39	30	99a	(33)
High school	20	28	52	100	(103)
College	12	19	68	99a	(57)
•		Gamma = .36			

^aPercentage does not sum to 100 due to rounding.

whites are more likely to condemn aspects of the political system.

How should we interpret these data? Lane's study of the political ideology of the "common man" is very instructive. He reports fascinating data on the attitudes toward political power of a small sample of whites interviewed in depth. Power, most of his respondents believe, is possessed by those who organize and work for it: "If one has little power at any given time, it is his own fault for failing to organize his interests." The working and lower middle classes have power by virtue of their numbers

and what is lacking is a function of their own failure to exercise available opportunities. In a phrase, people get what they deserve. The social and political systems are not seen as very important factors in explaining the inequities which exist in American society.³⁷

The consequences of this type of thinking should be debilitating for those at the bottom of the status hierarchy. Ira Katznelson draws on insights flowing from the concept of false

³⁶Robert E. Lane, Political Ideology (New York: Free Press, 1962), p. 143. See also Richard Sennett and Jonathan Cobb, The Hidden Injuries of Class (New York: Knopf, 1972).

³⁷This view is also prevalent in whites' notions about the sources of the deprivations blacks experience in American society. See Angus Campbell and Howard Schulman, Racial Attitudes in Fifteen American Cities: A Preliminary Report Prepared for the National Advisory Commission on Civil Disorders (Ann Arbor: Survey Research Center, Institute for Social Research, 1968), pp. 30-31.

consciousness (as well as identification with the aggressor) to describe and explain blacks' interpretations of their relative powerlessness. He gives special emphasis to the notion that the underprivileged powerless will be prone to blame themselves for their predicament in liberal democratic societies with an ethos of equality of opportunity:

Given the onus of choice, the powerless internalize their impossible situation and internalize their guilt. Thus, Elkins has argued, the complete powerlessness of the slave (and of the inmates of German concentration camps) "produced noticeable effects on the slave's very personality." The slave often identified with his master, and accepted society's estimate of himself as being without worth. The less complete, but nonetheless pervasive powerlessness of blacks in America's northern shettos . . . has had similar effects.... The low self-esteem of the powerless may be especially acute in liberal democratic societies with an ethos of equality of opportunity, for then the individual, "free" to choose, may blame himself and/or his group for his predicament. 38

Recent work on black powerlessness by social psychologists has identified a developing "shift from self to system blame" ³⁹ by ghetto residents. My data on power consciousness indicate that college educated blacks have broken the chains of self-blame which may once have gripped them, but that high school educated blacks are still more likely than comparable whites to blame an unsatisfactory power situation on themselves and not on problems with the political system. ⁴⁰ Those who have not gone beyond grammar school, regardless of race, are equally prone to self-blame and share the same high level of satisfaction with the amount of power they possess.

The latter fact is explainable by the notion that most of those who are very low in social status, regardless of race, share a belief prevalent in the culture that there can "be no thought that the poor and broken [have] too little power—they [do] not deserve more..."41 There are, however, clear racial effects at the high school level on the reasons given for power dissatisfaction and these may

well reflect the special debilitating effects of the ghetto experience described in Katznelson's statement. High school educated whites are generally satisfied with their level of political power, as Lane indicates was the case for lower middle class and upper working class Eastport residents, but much less likely than comparable blacks to heap the blame on themselves if they are dissatisfied. As noted, when upper status (college educated) people of both races are dissatisfied, they are generally confident that the political system is to blame.

I do not wish to devote too much space to demographic factors, but should note that sex is an inconsequential predictor of power consciousness for both races. (Gamma is -.07 for whites and .02 for blacks.) Age, not surprisingly, is inversely related to power dissatisfaction. (Gamma is -.22 for blacks and -.20 for whites.) Younger respondents are less satisfied with the amount of power people like them possess and are more likely to explain power dissatisfaction in terms of deficiencies in the political system. The higher level of educational achievement of younger respondents, incidentally, explains part, but not all, of this relationship, reinforcing the interpretation cited above of a generational shift from self to system blame. Variables that have been used as indicators of traditionalism for blacks-where respondents were raised and whether or not they are church members⁴²-also have a clear impact on power consciousness. Blacks who were raised in Michigan are much more likely to be dissatisfied with the amount of power they have than are blacks raised in the South (Gamma = .31).⁴³ Blacks who are not church members are more dissatisfied than those who are members (Gamma = .21).44 In summary, younger blacks with less traditional backgrounds are the most dissatisfied with the amount of political power people like them possess. Traditionalism, in the sense used here, is not a meaningful variable for whites, but younger whites are the most dissatisfied,

Subjective Evaluations of Government. An ideal analysis of the correlates of power con-

³⁸Ira Katznelson, *Black Men, White Cities* (New York: Oxford University Press for the Institute of Race Relations, 1973), pp. 199-200.

³⁹Forward and Williams, p. 88. See also Gurin et al. for the seminal work in this area.

⁴⁰This is not a function of the lower number of terms of high school completed by the average black respondent in the high school group.

⁴¹Lane, p. 142.

⁴² Aberbach and Walker, pp. 124-30.

⁴³The Gamma for whites is -.02. Respondents raised in the South and border states are coded 0, those raised in Michigan are coded 1, and those raised in other parts of the North are coded 2. Respondents who lived in more than one place are coded in the category representing the place lived in the longest between ages 6 and 18.

⁴⁴The Gamma for whites is -.09. Church members are coded 0, nonmembers 1.

sciousness would be based on a longitudinal design which allowed for variation in such factors as economic conditions, income distribution, and the performance of the political system.⁴⁵ On the latter point, one would be interested in how power consciousness varied with the provision of public services and benefits, measured both objectively and in terms of perceptions about their quality and adequacy, and with citizens' overall evaluations of the responsiveness and legitimacy of government. Since this analysis is based on data gathered at one point in time I cannot correlate changes in power consciousness with changes in the level of services (measured either objectively or subjectively) or changes in perceptions of government responsiveness and legitimacy. I can, however, look at the relationship between power consciousness and measures of satisfaction with local government services and responsiveness and with a more general measure of basic trust in the authorities and institutions of government. The proposition to be tested is that scores on the power consciousness measure increase with negative perceptions of both the day-to-day workings of government and its overall trustworthiness.

45 Marxist notions about the dynamics of class consciousness make one especially interested in a longitudinal examination of the relationship between objective social status and power consciousness which covered periods of social and economic stress as well as equanimity. The expectation would be that significant numbers of lower class individuals would begin to blame the system for their powerlessness only in a time of crisis. See Lipset, p. 299.

Table 3 presents the correlations between power consciousness and measures of discontent with local government services and responsiveness as well as with an index of basic political trust which is composed of items focusing on both the federal and local governments. For purposes of this table, all variables are coded so that a positive coefficient indicates that power satisfaction is greatest when the respondent is satisfied with government services or responsiveness or is politically trustful, and power dissatisfaction blamed on problems with the political system is greatest when the respondent is politically distrustful or dissatisfied with services or responsiveness. References for each measure are found in the footnotes to the table.

All of the relationships are in the predicted direction, with the political trust correlations the highest. It is not too surprising that this is the case because the indicators dealing with specific government services and responsiveness are quite concrete and focused on the present, while the political trust measure is more abstract, represents more diffuse and basic evaluations of government, and is a reflection of deeper-seated feelings formed over a longer time span. The power consciousness measure shares these features with the trust measure and, if we wished to use the language commonly employed by alienation theorists, both consciousness and distruct could be labeled as variants of alienation.46

46See Seeman, "On the Meaning of Alienation," pp. 783-91, and "Alienation and Engagement," pp. 472-74.

Table 3. Correlations (Gamma) Between Measures of Political Discontent and Power Consciousness, by Race^a

Blacks		Whites
Evaluations of:		
City services ^b	.19	.20
Police behavior ^c	.15	.24
Likelihood of favorable policiesd	.21	.19
Political Trust Index ^e	.40	.31

^aFor purposes of this table, all variables are coded in the same direction, i.e., low scores indicate satisfaction or trust and high scores indicate dissatisfaction or distrust. See the text for a more detailed explanation.

^bThe City Services measure is a simple additive index of the number of city services about which the respondents expressed dissatisfaction. The index runs from 0 to 5. The services are garbage collection, parks and playgrounds for children, sports and recreation centers for teenagers, police protection, and public schools.

^cThe Police Behavior measure is a simple additive index of feelings that neighborhood police lack respect or use insulting language, frisk people without good reason, or use unnecessary force in making arrests.

dRespondents were first asked, "As you well know, the government in Detroit faces many serious problems. What do you personally feel is the most important problem the government in Detroit should try to take care of?" Then: "You have mentioned [cite problem]. What would you like to see the government do about that?" The question was followed by the item used in the analysis: "How likely do you think it is that the government will do what you want about this problem: very likely, somewhat likely, or not very likely?"

^eSee Aberbach and Walker, pp. 180-84 for a complete description of the political trust measure,

The introduction to this essay describes in some detail the debate within the alienation literature about the appropriateness of correlating primarily evaluative measures of powerlessness with measures of resentment, despair, or distrust since all share common elements of discontent. This issue will provoke debate for a long time because it encompasses both a highly complex methodological problem and a fundamental question about the nature of alienation. I am not in a position to offer a definitive resolution to it here, but it is clear that a person's power consciousness, as I measure it, is closely tied to his or her basic evaluation of the trustworthiness of governmental authorities and institutions.47 If government is seen as worthy of trust, people are likely to be satisfied with the share of political power they and others like them possess; if government is not to be trusted, then power distribution is more likely to be seen as unsatisfactory, and, as the data in the tables used to calculate the trust correlations demonstrate, some problem with the political system is seen as the reason for the unsatisfactory situation. The relationship is obviously reciprocal, with each evaluation (power dissatisfaction and political distrust) feeding the other, although the correlation is certainly not strong enough to claim that the two always occur together. 48 The two variables share much in common, but they do not appear to lie on the same psychological dimension and can be distinguished empirically.

Power Consciousness, Racial Militance, and Political Behavior

What attitudinal and behavioral patterns should we expect from those who do not feel that they have the right amount of political power? There are some suggestions in the literature. As noted earlier, Gurin et al. hypothesize that people who blame powerlessness on the social system, and for whom this is a reasonable rationale (oppressed minorities, for example) will cope better than others with adverse circumstances. In their research, Gurin

⁴⁷Incidentally, the measure of power consciousness is related to political trust with the measures of "Ability to Influence Government" and "Amount of Political Power Held by People like You" controlled.

48For example, it is easy to imagine people who distrust the government, but feel they have a satisfactory amount of influence. Litt describes this for efficacy operationally defined in a cognitive manner, but the logic is the same. See Edgar Litt, "Political Cynicism and Political Futility," Journal of Politics, 23 (May 1963), 312-23.

et al. found that black college students who attributed the problems faced by blacks to the social system rather than to individual qualities were more militant on racial matters, more likely to support confrontation tactics in the civil rights struggle, and engaged more in civil rights activities.⁴⁹ Portes, in a study of the impact of class consciousness on leftist radicalism in Chile, examined the effects on radicalism of separate measures of frustration with one's position in the social order and of structural versus nonstructural attribution of blame for the frustrations felt. He found that each had an independent effect, but there was no evidence of significant interaction effects.⁵⁰

The data collected in this study are not, unfortunately, ideal for examining the behavioral effects of power consciousness. They provide no detailed information on the civil rights activities described by Gurin et al. nor on the types of radical activities emphasized in the Portes study (participation in collective movements or leftist radicalism). They do, however, enable us to look at the relationship of power consciousness to racial militancy and to measures of participation in a series of conventional political activities as well as to a summary measure of participation in unconventional, nonviolent political activities.⁵¹ With regard to these data, both the class consciousness analogy pursued in this article and the logic of Gurin et al.'s and Portes' findings and speculations lead one to expect power satisfaction to encourage conventional forms of political participation, structurally explained dissatisfaction to be associated with unconventional techniques of political expression and with racial militancy for blacks, and dissatisfaction blamed on the self to lead either to inactivity or to very conventional attitudes and behavior.

⁴⁹Gurin et al., pp. 31-34, 47-52.

⁵⁰Portes, pp. 232-44.

⁵¹The study also contained a measure of participation in violent activities, but the number of participants in such activities in the type of community study from which these data were drawn was too small to permit a meaningful analysis. (Three percent of the black respondents questioned and none of the whites reported taking part in a violent protest such as a riot or rebellion.)

I use the terms conventional and unconventional here as defined in *Race in the City*, p. 203: "The term conventional techniques means those customarily used in standard American electoral or pressure politics. The term unconventional techniques refers to actions, ranging from peaceful protest to rebellion, which implicitly or explicitly threaten to disrupt the normal political process. Unconventional techniques need not be illegal, although they often are."

Turning first to black racial militancy, let us examine the relationship between power consciousness and approval of the black power slogan, a slogan which has been analyzed in depth as a component of militant ideology in the black community.⁵² Table 4 shows that approval of the black power slogan is highest among blacks who cited problems with the political system as the reason for their power dissatisfaction and about equal among those who are satisfied with the amount of power they possess and those who believe that personal inadequacies or failures are sufficient to explain an unsatisfactory situation. As expected, there is a distinct relationship between blaming the political system and not the self for failure to secure an adequate amount of political power and adopting more militant views on racial matters.53

With regard to the available data on political behavior, rates of participation in both conventional and unconventional activities were calculated for respondents classified according to their positions on the power consciousness measure. 54 The same basic pattern emerged for

both whites and blacks, although the percentage differences were generally quite modest.55 Those who were dissatisfied with the amount of power people like them possessed and who placed the blame for this on the political system tended to be the most active politically, regardless of the type of behavior. Those who blamed themselves for power dissatisfaction tended to be the least active, although the magnitudes of the differences in participation between this group and those who were satisfied with their political power or dissatisfied because of isolation or group weakness were not great. The overall pattern fit what Portes describes as the interaction model.⁵⁶ The factor stimulating political activity was dissatisfaction combined with blame of the political order, not dissatisfaction by itself. In fact, self-blame inhibited behavior slightly so that those who held themselves responsible for an unsatisfactory amount of power were usually the least active politically.

The data, then, suggest that political system explanations of power dissatisfaction are an impetus to political behavior.⁵⁷ However, the trend unexpectedly occurred for both the

Table 4. "Black Power" Approval and Power Consciousness

		Too.	Little Power because	of:
,	Right Amount of Power	1. Personal Inadequacies	2. Isolation or Group Weakness	3. Political System Problems
Percent approving black power ^a	55% (N = 119)	56% (N = 34)	65% (N = 43)	74% (N = 33)

^aQuestion: Do you approve or disapprove of "black power"?

⁵² See Aberbach and Walker, Ch. 4.

⁵³Since both black power approval and the reasons given for power dissatisfaction are related to education, the relationship between power consciousness and black power approval was examined with education controlled. The number of cases, unfortunately, makes some of the cell sizes much too small for reliable analysis, but the pattern is quite consistent on the crucial point: those who blame the political system for failure to secure an adequate amount of power are more favorable to black power than those who blame themselves.

⁵⁴The conventional activities were voting, wearing a campaign button or going to a political meeting, contributing money to a political campaign, and working actively in a political campaign. The measure of unconventional activities was a composite indicator of participation in nonviolent protests such as boycotts, marches, sit-ins, or picketing. (Only ten percent of the total black sample and six percent of the total white sample reported engaging in any such activities.) See fn. 51 for reasons that reports of violent activities were not analyzed.

⁵⁵A full display of these data in detailed, tabular form can be found in Joel D. Aberbach, "Power and Consciousness: A Comparative Analysis," paper prepared for the annual meeting of the American Political Science Association, New Orleans, La., September 4-8, 1973, pp. 25-26.

⁵⁶Portes, p. 242, describes the expected data display predicted by the interaction model as one where the attitudinal or behavioral dependent variable "will be inhibited among those who, being frustrated, do not blame society for their failures and will be stimulated among those who are frustrated and blame the social order for their situation."

⁵⁷The assumption that power consciousness causes behavior is made here in order to simplify the description. It is likely that behavior also influences power consciousness. Incidentally, controls for education (on the relationship between power consciousness and behavior) yield a rather complex pattern which is difficult to interpret with any confidence because of the small Ns in many of the categories.

conventional and unconventional modes of behavior measured, and the impact of political system blame, while fairly consistent, was not strong. With regard to highly unconventional behavior, such as taking part in riots or rebellions as a means of showing dissatisfaction or disagreement with government policy, there are some indications that the predictions derived from the literature hold, at least for blacks.58 Using a measure of approval of rioting or rebellion as a means of expressing discontent, we find that ten percent of those satisfied with the amount of power possessed approved, only two percent who are dissatisfied but blame personal inadequacies approved, while 21 percent of those giving political system explanations approved. The relationships are not so clear with other types of unconventional activities, such as sit-ins, however, and better data drawn from studies designed to yield a significant number of participants in a range of unconventional activities are necessary to support the point one way or the other.

Summary and Conclusions

Political scientists, long interested in political efficacy, have recently begun to examine the differences between the cognitive and evaluative components of operational measures of that concept. This article, drawing on recent studies of class consciousness and of powerlessness explained in individual versus system level terms, provides a comparative analysis of black and white respondents' satisfaction or dissatisfaction with the amount of political power they possess and of explanations given by them for power dissatisfaction.

The basic variable employed is a measure of power consciousness—defined as a person's evaluation of his or her power position and his or her explanation of the causes of any inadequacies or advantages perceived in this position. The operational measure arrays responses to two items on political power dissatisfaction (one of them open-ended) along a dimension of power consciousness running from satisfaction, to dissatisfaction ascribed to the personal failures of the respondent, to dissatisfaction ex-

⁵⁸As indicated above, too few respondents reported taking part in riots or rebellions to use this as a variable in the analysis. As a surrogate, I employed a measure of approval of such methods for showing dissatisfaction or disagreement with government policy (approved by ten percent of the total black sample and two percent of the total white sample), but approval, of course, is not the same as behavior. See Aberbach and Walker, pp. 203–06.

plained by reference to problems with the political system.

Whites are only slightly more satisfied than blacks with the amount of political power they hold, but the distribution of reasons the two groups give for power dissatisfaction is quite different. Blacks are much more likely to believe that individual failures or inadequacies are the sources of an unsatisfactory power distribution. The emphasis here is on the lack of initiative or interest of people who could achieve power if only they were willing to make the effort. Blacks are also more prone to cite group weaknesses as the reason for power dissatisfaction. Whites, more often than blacks, stress individual isolation as a source of power dissatisfaction, but mainly emphasize problems with the institutions, personnel, and procedures of government.

In spite of the fact that both the power consciousness indicator and the cognitive measures of political efficacy described in the text deal with political powerlessness, the relationships between them are quite modest. Power consciousness also has a different relationship than political efficacy to social status: the more highly educated tend to be the most dissatisfied. College educated blacks are the least satisfied with the amount of political power people like them possess, and both college educated blacks and whites are the most likely to explain power dissatisfaction in terms of difficulties with the political system. Lower status people of both races tend to condemn themselves rather than the political system for their unsatisfactory amounts of power. The majority of the lower status respondents are, in fact, satisfied with the amount of political power they have and the minority who feel they or people like them do not have the right amount of power often accept the blame for this situation. The negative impact of the black experience in the United States on black self-image is much in evidence in the power consciousness of high school educated blacks who are more likely to blame themselves for unsatisfactory amounts of power than are high school educated whites.

Power dissatisfaction is consistently, but modestly, related to measures of discontent with government services and responsiveness. It is also related to racial militancy for blacks. This, incidentally, coincides very nicely with the fact that the more traditional blacks—those who were raised in the South and maintain their ties with the church—are the most likely to say that people like them have the right amount of power. It is the militant, nontraditional blacks who are the most dissatisfied.

Political distrust is an especially potent predictor of power dissatisfaction. People who believe that government authorities and institutions do not deserve their trust are the most likely to believe that they have too little power. They are also the most likely to explain unsatisfactory power situations in terms of problems with the political system. The two feelings (distrust and power dissatisfaction) clearly feed on each other.

The behavioral correlates of power consciousness are worth a fuller exploration than that given here. Political system explanations of power dissatisfaction are associated with higher than average levels of political activity for both blacks and whites. This is true for both conventional and unconventional types of behavior. Blaming oneself for unsatisfactory amounts of power tends to depress activity slightly. Data on approval of rioting or rebellion as a means of expressing discontent indicate that it would be worth exploring, in some detail, the impact of power consciousness on unconventional behavior. To do this well, we would have to study purposefully selected samples which evidence high rates and a variety of disruptive activities.

This brings me to the question of directions for future research if we are to understand power consciousness more completely. A more sophisticated measure of the political power satisfaction dimension of power consciousness would be very helpful. Are people satisfied because they get what they want, or what they feel their own lack of drive and initiative condemns them to accept? How important are the highly visible symbols of democratic politics, like voting, in building satisfaction with the amount of political power people possess?

Longitudinal studies would permit us to trace trends in power consciousness and to understand the dynamic elements of the relationships discussed in this article. What are the impacts of changing economic and political conditions, measured both objectively and as perceived by people, on power consciousness? Does it take a severe crisis which undermines

the legitimacy of the political and economic systems before large percentages of the lower status population begin to express power dissatisfaction and to blame the political system for their dissatisfaction? What is the impact of changing government performance and actions (the data reported in this article are pre-Watergate, for example) on the perceptions of government services and responsiveness and, in turn, on power consciousness? How does the relationship between political trust and power consciousness change as situations change and the distributions of scores on the index change? What is the nature of the lags, if any, between changes in objective indicators of economic status and government performance, subjective reactions to these changes, and power consciousness?

A final set of issues concerns the impact of power consciousness on political behavior. The necessity for purposive samples of those who engage in unconventional activities is clear, but the suggestive data on the relationship of power consciousness to conventional participation indicate that more subtle information on political behavior might aid our understanding. We need better and more complete measures of the meaning of political behavior to people-that is, the subjective meaning of political expression as well as the type and quantity. Do those who are dissatisfied with the amount of power they have and condemn themselves for this situation tend to participate in a passive manner, doing their citizen duty in a ritual way as a matter of course (and perhaps, in rare cases, striking out in blind anger)? Do those who blame the political system perform their acts with an intensity brought on by anger, often lashing out at the authorities as a catharsis, or do they tend to be unusually purposeful in their mobilization of political resources? In larger terms, under what circumstances can power dissatisfaction be used by leaders for their own ends, when will it be dissipated in self-pity, and when can it be a creative force for political change?

On the Meaning of Political Support*

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The incumbent vs. system affect distinction is basic in the conceptualization of political support. It is based on the premise that system affect is a more important antecedent of aggressive political behavior than incumbent affect. The data reported here show that it is possible to distinguish incumbent from system affect empirically, and also theoretically important to make the incumbent-system distinction. Measures especially sensitive to incumbent affect correlate differently with ideology than does a measure especially sensitive to system affect. Byvariate correlations between measures of incumbent affect and a measure of aggressive political behavior are shown to be either spurious or indirect, due to the fact that incumbent affect is correlated with what appears to be a more powerful and direct antecedent of aggressive political behavior, namely, system affect. The theory behind the incumbent-system distinction is expressed in four propositions. In general, the data conform to it, but each prediction is qualified according to whether ideology and community context are inhibitory or facilitative.

Nearly two decades ago, in "On the Meaning of Alienation," Melvin Seeman addressed himself to clarification of one of the central concepts in social thought. Our concern here parallels his: clarification of an often-invoked yet imprecisely understood concept. In this instance, the concept is not alienation in general, but a specifically political variant of it; and the method by which we shall attempt to clarify its meaning is not theoretical but empirical.

The major work has been done.³ An abundance of constructs in the area of political

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¹Melvin Seeman, "On the Meaning of Alienation," American Sociological Review, 24 (December 1959), 783-91.

²We view the concept of political support and alienation as a continuous variable ranging from support at the positive end of the continuum to alienation at the negative end. Thus, when we use the terms "support" and "alienation" we are referring to the same variable.

³The most comprehensive theoretical treatment of political support is David Easton, A Systems Analysis of Political Life (New York: Wiley, 1965), pp. 153–140. A seminal, though brief, discussion appears in Seymour Martin Lipset, Political Man (Garden City: Doubleday, 1960), Ch. 3. See also Robert E. Lane, Political Ideology (New York: Free Press, 1962); Gabriel A. Almond and Sidney Verba, The Civic Culture (Princeton: Princeton University Press, 1963);

support and alienation have been proposed, but empirical validation of their utility is still in its infancy. Perhaps the most basic analytical distinction presented so far is that between affect for an incumbent administration versus affect for the political system. Yet, we do not know if this distinction is important.

What are the ingredients necessary to resolve the issue of the utility of the incumbent versus system distinction? Simply, one must have a set of variables which, on the basis of theory about the difference between incumbent evaluation and system support, relates to one differently than to the other—that is, discriminates between them. But that prescription has proved notoriously difficult to fill. This is because one member of the set of discriminating variables—perhaps the most necessary ingredient—is aggressive political behavior. And, in those soci-

William A. Gamson, Power and Discontent (Homewood, Ill.: Dorsey Press, 1968), ch. 3; David Easton and Jack Dennis, Children in the Political System: Origins of Political Legitimacy (New York: McGraw-Hill, 1969). Other studies dealing with the conceptual problems of political support and alienation include: Joel D. Aberbach, "Alienation and Political Behavior," American Political Science Review, 63 (March 1969), 86–99; Ada W. Finifter, "Dimensions of Political Alienation," American Political Science Review, 64 (June 1970), 389–410; Edward N. Muller, "Correlates and Consequences of Beliefs in the Legitimacy of Regime Structures," Midwest Journal of Political Science, 14 (August 1970), 392–412; John Fraser, "The Impact of Community and Regime Orientation on Choice of Political System," Midwest Journal of Political Science, 14 (August 1970), 413–33.

⁴We define aggressive political behavior as collective action on the part of nonelites that is illegal and has a political focus in that it is intended to disrupt the normal functioning of government. It is

eties best equipped to measure it systematically through highly developed survey research technology, aggressive political behavior has been difficult to find by means of normal crosssectional sampling.

This article reports the results of a study undertaken, in part, to come to grips empirically with the problem of determining the utility of certain analytical distinctions which have been made about the concept of political support. In particular, we shall focus on the question of the incumbent-system distinction. Also, in that context, we shall attempt to shed some light on the recent controversy over the status of political trust: Is political trust largely a measure of approval-disapproval of an incumbent administration? Or is it bound up closely with feelings about the legitimacy of the system of political authority as a whole?⁵

The question of the empirical meaning of political trust is especially important because of its frequent use in survey research.⁶ Though

important to the distinction between incumbent and system affect because the latter is thought to be a far more consequential antecedent of it than the former.

⁵This was the central issue raised in the recent Miller-Citrin debate. See Arthur H. Miller, "Political Issues and Trust in Government: 1964–1970," American Political Science Review, 68 (September 1974), 951–72; Jack Citrin, "Comment: The Political Relevance of Trust in Government," American Political Science Review, 68 (September 1974), 973–88; Arthur H. Miller, "Rejoinder to 'Comment' by Jack Citrin: Political Discontent or Ritualism?" American Political Science Review, 68 (September 1974), 989–1001.

6The literature employing the concept political trust (or "cynicism") is extensive, but for examples see Robert E. Agger, Marshall N. Goldstein, and Stanley A. Pearl, "Political Cynicism: Measurement and Meaning," Journal of Politics, 23 (August 1961), 477–506; Edgar Litt, "Political Cynicism and Political Futility," Journal of Politics, 25 (May 1963), 312–23; Herbert McCloskey, "Consensus and Ideology in American Politics," American Political Science Review, 58 (June 1964), 361–83; Frank A. Pinner, "Parental Overprotection and Political Distrust," The Annals of the American Academy of Political and Social Science, 361 (September 1965), 58–70; Robert E. Lane, "The Politics of Consensus in an Age of Affluence," American Political Science Review, 59 (December 1965), 874–95; M. Kent Jennings and Richard G. Niemi, "The Transmission of Political Values from Parent to Child," American Political Science Review, 62 (March 1968), 169–84; Kenneth P. Langton and M. Kent Jennings, "Political Socialization and the High School Civics Curriculum in the United States," American Political Science Review, 62 (September 1968), 852–67; Aberbach, "Alienation and Political Behavior"; Paul R. Abramson and Ronald Inglehart, "The Development of Systemic Support in Four Western Democracies," Comparative Political Studies, 2 (January 1970), 419–42; Jack Dennis,

commonly regarded as a measure sensitive to system affect, decisive evidence in favor of this interpretation has not yet been forthcoming. Citrin has called the prevailing interpretation into question, claiming that political trust is, in fact, a more sensitive register of affect for an incumbent administration than for the system of government.7 Miller has argued just the opposite position.8 Due to severe measurement constraints in the data at hand, neither Citrin's challenge nor Miller's rebuttal could really settle the issue. Satisfactory resolution of it would require, at a minimum, independent measures of incumbent and system affect, as well as measurement of variables-e.g., aggressive political behavior-which might discriminate between them. Although ample measures of incumbent affect were available, measurement of system affect and aggressive be-

"Support for the Institution of Elections by the Mass Public," American Political Science Review, 64 (September 1970), 819—35; Joel D. Aberbach and Jack L. Walker, "Political Trust and Racial Ideology," American Political Science Review, 64 (December 1970), 1199—1219; Edward N. Muller, "The Representation of Citizens by Political Authorities: Consequences for Regime Support," American Political Science Review, 64 (December 1970), 1149—66; Jeffery Paige, "Political Orientation and Riot Participation," American Sociological Review, 36 (October 1971), 810—20; Edward N. Muller, "A Test of a Partial Theory of Potential for Political Violence," American Political Science Review, 66 (September 1972), 928—59; Andre Modigliani, "Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy," American Political Science Review, 66 (September 1972), 960—78; Paul R. Abramson, "Political Efficacy and Political Trust Among Black School Children: Two Explanations," The Journal of Politics, 34 (November 1972), 1234—75; Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems," American Political Science Review, 67 (June 1973), 415—32; Richard L. Cole, "Toward a Model of Political Trust: A Causal Analysis," American Journal of Political Science, 17 (November 1973), 809—17; Meredith W. Watts, "Efficacy, Trust, and Commitment to the Political Process," Social Science Quarterly, 54 (December 1973), 623—31; Harrell R. Rodgers, Jr., "Toward Explanation of the Political Efficacy and Political Cynicism of Black Adolescents: An Exploratory Study," American Journal of Political Science, 18 (May 1974), 257—82; Miller, "Political Issues and Trust."

⁷Citrin, "Comment." See also Jack Citrin, Herbert McClosky, J. Merrill Shanks, and Paul M. Sniderman, "Personal and Political Sources of Political Alienation," British Journal of Political Science, 4 (September 1974), 1–31; Merrill Shanks and Jack Citrin, "The Measurement of Political Alienation: Strategic and Methodological Issues," paper prepared for the Conference on Political Alienation, Iowa City, Iowa, January 8–11, 1975.

⁸Miller, "Rejoinder."

havior was deficient. In this study we have sought to correct those deficiencies by including a multi-item measure that we think is sensitive to system affect, by using a measure of actual participation in aggressive political behavior, and by employing a research design intended to enhance variation in aggressive political behavior.

Theoretical Importance of the Incumbent-System Distinction

Why might one want to distinguish between affect for incumbents and affect for the system? And what are the objections which have been raised in regard to the utility of this distinction?

The hypothesis behind the distinction between incumbent affect and system affect is that the latter is more consequential for the stability of a political regime than the former. This hypothesis may be stated in very general form as follows: If system affect is negative among powerful or sizable segments of a polity, the threat to the stability of the prevailing regime will be great, even if affect for a particular incumbent administration is positive; conversely, if system affect is positive among powerful or sizable segments of a polity, the threat to the stability of the prevailing regime will be small, even if affect for a particular incumbent administration is negative. An application of this hypothesis to modern Anglo-American and European political history is Lipset's argument (where "effectiveness" refers to incumbent affect and "legitimacy" to system affect) that "when the effectiveness of various governments broke down in the 1930s, those societies which were high on the scale of legitimacy remained democratic, while such countries as Germany, Austria, and Spain lost their freedom, and France narrowly escaped a

⁹Citrin, in "Comment," p. 975, uses two single items (with limited response options) as surrogate measures of system support: one question elicits respondents' pride in "our form of government" and the other inquires whether a "change in our form of government" is needed. For a succinct discussion of the weaknesses inherent in single-item measures, see Jum C. Nunnally, Psychometric Theory (New York: McGraw-Hill, 1967), pp. 56-58; Also, as Citrin points out (p. 80; see also Shanks and Citrin, p. 15), cross-sectional surveys simply do not capture sufficient variation in antisystem behaviors. Because of the limitations of his data source. Miller ("Reioinder."

similar fate." 10 Societies presumed to be low on a scale of system affect ("legitimacy") experienced much higher rates of disruptive political protest and violence during the 1920s than did those presumed to be high on a scale of system affect. And when incumbent affect turned negative in those societies already threatened by low system affect, this was enough to bring about the demise of the prevailing regime.

Lipset's interpretation, and the hypothesis which underlies it, is intuitively plausible. But, it has not yet been cast into operational form and tested empirically. In the interim, serious doubts about the applicability of the incumbent-system distinction to the real world have arisen which center around the question of whether it is possible actually to measure something like "effectiveness" or approval of the performance of an incumbent administration, as opposed to something like "legitimacy" or support for the system of government in general.

In Loewenberg's view, the distinction between what Easton calls "specific" versus "diffuse" support—a distinction roughly comparable to effectiveness versus legitimacy—raises "insuperable problems of measurement." 11 These measurement problems arise from the fact that separate indicators of these analytical constructs generally are found to be highly correlated. The difficulties posed by this state of affairs have been elaborated succinctly by Shanks and Citrin:

At least in our Bay Area materials, the citizen's evaluations of incumbents and the political system are highly correlated enough that any analysis has to consider the rival hypothesis that indicators of disaffection from the system, on which "high" scores are relatively rare, merely assess a form of generalized dissatisfaction with government per se. But even if the attitude objects are conceptually distinct in the minds of all respondents (which we doubt), the very high correlation between the two theoretical dimensions would make dimensional analysis and item selection a hazardous business with unstable results.... This combination of highly intercorrelated theoretical dimensions and indicator items which are actually caused by more than one of the dimensions will, of course, produce hopelessly indeterminate statistical results, because the underlying causal structure is under-identified. 12

¹⁰Lipset, p. 69.

And if it turns out that the incumbent-system distinction is impossible to validate empirically, then it has no utility as part of any explanation of the stability or instability of a political regime, despite face plausibility.

In responding to these objections, Easton has raised the question: "Without discriminating in some way between specific and diffuse support could we explain adequately the occurrence of extreme political tensions, conflict, and discontent in some systems, especially democratic ones, without all these giving rise to serious threats to the stability of the regime, or political community?"13 To be sure, as he points out, "this is a transparent and universal phenomenon the explanation for which is not intuitively known." ¹⁴ But if it is not possible to discriminate between specific and diffuse support, then one must face the fact that it may be necessary to look elsewhere than to political support to account for this phenomenon.

Is there a way out of the dilemma posed by high correlation between measures of incumbent and system affect? Only if the correlation is not so close as to preclude the occurrence of enough instances of "inconsistent" combinations—positive incumbent affect together with negative system affect and vice versa—sufficient for reliable statistical analysis. If this condition is met, given that one also has available a measure of participation in political behavior potentially threatening to the stability of a political regime, then the theoretical utility of the incumbent-system distinction could be assessed. 15

Aggressive political behavior encompasses overt political actions which, if manifested by segments of a population that are powerful or sizable, can threaten the stability of a regime. Taking a person's rate of participation in aggressive political behavior as the dependent variable, the theory behind the incumbent-system distinction may be stated in the form of four propositions:

(P₁): If system affect is positive, participation in aggressive political behavior will show little tendency to vary with the extent to which affect for an incumbent administration is negative.

- (P₂): If system affect is negative, participation in aggressive political behavior will show a slight tendency to vary with the extent to which affect for an incumbent administration is negative.
- (P₃): If incumbent affect is positive, participation in aggressive political behavior will show a moderate tendency to vary with the extent to which affect for the system of government is negative.
- (P₄): If incumbent affect is negative, participation in aggressive political behavior will show a strong tendency to vary with the extent to which affect for the system of government is negative.¹⁶

What this theory says, in effect, is that if affect for an incumbent administration declines from positive to negative, the stability of a regime will not be threatened by virtue of aggressive political protest and violence, no matter how powerful or sizable the segment of the population registering this decline, as long as their affect for the political system is positive. But if affect for the political system were to decline from positive to negative, the threat to the stability of a regime posed by aggressive political protest and violence would be, at the very least, moderate, if powerful or sizable segments of the population were registering this decline; and the threat would become strong, if affect for the incumbent administration also turned negative.

Data

The data base for this investigation is a survey of 2663 adults carried out in the Federal Republic of Germany during the Fall of 1974 by *Infratest*. ¹⁷ Interviews were administered to

¹³David Easton, "A Re-Assessment of the Concept of Political Support," *British Journal of Political Science*, 5 (October 1975), 443–44.

¹⁴Ibid., p. 444.

¹⁵ For a discussion of this point, see Shanks and Citrin, pp. 15-21.

¹⁶ Any rules regarding the strength of ordinal measures of association are always strictly arbitrary, but, nevertheless, quite necessary for specifying predicted relationships. The following conventions are used in this article: The degree to which participation in aggressive behavior is expected to vary as a function of a predictor variable is defined in terms of the strength of the relationship as measured by an ordinal correlation coefficient. When the correlation (absolute magnitude) does not exceed .10, the strength of the relationship is considered practically zero. A relationship of slight magnitude is defined as a correlation ranging between .10 and .25; moderate magnitude is defined as a correlation ranging from .25 to .50; strong, a correlation ranging from .50 to .75; and very strong, a correlation ranging from .75 to 1.00.

¹⁷The interview schedule was prepared by the senior author and Jonathan Pool. Helpful advice in preparation of the schedule was received from many

a sample of persons residing in selected rural, urban, and university milieus throughout the Federal Republic. There were 12 sampling sites, each selected because opposition to the regime had been manifested there during the preceding five years at higher than average levels. In the rural and urban sites opposition had taken the form of voting support for extreme Left or extreme Right political parties. Oppositionist behavior in the universities had taken the form of civil disobedience and political violence.

The two major considerations of the research design were (1) to give the political support and opposition variables an opportunity to vary and (2) to investigate the effect of community context on relationships between political attitudes and behavior. Even though most civil disobedience and political violence in the Federal Republic has been manifested in communities with a university present, it was assumed that at least some such behavior also would be picked up in nonuniversity communities which had shown voting support for extremist parties; similarly, it was expected that higher than average support for extremist parties would register in the universities selected for inclusion in the design. A third consideration was to avoid completely sacrificing representativeness at the altar of enhanced variation. While the communities chosen are by no means representative of West Germany as a whole, they do capture basic regional and communitysize differences. Moreover, probability samples (supplemented by small samples of community influentials) of the population of each community were drawn so as to achieve representativeness within each research site.

The rural sites are four small villages: Friederichskoog and Neuenkirchen in northern Germany, Erpolzheim and Mauchenheim in southern Germany. From these sites a total of 569 persons were interviewed, of which 479 were drawn randomly from lists of eligible voters, 90 were drawn from lists of community influentials obtained from discussions with the mayor and other community leaders by the chief of the team of interviewers for each site. The urban sites are working-class sections of Bremen in northern Germany and Nürnberg in southern Germany. From these sites a total of 990 persons were interviewed, of which 928 were drawn randomly from lists of eligible

voters, 62 were drawn from lists of community influentials compiled from nominations submitted by persons in the eligible voters sample who were active in local organizations. The university sites are six of the major universities in West Germany: Berlin, Bochum, Frankfurt, Heidelberg, Köln, and München. A total of 1104 students and faculty from the arts and sciences at these universities were interviewed, of which 956 were drawn by quota sampling, 148 were drawn from lists of influential persons in various university organizations compiled from nominations submitted by persons in the quota sample. ¹⁸

Measurement of Political Affect

The concepts of effectiveness and specific support refer to evaluations of the actual performance of an incumbent administration. To measure affect for the incumbent administration in West Germany (a coalition government of Social Democrats and Free Democrats headed by Willy Brandt) respondents were first asked to sort a deck of 11 cards, each listing one general area of public policy, into two categories: (1) a task of the government and (2) not a task of the government. The policies were:

- 1. Guaranteeing justice for all.
- Providing welfare services for all who need them.
- 3. Effectively combating pollution.
- Creating more opportunities for all citizens to participate in making political decisions.
- Guaranteeing protection and security for individuals.
- 6. Providing for economic stability.
- Seeing to it that business corporations, unions, and universities are run in accordance with basic democratic principles.
- 8. Ensuring a strong national defense.
- 9. Providing for strong and capable political leadership.
- 10. Ensuring a free-market economy.
- 11. Providing for peace and order in society.

For each policy considered to be a task of the government, respondents were asked first to state how important they thought it was, then to grade the present government on its performance in that area. The grading scheme was

colleagues at the University of Mannheim, especially Rudolf Wildenmann and Uwe Schleth, and from Yola

¹⁸A quota sample was used to alleviate previous sampling problems. In early studies of students, it was very difficult to acquire a proper sampling frame. For detailed report of the sampling procedures see the

that used in the public schools (a 5-point scale, ranging from 1 through 5, where "1" stands for excellent work and "5" for failure). A mean score was computed across those policy areas considered to be a task of the government. This score was rounded to the nearest integer in the range 1-5. These mean policy evaluation scores constitute one measure of incumbent affect, the Policy Output Evaluation index.

After grading the incumbent government on its performance in particular policy areas, respondents gave the government an overall grade, without regard to specific policies. This single-item indicator, a second measure of incumbent affect, has a range of 1 to 5. We label it Incumbent Evaluation.

The concepts of legitimacy and diffuse support refer to affect for the political system. In Lipset's conceptualization, legitimacy is tied to affect for political institutions. Easton's conceptualization of diffuse support is more inclusive than this, encompassing affect for authorities in general, for the values, norms, and institutions of the regime, and for the political community.¹⁹ But legitimacy is seen as a fundamental component of diffuse support. Defined as belief that the authorities and regime "in some vague or explicit way ... [conform to a person's] own sense of what is right and proper in the political sphere," the inculcation of legitimacy sentiment is regarded as "the single most effective device for regulating the flow of diffuse support in favor both of the authorities and of the regime."20

If one approaches the problem of operationalizing the idea of affect for the political system from the perspective of the broad definition of legitimacy advanced by Easton, system affect can be seen to entail a number of components, each of which may, from slightly different angles, tap into beliefs that authorities and regime conform to a person's sense of what is right and proper. One component would be affect tied to evaluation of how well political institutions conform to a person's sense of what is right. Another component would be affect tied to evaluation of how well the system of government upholds basic political values in which a person believes. A third component

19In Easton's terms, diffuse support "is not contingent on specific inducements or rewards of any kind, except in the very long run. On a day-to-day basis, if there is a strong inner conviction of the moral validity of the authorities or the regime, support may persist even in the face of repeated deprivations attributed to the outputs of the authorities or their failure to act." Ibid., p. 278.

20 Ibid.

would be affect tied to evaluation of how well the authorities conform to a person's sense of what is right and proper behavior.

To measure system affect in the sense of belief in the legitimacy of the system of government respondents were given a deck of cards each containing a statement designed to capture an aspect of legitimacy sentiment. They were asked to report how strongly they agreed or disagreed with each statement, using a 7-point scale which ranged from "+3," labeled "agree completely," to "-3," labeled "disagree completely." English translations of the statements are given in Table 1.²¹

Items S_1 , S_3 , and S_4 refer to affect tied to evaluation of how well the system upholds basic values: S_1 is intended to capture system affect stemming from a person's belief that the political system does or does not operate in a way that is consonant with his basic values; S_3 and S_4 are intended to capture system affect which stems from belief that the political system does or does not operate in such a way as to uphold presumably consensual political values such as representation of citizens and protection of basic rights due to them. 22 Items S_2 and S_5 are intended to capture system affect stemming from a person's belief that political institutions and the political system in general do or do not conform to his own sense of what

21The German test of the items is: (S₁) Es macht mir Sorge, wenn ich an den Unterschied zwischen dem denke, was Leute, wie ich, im Leben wollen, und was atsächlich in unserem politischen System geschieht"; (S₂) "Die politischen Einrichtungen der Bundesrepublik sind mir lieb und wert und ich achte sic hoch"; (S₃) "Meine Freunde und ich fühlen uns in unserem politischen System eigentlich doch sehr gut vertreten"; (S₄) "Ich bin immer wieder erschrocken und betroffen darüber, dass die wesentlichen Rechte der Bürger in der deutschen Politik so wenig beachtet werden"; (S₅) "Heutzutage bin ich gegenüber unserem politischen System sehr kritisch eingestellt"; (S₆) "Die Gerichte in der Bundesrepublik gewähren jedermann einen fairen Prozess—es spielt dabei keine Rolle, ob er arm oder reich, gebildet oder ungebildet ist"; (S₇) "Die Grundeinstellung der Leute, die bisher in der Bundesrepublik politisch tonangeband waren, war immer in Ordnung"; (S₈) "Alles in allem genommen, verdient, die Polizei in der Bundesrepublik grossen Respekt."

 22 Two of these items are analogous in tone and content to items used in a measure called Political Alienation from the National Polity, developed in the United States by David C. Schwartz, Political Alienation and Political Behavior (Chicago: Aldine, 1973). Item S_1 is similar to: "I generally share the basic values exhibited in the policies and processes of American politics" (p. 265); item S_3 is similar to: "I feel that my friends and I are still really represented in our government" (p. 266).

is right and proper. 23 Items S_6 , S_7 , and S_8 refer to affect tied to broad classes of political authorities: politicians, the courts, and the police. The courts and the police are included because, as Easton and Dennis have argued in the context of the United States, such authorities symbolize the law; and in a constitutional, legal society (the Federal Republic is even more of a legalistic society than the United States) feelings about the legitimacy of the courts and police are probably bound up fairly closely with belief in the legitimacy of political authority in general.²⁴ These items are intended to capture system affect which stems from belief that the authorities in general do or do not conform to a person's sense of what is right and proper behavior: Have political authorities in the past been well-intentioned, regardless of their actual performance? Have the courts and

the police wielded their power properly, i.e., in the manner prescribed by their roles in the authority structure?

Thus, affect for the political system, as viewed here, is a wide-ranging, multifaceted concept, for which single-item indicators are inappropriate. Our goal in developing a measure of system affect was to identify a set of indicators of its different analytical components that (1) would show a pattern of at least moderate interitem correlations (no item correlating with a majority of the others at less than .3), and (2) a high overall reliability coefficient. The correlation matrix for those items which conformed to the desired pattern of interitem correlations is shown in Table 1.25 Their reliability coefficient yields an estimated correlation of test scores with true scores that

 $25\,\mathrm{Two}$ other items in the original political support scale were eliminated. One item ("The real political power in the Federal Republic is held by only a small elite; even citizens who have a great interest in politics have hardly any chance to influence political decisions") correlated less than .25 with all other scale items. The other item ("In general, the police treat everybody in the Federal Republic equally, regardless of whether they are poor or rich, educated or uneducated") was essentially a redundant measure to item S8 (r=.53).

Table 1. Intercorrelation Matrix (r): Political Support Items

	Political Support Items	S ₁	S ₂	S ₃	S ₄	S ₅	S ₆	S ₇	S ₈
S ₁ :	It makes me concerned when I think about the difference between what people like me value in life and what actually happens in our political system.	1.00	26	37	.41	.39	25	34	25
S ₂ :	I have great respect and affection for the political institutions in the Federal Republic.	•	1.00	.58	34	31	.40	.49	.55
S ₃ :	My friends and I feel that we are quite well represented in our political system.			1.00	36	37	.41	.50	.43
S4:	I find it very alarming that the basic rights of citizens are so little respected in our political system.				1.00	.36	26	30	22
S ₅ :	At present, I feel very critical of our political system.					1.00	21	32	22
s ₆ :	The courts in the Federal Republic guarantee everyone a fair trial regardless of whether they are rich or poor, educated or uneducated.						1.00	.40	.47
S ₇ :	Looking back, the leading politicians in the Federal Republic have always had good intentions.							1.00	.47
S ₈ :	Considering everything, the police in the Federal Republic deserve great respect.								1.00

Note: Kuder-Richardson Reliability Coefficient (KR-20), r_{kk} = .822; estimated correlation of test with true scores, $\sqrt{r_{kk}}$ = .907.

²³These items are analogous to two items used by Schwartz in a measure called Conformity-Acceptance of the American National Polity: "If people would only use the laws and institutions of our political system they would find that the system really works quite well"; "I used to accept the American political system more fully than I do now. Today, I feel highly critical of it," Ibid., p. 273.

²⁴Easton and Dennis, p. 293.

slightly surpasses the desired level of .9.26 The eight items, scored from 0 to 6, were summed to form an index ranging from 0 to 48, where low scores indicate support for the political system and high scores indicate alienation from the system.²⁷ This measure of system affect is labeled Political Support.

The fourth type of political affect dealt with in this study is, like Political Support, a putative measure of system affect; known variously as "political trust," "political cynicism," or "trust in government," it was developed by the Survey Research Center at the University of Michigan. Stokes, who first used the SRC "trust in government" items, was relatively unambiguous in specifying the construct which these items were designed to operationalize. According to him, they were included in the SRC biennial American election surveys for the purpose of tapping "evaluative orientations toward the national government."28 But were these evaluative orientations intended to focus only on an incumbent administration? Or on institutions of the national government as well? Or on even more diffuse system referents? Stokes explicitly cautions the reader against misinterpreting the meaning of the items: "Plainly we ought not to assume that the people expressing negative feeling toward government in this research were calling for fundamental change in the political order."29

Despite Stokes' warning, the SRC "trust in government" items often have been interpreted as if they measured affect for the system of government.³⁰ This may be due in part to the

 26 The standard Kuder-Richardson reliability formula was used for computing scale reliabilities. See Allen L. Edwards, *The Measurement of Personality Traits by Scales and Inventories* (New York: Holt, 1970), pp. 20–21; and Nunnally, pp. 191–97. The reliability index indicates the effectiveness of a measuring instrument and is a necessary, though not sufficient, condition for validity. The magnitude of r_{kk} is a function of the homogeneity of the scale and the number of items in the scale. For basic research, reliability coefficients around the order of .80 are considered desirable, because at that level correlations are not attenuated appreciably by measurement error. See Nunnally, p. 226.

²⁷Positively worded items were recoded so that all items ranged from positive to negative.

²⁸Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment," in Ethics and Bigness: Scientific, Academic, Religious, Political, and Military, eds. Harlen Cleveland and Harold D. Lasswell (New York: Harper, 1962), p. 64.

²⁹Ibid., p. 72.

³⁰See, for example, Muller, "Correlates and Consequences"; Muller, "The Representation of Citizens";

fact that the manifest content of the items appears to tap an aspect of diffuse support, called personal legitimacy by Easton, which derives "from the estimate of the personal merit and worth of the authorities rather than only from the validity of their position in the system or their compatibility with the ideological premises of the members."31 Also, Gamson subsequently defined the concept of political trust as synonomous with diffuse support, 32 and many researchers seeking an operational indicator of Gamson's trust concept-in the sense of diffuse support-seized upon the already available SRC "trust in government" items. Finally, and most importantly, empirical evidence has been forthcoming which suggests that trust in government is, in fact, bound up closely with feelings about the legitimacy of the system of government. A number of studies have shown that the "trust in government items" do correlate in the expected direction (negatively) with attitudes toward aggressive political behavior-just as predicted under the assumption that these items, regardless of their manifest content, actually comprise an instrument which captures affect for the political system.33

But there is an alternative hypothesis to account for the observed correlation between trust in government and favorable disposition toward aggressive political behavior. It is premised on the assumption that even if trust in government is largely a measure of generalized approval of incumbent politicians, an absence of association or even only a small association between trust in government and an instrument sensitive to system affect would be rather astonishing, since it would be logical to expect that trust in government might be a cause of system affect and/or system affect might condition a person's trust in government. If this assumption held, then an observed correlation between trust and aggressive attitude/behavior could be due to the fact that trust in government was associated with an unmeasured third variable, affect for the political system, which in reality was the true direct cause. In this case, a correlation between trust in government and aggressive attitude/behavior would disappear

Aberbach and Walker; Miller, "Political Issues and Trust"; Miller, "Rejoinder."

³¹ Easton, A System Analysis, p. 303.

³²Gamson, p. 45.

³³See, for example, Aberbach and Walker; Muller, "A Test of a Partial Theory"; Miller, "Rejoinder."

when system affect was controlled for—because it was either a spurious correlation or only an indirect one. Clearly, the spurious/indirect correlation possibility is a plausible rival hypothesis that must be tested.

Our measure of political trust is based on four items, English translations of which are given in Table 2, along with the interitem correlation matrix. The reliability coefficient yields an estimated correlation of test scores with true scores which is just slightly under the desired level of .9.

Two of the items in the political trust instrument, T_1 and T_2 , are variants of the SRC item which asks respondents directly about their evaluation of the trustworthiness of the national government.³⁴ T_3 and T_4 were designed to capture beliefs in the integrity of elected representatives and their concern for the public interest in general; they are analogous to SRC items, though developed by Alan

34One of these items, T₁, was presented to the respondents as part of the cardsort procedure used for the political support items, and scoring was identical to that for the support items. Cf. supra, p. 12. The purpose of administering this item with the support indicators was to get at feelings of trust in government as a whole—a different referent from T₂ (the government in Bonn). The modest correlation of the two items indicates they are tapping somewhat different beliefs, although only 14 percent of those scoring in the negative range on the trust-government-in-Bonn item were positive on trust-in-government, while 24 percent of those scoring positive on the former item registered in the negative range on the latter item. Item T was administered identically to T₃ and T₄; see below, fn. 35. The German text of the two items is: (T₁) "Man kann sich im allgemeinen darauf verlassen, dass die Bundesreigierung das Richtige tut"; (T₂) "Wie sehr vertrauen Sie darauf, dass die Regierung in Bonn so handelt, wie sie es eigentlich sollte?"

Marsh specifically for use in the Nine Nations Project, a cross-national study of values and political behavior in advanced industrial societies. The four trust items, scored from 0 to 4, were summed to form a Political Trust index ranging from 0 (very trusting) to 16 (very distrusting). 36

For the initial stage of the analysis—investigation of bivariate and first-order multivariate relationships—we divided each variable into five equal "zones" or levels of affect: very positive, positive, intermediate, negative, and very negative.³⁷ This gives a sufficient number of cases at each zone of affect for reliable statistical analysis, without distorting the full range of the discrete variable.

³⁵Alan Marsh is director of the British segment of the project. The German version of these items was prepared by Max Kaase and Hans Klingemann, directors of the German segment of the project. Items T₃ and T₄ were presented with T₂ as part of a series of questions that included personality and social control items. The German text of these two items is: (T₃) "Wenn Bundestagsabgeordnete oder Minister im Fernsehen oder im Parliament mit Journalisten sprechen, wie oft sagen sie Ihrer Meinung nach die Wahrheit?" (T₄) "Wie sehr vertrauen Sie darauf, dass Regierungsmitglieder das Wohl des Volkes über die Interessen ihrer eigenen politischen Partei stellen?"

 36 To attain comparability with the distributions of the other three trust measures, the response scores on either side of the neutral value for T_1 were collapsed (+1 and -1 with 0), providing for five response types in all. Response options for items T_2 , T_3 , and T_4 were: "about always," "mostly," "sometimes," "only very seldom," "never."

³⁷For both the Policy Output Evaluation and the Incumbent Evaluation variables the "very positive" to "very negative" zones correspond to each integer from 1 to 5 in ascending order. For the Political Support index, the "very positive" zone includes scores from 0

Table 2. Intercorrelation Matrix (r): Political Trust Items

	Political Trust Items	T ₁	T ₂	T ₃	T ₄
T ₁ :	In general, one can rely on the federal government to do the right thing.	1.00	.45	.29	.34
T ₂ :	How much do you trust the government in Bonn to act as it really should?		1.00	.55	.54
T ₃ :	When members of Parliament or cabinet ministers speak on television or in Parliament with journalists, how often in your opinion do they tell the truth?			1.00	.56
T4:	How much do you trust members of the government to put the interests of the people over the interests of their own parties?		ı		1.00

Note: Kuder-Richardson Reliability Coefficient (KR-20), r_{kk} = .770; estimated correlation of test with true scores, $\sqrt{r_{kk}}$ = .877.

Correlates of Political Affect

Our basic hypothesis about the connection between the measures of political affect is that they will show at least a moderate degree of fit to a strict monotonic function: as affect on any one variable becomes more negative, affect on any other variable will become more negative. There are two reasons why this hypothesis might hold. First, political affect could be unidimensional; if so, these variables would be correlated because each was a slightly different indicator of the same thing. Second, political affect could be multidimensional, but there might be causal links between dimensions; if so, these variables would be correlated due to asymmetric or symmetric dependencies. In particular, the two dimensions of affect might be those postulated analytically: evaluation of incumbent performance and support for the system of government. And these might be correlated because how one evaluates incumbent performance has an effect on one's support for the system of government, or because one's support for the system of government conditions how one views incumbent performance, or because both effects occur simultaneously or reciprocally. The upshot is that while the absence of correlation (at least for variables stipulated as incumbent affect versus those stipulated as system affect) would support the validity of the incumbent-system distinction, the presence of correlation would not militate against it.

As Table 3 shows, a rather sizable Taub correlation obtains between Political Support and Political Trust. These variables covary according to a strict monotonic function more closely than do Political Support and Incumbent Evaluation or Policy Output Evaluation. By comparison, 41.6 percent of those in the "very positive" zone on Political Trust score at the exact same zone on Political Support, whereas only 22.7 percent of those in the "very positive" zone on Incumbent Evaluation and only a miniscule 3.6 percent of those "very positive" on Policy Output Evaluation also score at "very positive" on Political Support.

Reading up the diagonal, on which all persons would be located if the relationship conformed to a strict monotonic function, a majority of those at "positive," "intermediate," and "very negative" Political Trust also manifest the corresponding level on Political Support, but there is no instance of a majority at any level of Incumbent Evaluation manifesting the corresponding level on Political Support, and a majority only of those who score "very negative" on Policy Output Evaluation also manifest the corresponding level on Political Support.

The empirical correlation between Political Support and Political Trust is stronger than that between Political Support and either Incumbent Evaluation or Policy Output Evaluation. Is this evidence that the Political Trust variable is more a measure of system than of incumbent affect? As we have already suggested, one should be wary of such an interpretation because of the possibility that Political Trust is just a measure of incumbent affect related more powerfully to Political Support either as cause or effect than are other measures of incumbent affect. Moreover, the data in Table 4 sound an empirical note of caution. Political Trust and Incumbent Evaluation-the variable tied most directly to straight incumbent affect from the standpoint of manifest content alone-show a healthy fit to a strict monotonic function. On the other hand, the relationship between Political Trust and Policy Output Evaluation is similar to that between Political Support and Policy Output Evaluation.

In sum, the relationships between these measures of political affect are in line with our initial hypothesis that they will be intercorrelated. But as to the question of the incumbent-system distinction, and where to place Political Trust if such a distinction can be made, the evidence is inconclusive.

To generate more conclusive evidence on the incumbent-system distinction we must turn to other variables which should relate to a measure of incumbent affect differently than to a measure of system affect. Ideology is a prime candidate. If one had a polity where an incumbent administration was widely perceived as differing from the overall system of government on a left-right continuum, then it would be logical to hypothesize that a measure of incumbent affect would show a different relationship to self-designated ideological position than would a measure of system affect. For example, let us suppose that a leftist political party is elected to power in a regime long dominated by rightists. Under this condition, what Easton calls the operating values and

to 9; "positive" includes scores from 10 to 19; "intermediate" includes scores from 20 to 29; "negative" includes scores from 30 to 39; "very negative" includes scores from 40 to 48. For the Political Trust index the "very positive" zine includes scores from 0 to 3; "positive" includes scores from 4 to 6; "intermediate" includes scores from 7 to 9; "negative" includes scores from 10 to 12; "very negative" includes scores from 13 to 16.

Table 3. Relationship Between Political Support and Political Trust, Incumbent Evaluation, Policy Output Evaluation

Political	Very P	ositive	Pos	itive	Interme	ediate	Neg	ative	Very N	legative
Support	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)
					Political	Trust				
Very negative	0	(0)	0.4	(2)	-	(19)	14.5	(84)	55.6	(85)
Negative	0	(0)	4.5	(21)		(192)	34.0	(197)	24.2	(37)
Intermediate	22.5	(20)	37.1	(173)	52.3	(526)	40.3	(234)	17.6	(27)
Positive	36.0	(32)	50.4	(235)		(247)	10.5	(61)	2.6	(4)
Very positive	41.6	(37)	7.5	(35)	2.2	(22)	0.7	(4)	0	(0)
TOTAL	100	(89)	100	(466)	100 (1	1006)	100	(580)	100	(153)
					Taub =	.48				
	-			Ir	cumbent l	Evaluat	ion			
Very negative	4.5	(2)	0.9	(5)	5.4	(55)	16.0	(69)	42.0	(58)
Negative	6.8	(3)	13.4	(77)	19.6	(201)	28.1	(121)	23.2	(32)
Intermediate	27.3	(12)	37.5	(216)	47.6	(489)	43.6	(188)	25.4	(35)
Positive	38.6	(17)	38.2	(220)	24.9	(256)	12.1	(52)	8.7	(12)
Very positive	22.7	(10)	10.1	(58)	2.6	(27)	0.2	(1)	0.7	(1)
TOTAL	100	(44)	100	(576)	100 (1	1028)	100	(431)	100	(138)
					Taub =	.34				
			***************************************	Pol	icy Output	t Evalua	ation			
Very negative	1.8	(1)	1.1	(6)	4.4	(52)	19.4	(90)	69.5	(41)
Negative	16.1	(9)	10.1	(53)	20.3	(238)	30.8	(143)	13.6	(8)
Intermediate	35.7	(20)	38.6	(203)	48.8	(571)	37.0	(172)	13.6	(8)
Positive	42.9	(24)	38.8	(204)	23.8	(278)	12.7	(59)	3.4	(2)
Very positive	3.6	(2)	11.4	(60)	2.6	(31)	0.2	(1)	0	(0)
TOTAL	100	(56)	100	(526)	100 (1	170)	100	(465)	100	(59)
					Tau _b =	.35				

Table 4. Relationship Between Political Trust, Incumbent Evaluation, and Policy Output Evaluation

	Very F	ositive	Pos	sitive	Intermediate	Ne	gative	Very l	Negative
Political Trust	%	(N)	%	(N)	% (N)	%	(N)	%	(N)
				Ir	cumbent Evalua	tion			
Very negative	4.2	(2)	0.5	(3)	3.0 (33)	11.8	(53)	42.0	(60)
Negative	4.2	(2)	13.0	(77)	21.4 (234)	45.6	(205)	42.7	(61)
Intermediate	16.7	(8)	39.5	(234)	53.1 (580)	38.9	(175)	15.4	(22)
Positive	41.7	(20)	37.4	(222)	20.8 (227)	3.3	(15)	0	(0)
Very positive	33.3	(16)	9.6	(57)	1.7 (19)	0.4	(2)	0	(0)
TOTAL	100	(48)	100	(593)	100 (1093)	100	(450)	100	(143)
					$Tau_b = .44$				
			***************************************	Pol	icy Output Evalu	ation	***************************************		
Very negative	1.4	(1)	1.3	(7)	3.5 (43)	13.4	(64)	66.7	(38)
Negative	20.8	(15)	14.2	(79)	23.2 (281)	44.5	(212)	26.3	(15)
Intermediate	37.5	(27)	40.2	(223)	50.5 (613)	35.9	(171)	7.0	(4)
Positive	30.6	(22)	33.9	(188)	20.8 (252)	5.7	(27)	0	(0)
Very positive	9.7	(7)	10.5	(58)	2.0 (24)	0.4	(2)	_0	(0)
TOTAL	100	(72)	100	(555)	100 (1213)	100	(476)	100	(57)
					$Tau_b = .36$		•		

norms of the system³⁸ would be heavily right-of-center in tone, whereas the tenor of the incumbent administration would be left-of-center. Then we would expect persons who designate themselves as leftist to register more positively than rightists on a measure of incumbent affect, while persons who designate themselves as rightist should register more positively than leftists on a measure of system affect.

Fortunately for our purposes, it is probably fair to characterize the Federal Republic of Germany today as a polity with a right-of-center regime and a left-of-center incumbent administration. The SPD, or socialist party, is currently in power.³⁹ But the regime began under the auspices of an American-dominated military government and for the first decade and a half of its existence was run by Adenauer's conservative party, the CDU/CSU.

Obviously, not everyone in Germany perceives the incumbent administration as leftist and the system of government as rightist; but if it is fair to expect that at least a substantial majority share this view, then our hypotheses with regard to ideology may be formulated as:

- (I₁): Measures especially sensitive to affect for an incumbent administration will register more highly in the negative range among rightists than will measures especially sensitive to affect for the system of government.
- (I₂): Measures especially sensitive to affect for the system of government will register more highly in the negative range among leftists than will measures especially sensitive to affect for an incumbent administration.

To test these hypotheses we use the leftright scale, previously shown to be a meaningful yardstick in the German context by Hans Klingemann. This variable, which we call Ideological Commitment, is a horizontal, visual scale consisting of ten boxes stretching from "Left" to "Right" (so labeled at the extremes). We have divided it into five equal categories, ranging from "extreme Left" to "extreme Right."

As Table 5 shows, 23.9 percent of those who locate themselves in the "extreme Right" posi-

tion on Ideological Commitment are negative on Political Support. By contrast, almost a majority of the extreme Right (49.2 percent) is negative on Incumbent Evaluation. Turning to the "Right" category on Ideological Commitment, we see that only 13.4 percent express negative political support, whereas 37.7 percent give the present government a negative grade. As predicted by the first hypothesis, the measure which we think captures system affect registers far less negative sentiment among rightists than does the measure which, on the basis of manifest content, obviously taps incumbent affect. And where does Political Trust fall? A full 52.8 percent of the extreme Right and 31.6 percent of the Right register negative trust. These rates of negative affect are about the same as those registered by rightists on Incumbent Evaluation, and they are more than twice as great as the rates of negative affect registered by rightists on Political Support. At this end of the Left-Right continuum, trust very much resembles the Incumbent Evaluation variable.

Now what about the Left? According to the second hypothesis, leftists should be more negative on Political Support than on Incumbent Evaluation-if these variables capture what we think they do. Our alleged system measure does, in fact, register considerably more negative sentiment on the Left than does our alleged incumbent measure: 69.8 percent of the extreme Left register negative political support. but their rate of negative sentiment for the incumbent administration is some 20 percentage points lower; 37.8 percent of the Left register negative support, but less than half this many register negative sentiment for the incumbent administration. And on Political Trust: For the extreme Left, the rate of negative sentiment on Political Trust is 56.6, quite close to their rate of negative sentiment on Incumbent Evaluation. But among the Left, the rate of negative sentiment on Political Trust (27.4 percent) lies almost exactly between their rate of negative sentiment on Political Support and their rate on Incumbent Evaluation. Again. trust looks more like the Incumbent Evaluation variable than the Political Support variable, though the differentiation is not quite as sharp on the Left as on the Right.

The relationship between Policy Output Evaluation and Ideological Commitment is, as would be expected, more similar to that which holds for Incumbent Evaluation and ideology than to that between Political Support and ideology. Among those at the "extreme Left" position, 51.1 percent register negative affect

³⁸Easton, A System Analysis, pp. 289-90.

³⁹The SPD administration also includes the FDP (Free Democrats), a small centrist party, as a coalition partner.

⁴⁰ See his "Testing and the Left-Right Continuum on a Sample of German Voters," Comparative Political Studies, 5 (April 1972), 93-106.

on the Policy Output Evaluation variable-a figure almost identical to the rate of negative sentiment which they register on Incumbent Evaluation (48.6 percent); likewise, those at the "Left" position register a rate of negative affect on Policy Output Evaluation (18.9 percent) which is almost the same as their negative rate on Incumbent Evaluation (16.3 percent). Rightists, however, register rates of negative affect on Policy Output Evaluation which lie between their comparatively high negative rates on the Incumbent Evaluation variable and their comparatively low negative rates on the Political Support variable. Of those at the "Right" position on Ideological Commitment, 23.4 percent express negative affect with regard to policy performance; 31.4 percent of those at the "extreme Right" position on Ideological Commitment are negative on the policy variable. The Right appears to be more negative toward the leftist incumbent administration in an "expressive" sense (the overall grade they give the incumbents, regardless of policies) than in an "instrumental" sense (the grade they give specifically on policy performance).

We have found that leftists and rightists register consistent and in many instances rather sharp differences on the measures of political affect, differences which coincide with our

predictions under the assumption that the Political Support variable is especially sensitive to affect for the system of government while the Policy Output Evaluation and Incumbent Evaluation variables are especially sensitive to affect for an incumbent administration. The Political Trust variable, despite its fairly close correlation with Political Support, relates to ideology in much the same way as the Policy Output Evaluation and Incumbent Evaluation variables. Thus, the analytic incumbent-system distinction does appear to be grounded in reality, and we can sort our measures of political affect according to it. But does it help us in any important way to account for political behavior?

Political Affect and Aggressive Political Behavior

We have formulated a theory about behavioral consequences of the incumbent-system distinction, suggested by the work of Lipset and Easton, in terms of four abstract propositions. The dependent variable is aggressive political behavior: that is, political behavior that is illegal, disruptive of the normal functioning of government, and may entail the use of violence.

Table 5. Relationship Between Political Support, Political Trust, Incumbent Evaluation, and Ideological Commitment

				Ide	ological (Commite	nent			
	Extre	me Left	L	eft	Ce	nter	Ri	ight	Extren	ne Right
	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)
Political Support							,			
Very negative	41.4	(92)	9.3	(71)	0.8	(6)	0.9	(3)	8.0	(9)
Negative	28.4	(63)	28.0	(213)	12.3	(89)	12.5	(41)	15.9	(18)
Intermediate	18.9	(42)	38.0	(289)	49.9	(361)	52.9	(173)	45.1	(51)
Positive	6.3	(14)	21.0	(160)	31.2	(226)	31.2	(102)	24.8	(28)
Very positive	5.0	(11)	3.7	(28)	5.8	(42)	2.4	(8)	6.2	(7)
TOTAL	100	(222)	100	(761)	100	(724)	100	(327)	100	(113)
Political Trust										
Very negative	29.7	(65)	4.2	(33)	2.4	(18)	4.0	(14)	8.9	(11)
Negative	26.9	(59)	23.2	(184)	22.4	(171)	27.6	(96)	43.9	(54)
Intermediate	27.4	(60)	46.0	(364)	46.1	(351)	46.8	(163)	35.0	(43)
Positive	7.8	(17)	23.0	(182)	25.1	(191)	18.7	(65)	10.6	(13)
Very positive	8.2	(18)	3.7	(29)	4.1	(31)	2.9	(10)	1.6	(2)
TOTAL	100	(219)	100	(792)	100	(762)	100	(348)	100	(123)
Incumbent Evaluation										
Very negative	19.6	(44)	2.1	(16)	3.9	(29)	7.0	(24)	15.3	(18)
Negative	29.0	(65)	14.2	(111)	15.0	(110)	30.7	(106)	33.9	(40)
Intermediate	33.0	(74)	49.3	(384)	48.7	(358)	49.3	(170)	38.1	(45)
Positive	15.2	(34)	31.8	(248)	30.3	(223)	12.8	(44)	11.0	(13)
Very positive	3.1	(7)	2.6	(20)	2.0	(15)	0.3	(1)	1.7	(2)
TOTAL	100	(224)	100	(779)	100	(735)	100	(345)	100	(118)

Muller has shown that the relationship between political support and aggressive political behavior conforms to a corner correlation pattern: At high and medium levels of political support, aggressive political behavior is very unlikely; only at low political support does aggressive political behavior show a tendency to occur, but even under this condition other responses such as behavioral withdrawal (complete inactivity) or conformative participation are also quite likely to occur. ⁴¹ In other words, low political support is a necessary but not sufficient condition of aggressive political behavior.

In our analysis of relationships between the political affect variables and aggressive political behavior, we shall ask these questions of the data. First, is the degree of fit to the "necessary but not sufficient" correlation pattern weakened or strengthened according to (1) whether the political affect variable is an incumbent or a system measure, and according to (2) the different combinations of incumbent and system affect specified in the theory about their joint effect on aggressive political behavior?

Space limitation precludes analysis in this report of the typology of political action developed by Muller. 42 This typology is based on combinations of aggressive and conventional behavior. People who manifest low, medium, or high levels of aggressive action are differentiated according to whether or not they also participate actively in conventional electoral politics. These distinctions are important with regard to a general theory of mass political behavior. But they are not germane to the purpose at hand which is to determine the kinds and combinations of kinds of political affect yielding the sharpest prediction of aggressive political behavior per se.

Measurement of Aggressive Political Behavior. The measure of the dependent variable was obtained by the following procedure. Respondents were given a set of cards. On each card a different kind of behavior was listed.⁴³ The interviewer explained that these behaviors were actions which one might take in order to

influence the government. Respondents were asked to indicate (1) whether or not they approved of each behavior; (2) how large a percentage, in their opinion, of citizens in the Federal Republic would approve of that behavior; (3) whether or not they themselves would engage in the behavior; and (4) whether or not they had engaged in the behavior. Before each question, the cards were shuffled.

The set of cards contained five behaviors which were not aggressive, according to the definition given above, and five behaviors which were aggressive. The aggressive actions, in English translation, were: (1) refusal to pay rents or taxes; (2) participation in a wildcat strike; (3) takeover of factories, offices, and other buildings; (4) participation in fights (battle with police, battle with other demonstrators); (5) participation in a group which wants to dislodge the government by violent means.44 Participation in each activity was classified as "negative" if respondents said they would not do it; "transitional" if the respondents said that they might or definitely would do it, but had not yet done it; "positive" if they said that they might or definitely would do it and had already done it.45 A composite Aggressive Behavior index was constructed by assigning a score of "0" to the "negative" category, a score of "1" to the "transitional" category, a score of "2" to the "positive" category, then summing scores across the five items.

There are two reasons for adopting this particular scoring procedure. First of all, persons who have done an action and would do so again ought to be differentiated from those who would not repeat the action. If one desires to predict future participation in aggressive behavior, then high scores on such an index should not be contaminated by persons who

⁴¹Edward N. Muller, "Behavioral Correlates of Political Support," American Political Science Review, 72 (June 1977), 454–67.

⁴² Ibid.

⁴³The phrasing for many of these items comes from a pretest questionnaire developed for the German segment of the Nine Nation Study which is being carried out in West Germany under the direction of Max Kaase and Hans Klingemann of the Zentrum für Umfragen, Methoden und Analysen, Mannheim.

⁴⁴The German test of these behaviors is: (1) "Sich weigern, Mieten, Raten oder Steuern zu bezahlen"; (2) "Beteiligung an einem wilden Streik"; (3) "Besetzung von Fabriken, Ämtern und anderen Gebäuden"; (4) "Beteiligung an Schlägereien (Kampf mit Polizisten, Kampf mit anderen Demostranten"; (5) "Beteiligung an einer Gruppe, die Regierung mit Gewalt sturzen will."

⁴⁵ Missing data responses were treated in the following manner: Among those respondents refusing to say whether or not they had done the behavior, those who said they would do it or might do it were classified as "transitional." Respondents who did not say whether they would do it or might do it were treated as having missing data. Individuals with no more than two missing data responses on the activity items were assigned the median score value for each activity in which a missing response occurred. Respondents with missing data on more than two activities were excluded from this analysis.

have no intention of repeating an action which they once did in the past. Second, and most importantly, it is theoretically desirable to combine intentions and behavior into a composite index. We conceptualize an aggressive political behavior continuum as ranging through four distinct zones. This conceptualization is supported by our data. (For further details see the analysis of components of the Aggressive Behavior index given in the Appendix). The lowest zone is characterized by inactivity and negative intention toward participation in aggressive action. As one moves higher on the continuum, the inactivity zone shades into positive intention for nonviolent aggressive action but little or no participation in such action. Moving still higher, the potential nonviolent civil disobedience zone shades into a zone characterized by actual participation in civil disobedience with progressively greater positive intention to engage in violent aggressive action. Finally, this civil disobedience/potential violence zone shades into the highest zone, characterized by actual participation in political violence.

Relationships Between Political Affect Variables and Aggressive Behavior. We know that the relationship between attitude and behavior in general is influenced by situational variables, and we can expect that the relationship between our measures of political affect and the Aggressive Behavior index will be subject to such contextual influences, captured in large part, perhaps, by the community-type "stratification" factor in the research design for this investigation. Prior to cutting the sample by community type, though, it is useful to look at the overall relationships between the political affect variables and the Aggressive Behavior index; then we can assess community type as a factor which may exert a specification effect on the relationships.

Given that we expect to find association between the political affect variables and the Aggressive Behavior index, what form do we expect the relationship to take? Since we are talking about an index of behavior which can be quite costly, especially at the higher levels, we do not expect the relationship to conform to a strict monotonic function according to which the cases would tend to fall on the diagonal of a contingency table. Rather, we expect that it will conform to a pattern of corner correlation where cases would tend to be located along two adjacent edges of a contingency table—the necessary but not sufficient

correlation pattern. ⁴⁶ A summary measure of correlation for ordinal variables which is sensitive to the kind of relationship we have in mind—a monotonic nondecreasing relationship such that as one variable increases another variable either stays the same or increases—is the Gamma coefficient.

The relationships between the political affect variables and the Aggressive Behavior index are given in Table 6. In the case of Political Support, the fit to a corner correlation pattern is quite strong in and of itself, and the strength of the relationship between support and the Aggressive Behavior index is far greater than that which obtains for any of the other measures of political affect. The dropoff in the rate of very low aggressive political behavior as one moves from the "very positive" zone of Political Support to the "very negative" zone is striking, with an especially precipitous decline from the "intermediate" zone-where the rate of inactivity on the Aggressive Behavior index is fully 75 percent-to the "very negative" zonewhere the rate of inactivity is only 16 percent or barely one-fifth as large. High Civil Disobedience and Political Violence are virtually nonexistent throughout the "very positive" to "intermediate" zones of Political Support. This is very similar to the relationship found in the pilot study for this project, where cruder measures of support and aggressive political behavior were used.⁴⁷ Low or negative support is very close to being a necessary condition for actual participation in actions of civil disobedience and political violence. And with this more comprehensive measure of system affect, when we differentiate between the "negative" and "very negative" zones, the latter begins to tend toward being a necessary and sufficient condition of actual participation in aggressive action, as evidenced by the fact that almost two-thirds (62.4 percent) of those in the "very negative" support zone also score in the High Civil Disobedience and Political Violence zones on the Aggressive Behavior index. Thus, the observed relationship between Political Support and the Aggressive Behavior index actually corresponds to an ordinal analogue of a positively accelerated power function, according to which, at the most negative zone of support, the likelihood of actual aggressive action is strong. This pattern of relationships, of course,

⁴⁶A good discussion of corner correlation appears in Johan Galtung, Theory and Methods of Social Research (Oslo: Universitets Forlaget, 1967), p. 223.

⁴⁷See Table 4 in Muller, "Behavioral Correlates of Political Support," p. 461.

has considerably more powerful causal implications than the corner correlation pattern. Of the four relationships between aggressive behavior and political affect, that with Political Support as the measure of affect is the only one which corresponds well to the positively accelerated power function pattern.

When Political Support is controlled for, the moderate zero-order correlations between the other political affect variables and the Aggressive Behavior index are simply washed out, as evidenced by the consistently low partial correlations. But when the other political affect variables are controlled for, the correlation between Political Support and the Aggressive Behavior index remains quite strong. Table 7 shows this.⁴⁸

⁴⁸When levels on a control variable are actually held constant, the Gamma coefficient is not reported

Political Trust does not correlate with the Aggressive Behavior index at any level of Political Support except the most negative. In this it also resembles Incumbent Evaluation and Policy Output Evaluation, thus giving further indication that Political Trust is especially sensitive to incumbent rather than system affect—or whatever it is that differentiates Incumbent Evaluation and Policy Output Evaluation from Political Support. However, the trust and policy variables do appear to have a modest specification effect on the relationship between Political Support and the Aggressive Behavior index, depressing the relationship

if there is practically zero covariation on the Aggressive Behavior index, i.e., under 10 cases scoring at levels other than Inactivity.

Table 6. Aggressive Behavior Index as a Function of Political Affect Variables

Aggressive	Very Positi	ve Po	sitive	Inter	mediate	Neg	ative	Very l	Vegative
Behavior Index	% (N) %	(N)	%	(N)	%	(N)	%	(N)
				Politica	l Suppor	t			
Violence	0 (0		(0)	0.5	(5)	1.6	(7)	7.9	(15)
High C.D.	2.0 (2		(11)	6.7	(65)	25.6	(113)	54.5	(103)
Low C.D.	8.0 (8	,	(50)	18.1	(176)	25.9	(114)	21.2	(40)
Inactive	90.0 (90	99.3	(508)	74.7	<u>(728)</u>	46.9	(207)	16.4	(31)
TOTALS	100 (100) 100	(569)	100	(974)	100	(441)	100	(189)
				Gam	ma = .66				
				Politic	cal Trust				
Violence	0 (0	0.2	(1)	0.5	(5)	1.2	(7)	8.6	(13)
High C.D.	2.1 (2		(27)	9.5	(101)	17.6	(105)	40.4	(61)
Low C.D.	9.4 (9		(59)	18.1	(192)	17.3	(103)	13.2	(20)
Inactive	88.5 (85	82.7	(416)	71.9	(761)	63.9	(381)	37.7	(57)
TOTALS	100 (96	100	(503)	100	(1059)	100	(596)	100	(151)
				Gam	ma = .38				
			Incumb	ent Perfe	ormance :	Evaluatio	n		
Violence	0 (0		(1)	0.5	(5)	2.8	(12)	6.2	(9)
High C.D.	6.8 (3	,	(47)	10.4	(112)	19.8	(86)	31.0	(45)
Low C.D.	2.3 (1		(83)	19.0	(204)	18.4	(80)	9.7	(14)
Inactive	90.9 (40	78.0	(465)	70.1	(754)	59.1	(257)	53.1	(77)
TOTALS	100 (44) 100	(596)	100	(1075)	100	(435)	100	(145)
				Gam	ma = .30				
•			Po	licy Out	put Evalu	ation			
Violence	0 (0		(1)	0.6	(7)	2.6	(12)	12.1	(7)
High C.D.	0 (0		(31)	11.0	(131)	22.0	(103)	51.7	(30)
Low C.D.	5.9 (4		(61)	20.1	(239)	17.1	(80)	12.1	(7)
Inactive	94.1 (64		(463)	68.3	(814)	58.4	(274)	24.1	(14)
TOTALS	100 (68	100	(556)	100	(1191)	100	(469)	100	(58)
				Gam	ma = .42				

when they are positive, facilitating it when they are negative.

Specification Effect of Ideology. A control variable of potentially crucial importance is ideology. ⁴⁹ As Miller has pointed out, "Ideology can be expected to condition the predisposition to participate in protest behaviors." ⁵⁰ Ideologies are behaviorally charged belief systems: They include prescriptions for and against various kinds of political action. In Germany today, though not of course in the

⁴⁹For a good discussion of ideology and aggressive political behavior, see Ted Gurr, Why Men Rebel (Princeton: Princeton University Press, 1970), ch. 7.
 ⁵⁰Miller, "Rejoinder," p. 995.

past, most civil disobedience and political violence has come from the Left. Most of those who designate themselves as leftist on the Ideological Commitment variable probably share what Miller calls a "social change" orientation, whereas most of those who designate themselves as rightist probably share a "social control" orientation.⁵¹ If aggressive political behavior is more acceptable to those holding a social change orientation than to those holding a social control orientation, then Ideological Commitment should have a specification effect such that the relationship between political affect and the Aggressive Behavior index will be

Table 7. Correlations (γ) Between Aggressive Behavior and Political Affect,
Controlling for Each Political Affect Variable

	Correlati	on Between Aggressive Behavior	Index and:
Controlling for: Political Support	Political Trust	Incumbent Evaluation	Policy Output Evaluation
Very positive Positive Intermediate Negative Very negative	$\begin{array}{ccc}07 & (98) \\ .03 & (562) \\09 & (946) \\15 & (425) \\ .48 & (186) \end{array}$ $Partial \gamma =06$.14 (97) .13 (543) 12 (909) .00 (413) .40 (186) Partial $\gamma =04$	33 (94) .22 (551) .02 (939) .18 (428) .44 (186) Partial γ = .10
Political Trust	Political Support	Incumbent Evaluation	Policy Output Evaluation
Very positive Positive Intermediate Negative Very negative	.22 (87) .54 (455) .60 (969) .63 (558) .89 (148) Partial $\gamma = .61$.02 (89) .00 (461) .08 (977) .17 (557) .27 (147) Partial $\gamma = .10$	09 (88) .38 (470) .22 (990) .29 (575) .46 (148) Partial γ = .26
Incumbent Evaluation	Political Support	Political Trust	Policy Output Evaluation
Very positive Positive Intermediate Negative Very negative	$ \begin{array}{rrrr} & - & (42) \\ .57 & (563) \\ .61 & (1000) \\ .72 & (407) \\ .79 & (136) \\ Partial \gamma = .62 $	$\begin{array}{ccc} - & (43) \\ .16 & (574) \\ .31 & (1051) \\ .33 & (422) \\ .64 & (141) \\ Partial \gamma = .29 \end{array}$	$ \begin{array}{ccc} & - & (42) \\ .28 & (572) \\ .30 & (1027) \\ .41 & (413) \\ .52 & (141) \end{array} $ Partial $\gamma = .32$
Policy Output Evaluation	Political Support	Political Trust	Incumbent Evaluation
Very positive Positive Intermediate Negative Very negative	$ \begin{array}{rrrr} & (49) \\ .45 & (513) \\ .57 & (1128) \\ .72 & (452) \\ .85 & (56) \end{array} $ Partial $\gamma = .58$	- (63) .16 (535) .23 (1157) .31 (460) .58 (54) Partial $\gamma = .23$	$ \begin{array}{cccc} & - & (56) \\ & .08 & (504) \\ & .08 & (1129) \\ & .18 & (452) \\ & .30 & (54) \\ & Partial \gamma = .09 \end{array} $

⁵¹ Miller, "Political Issues and Trust," p. 962.

strongest among persons who place themselves on the Left of the ideological continuum, becoming progressively weaker among those who place themselves in the center and on the Right.

We have seen that there is no relationship between the Aggressive Behavior index and Political Trust, Incumbent Evaluation, or Policy Output Evaluation when Political Support is controlled for, except when support is very negative. But if ideology has a specification effect, then a relationship should emerge among the Left when ideology is introduced as a second control.⁵² However, as the data given in Table 8 show, this does not happen. To be sure, when support is in the negative zone, trust and policy correlate modestly with aggressive behavior only among leftists; but even among persons on the Left, at the other levels of support there is still virtually no relationship between aggressive behavior and either trust or policy. The only other correlations of even modest magnitude occur between policy and aggressive behavior under the condition of positive support and rightist ideology and between trust and aggressive behavior under the condition of intermediate support and rightist ideology. Both are something of a puzzle because the former does not conform to the specification hypothesis, while the latter is in a theoretically implausible direction.

When we turn to the Political Support variable, the expected specification effect due to ideology does appear. At all levels of trust and policy, the strong correlation between support and aggressive behavior holds up on the Left. It is considerably depressed among centrists and rightists, reaching moderate magnitudes only when the incumbent affect variables are intermediate or negative. Interestingly, when trust is negative, a phenomenon appears which is reminiscent of what probably was the case in Germany during the 1920s and early 1930s: The correlation between Political Support and the Aggressive Behavior index is weakest among centrists, strong among leftists, and close to the range which we call strong among rightists.

Specification Effect of Community. Ideology is an important specification variable. But an even more powerful specification effect may come from a contextual factor: the type of community in which one lives and works. What are the most relevant distinctions with respect to community? The simple dichotomy, university versus nonuniversity, probably overshadows all others. As Huntington observed not too long ago, in most modernizing countries:

The city is the center of opposition within the country; the middle class is the focus of opposition within the city; the intelligentsia is the most active oppositional group within the middle class; and the students are the most coherent and effective revolutionaries within the intelligentsia. . . . It is here, in the university, that the most consistent, extreme, intransigent opposition to the government exists. ⁵³

And as we know from recent history, Huntington's description aptly characterizes the industrialized nations of the world as well.

Why the university? Because it is likely that the university reflects high or facilitative values on three variables which condition the relationship between attitudes conducive to aggressive political behavior and actual aggressive political behavior. These variables are (1) availability, (2) costs, 54 (3) community norms. At the university, students and faculty are not tied to a daily routine. They have far greater freedom of time and movement than do most people who are not in a university context. Students have especially high availability simply because they are young and likely to be unmarried. The costs of aggressive political behavior are, relatively speaking, lower in the context of the university. Authorities are likely to be more lenient-faculty in positions of administrative authority may sympathize with students and other faculty who take part in aggressive political action. And, of course, there is less to lose: the student is not in mid-career, his career lies ahead; the faculty member, if untenured, stands to lose his job, but this may depend largely on whether or not his behavior is viewed as deviant by his immediate departmental colleagues, or, in the case of the Lehrstuhl system, by a few senior professors. This last point leads into what may be the most important of the facilitating conditions reflected in the university-nonuniversity distinction: community norms.

Consider Table 9: Community context has a very powerful inhibitory-facilitative effect on

⁵²In the SRC data analyzed by Miller, there was no zero-order relationship between trust and the approval of protest items, but when ideology was controlled for, a modest relationship occurred between these variables among those with a leftist ideological orientation. See Miller, "Rejoinder," p. 996.

⁵³Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), pp. 290-91.

⁵⁴For an especially good discussion of costs, see Anthony Oberschall, Social Conflict and Social Movements (Englewood Cliffs, N.J.: Prentice-Hall, 1973), ch. 5.

the Aggressive Behavior index. The likelihood of a person scoring at any level other than inactive on the Aggressive Behavior index is practically zero in the urban and agrarian milieus. In the university milieu, there is a strong likelihood that a person will score other than inactive. (The majority of those who score at other than inactive are students in their twenties working on the German equivalent of an American M.A. degree; the minority are persons fully or partly employed at the university in some capacity, ranging from a graduate

assistant working on the Ph.D. to full professor. Almost everyone scoring at other than inactive is under 40 years of age.) Although persons from the university milieu are unlikely to score at the Political Violence level on the Aggressive Behavior index (though almost all of those who do score at this level are from the university), civil disobedience is certainly not deviant behavior in this context: 57.5 percent of those in the university sample score at the civil disobedience levels.

Table 8. Correlations (γ) Between Aggressive Behavior Index and Political Affect Variables,
Controlling for Ideological Commitment and Other Political Affect Variables

Contr	olling for:	Correl	ation Between Aggre	essive Behavior Ind	ex and:
Ideological		Politic	cal Trust	Policy Evalu	Output ation
Commitment	Political Support	γ	(N)	γ	(N)
Left ^a	Positive ^c Intermediate	.10 .01	(206) (309)	.08 .14	(203) (314)
	Negative ^d	.34	(412)	.14 .43	(417)
Center	Positive ^c	.02	(255)	.17	(248)
	Intermediate Negative ^d	.08 03	(344) (90)	09 .15	(336) (87)
Right ^b	Positive ^c	12	(142)	.38	(139)
	Intermediate Negative ^d	28	(208) (64)	.10	(204) (66)
	ivegative-	_		-	(00)
Ideological	Political Trust		******	al Support	
Commitment			<u>γ</u>	(N)	
Left ^a	Positive ^c		.63	(227)	
	Intermediate Negative ^d		.67 .59	(381) (319)	
Center	Positive ^c		01	(200)	
	Intermediate		.26	(313)	
	Negative ^d	•	.35	(176)	
Right ^b	Positive ^c			(71)	
	Intermediate		22	(185)	
	Negative ^d		.47	(158)	
Ideological	Policy Output			al Support	
Commitment	Evaluation		γ	(N)	
Left ^a	Positive ^c		.51	(216)	
	Intermediate		.64	(468)	
	Negatived		.65	(250)	
Center	Positive ^c		.23	(189)	
	Intermediate Negative ^d		.26 .31	(376)	
	_		.31	(106)	
Right ^b	Positive ^c			(85)	
	Intermediate Negative ^d		03	(217) (107)	

^aIncludes "Extreme Left."

bIncludes "Extreme Right."

^cIncludes "Very Positive."

dIncludes "Very Negative."

Or take Ideological Commitment and Political Support, the two most powerful attitudinal predictors of aggressive behavior identified in the analysis to this point: Nearly two-thirds of the university sample is leftist; by contrast, only about one-third of those in the urban milieu have a leftist ideological orientation and only about one-quarter of those in the agrarian milieu designate themselves as leftist. Especially striking differences on the Political Support variable are registered when one controls for community type: Nearly one-sixth of those in the university milieu express very negative support and almost one-third score at the "negative" zone; by contrast, outside of the university milieu, very few register in the "very negative" zone on Political Support, while only

9.1 percent of those in the urban milieu and only 14.8 percent of those in the agrarian milieu register in the "negative" zone.

In sum, availability (by definition), costs (by inference), and community norms (by the evidence in Table 9) are facilitative in the university milieu, inhibitory in the nonuniversity milieu. Therefore, we hypothesize that the relationships among ideology, the political affect variables, and the Aggressive Behavior index will either be enhanced or else little changed among persons in the university milieu and depressed among persons in the nonuniversity milieu.

The first proposition in the theory about behavioral consequences of the incumbentsystem distinction is a statement about the

Table 9. Distributions on Political Affect and Behavior Variables by Community Type

			Commu	nity Type		
Affect and	Ru	ral	Uı	rban	Uni	versity
Behavior Variables	%	(N)	%	(N)	%	(N)
Political Support						,
Very negative	2.4	(11)	1.8	(15)	15.6	(167)
Negative	14.8	(69)	9.1	(74)	30.1	(322)
Intermediate	46.9	(219)	44.1	(360)	40.3	(432)
Positive	28.9	(135)	38.6	(315)	12.7	(136)
Very positive	7.1	(33)	6.5	(53)	1.3	(14)
TOTAL	100.0	(467)	100.0	(817)	100.0	(1071)
Political Trust						
Very negative	5.1	(28)	2.5	(23)	10.0	(107)
Negative	24.0	(132)	20.0	(184)	29.0	(310)
Intermediate	40.2	(221)	43.5	(401)	46.5	(498)
Positive	24.0	(132)	27.4	(252)	14.0	(150)
Very positive	6.7	(37)	6.6	(61)	0.5	(5)
TOTAL	100.0	(550)	100.0	(921)	100.0	(1070)
Policy Output Evaluation		(* • • • • • • • • • • • • • • • • • • •		()		(
Very negative	0.8	(4)	1.1	(10)	4.4	(47)
Negative	18.2	(86)	14.6	(131)	24.9	(269)
Intermediate	50.3	(238)	45.0	(405)	56.0	(604)
Positive	26.2	(124)	34.3	(309)	13.5	(146)
Very positive	4.4	(21)	5.0	(45)	1.2	(13)
TOTAL	100.0	(473)	100.0	(900)	100.0	(1079)
Ideological Commitment		(()		()
Extreme right	15.2	(75)	4.9	(38)	1.2	(13)
Right	23.4	(116)	17.5	(136)	10.2	(107)
Center	33.7	(167)	42.9	(334)	27.8	(290)
Left	21.6	(107)	29.0	(226)	45.7	(477)
Extreme left	6.1	(30)	5.7	(44)	15.0	(157)
TOTAL	100.0	(495)	100.0	(778)	100.0	(1044)
Aggressive Behavior Index						
Violence		(0)	-	(0)	2.6	(27)
High C.D.	1.3	(7)	2.0	(18)	26.6	(277)
Low C.D.	4.6	(25)	5.3	(49)	30.9	(322)
Inactive	94.1	(507)	92.7	(853)	39.9	(415)
TOTAL	100.0	(539)	100.0	(920)	100.0	(1041)

relationship between incumbent affect and aggressive political behavior under the condition of positive system affect. Given the form of the relationship expected to obtain generally between political affect and aggressive behavior, the Gamma correlation will be used to test the propositions. From Tables 10 and 11 we see that the Gamma correlations are practically zero, as predicted from P₁, except for the relationship between trust and aggressive behavior in the nonuniversity milieu—but here, the correlation, while barely moderate, is in a theoretically implausible direction.

The second proposition, which predicted a slight, positive correlation between incumbent affect and aggressive behavior under the condition of negative system affect, is not confirmed due to the specification effect of community context. In the nonuniversity milieu, the correlation is either lower than predicted (practically zero) or else its sign (in the case of trust) is in the wrong direction. By contrast, in the university milieu the correlation is moderate—a greater magnitude than predicted.

Tables 12 and 13 show data relevant to the test of the third and fourth propositions. The third proposition predicts a moderate correlation between system affect and aggressive behavior, under the condition of positive incumbent affect. This is disconfirmed in the nonuniversity milieu, where the correlation is only slight. It is confirmed in the university milieu when Policy Output Evaluation is taken as the indicator of incumbent affect. But it is disconfirmed in the university milieu with Political Trust as the indicator of incumbent affect because, in this case, a strong correlation is observed between aggressive behavior and support when trust is positive.

The fourth proposition predicts a strong correlation between system affect and aggressive behavior, under the condition of negative incumbent affect. When Policy Output Evaluation is taken as the indicator of incumbent affect, the fourth proposition is strongly confirmed in the university milieu, and even in the nonuniversity milieu the Gamma correlation of .49 verges on the lower boundary of the magnitude predicted.

When Political Support is controlled for, the measures especially sensitive to affect for an incumbent administration show, at best, only a modest correlation with the Aggressive Behavior index. Generally, aggressive behavior is unrelated to incumbent affect after Political Support is taken into account. But Policy Output Evaluation does appear to have a specification effect on the relationship between

Political Support and the Aggressive Behavior index, inhibiting the relationship when Policy Output Evaluation is positive, facilitating it when Policy Output Evaluation is negative, although this occurs more markedly in the nonuniversity than in the university milieu.

What happens when policy and trust are both taken into account? Will there be a specification effect due to incumbent affect? And will it hold up independently of ideology? These questions are addressed by the data shown in Table 14. Policy Output Evaluation and Political Trust are combined into three conditions: High, defined by positive scores on both policy and trust; Medium, defined by a positive score on one incumbent affect variable, a nonpositive score on the other; Low, defined by nonpositive scores on both policy and trust.

When evaluation of incumbents is consistently positive or high-they are trusted and their policy performance is satisfactory-the range on Political Support is confined almost completely to "very positive" through "intermediate." Even so, a specification effect due to ideology is apparent. The non-Left is virtually certain to score at the inactive level on the Aggressive Behavior index, regardless of whether their support is at a positive level or at an intermediate level. Among the Left, however, the rate of civil disobedience is higher when support is intermediate than when it is positive, and the relationship is stronger in the university than in the nonuniversity milieu. Even though the number of cases under this condition in the university milieu is small, the relationship is nevertheless sufficiently sharp to warrant confidence in its reliability.

At the low condition of incumbent affect, where incumbents are not trusted and their policy performance is not satisfactory, the specification effect of ideology is quite pronounced. On the Left, as Political Support decreases from "positive" to "very negative," the percentage of persons scoring Inactive on the Aggressive Behavior index consistently declines. The magnitude of the decline is somewhat greater in the university milieu (62.4 percentage points), and this difference is reflected in the size of the Gamma correlations (.55 and .49, respectively); the correlation, however, is strong or very close to strong in both contexts. Community context does not affect the strength of the correlation. But it does have an effect on the rates of inactivity at given levels of support. In the nonuniversity milieu, the inactivity rate on the Aggressive Behavior index begins at 97.6, when Political Support is "positive," then drops to 50, when

Table 10. Aggressive Behavior Index as a Function of Political Trust, Controlling for Political Support and Community Type

	WATER TO THE TOTAL PROPERTY OF THE TOTAL PRO						Political Trust	1 Trainst					
, de la company	Dollston	Aggressive	Very Positive	sitive	Positive	ive	Intermediate	ediate	Negative	tive	Very Negative	egative	
Type	Support	Index	%	3	%	3	%	રિ	%	3	%	3	
Rural and Urban	Positive ^a	Violence High C.D.	1.6	(0)	0.5	(0)	1.6	(6)	10	66	1 !	66	γ=29
		Low C.D. Inactive	7.8	(88)	5.3 94.2	(11) (195)	2.7 95.7	(180 (180)	2.0 98.0	(48) (48)	1 1	<u> </u>	(N = 511)
	Intermediate	Violence High C.D.	10	66	3.2	(C)	- 1 .	○ ○ 94	1.4	66	iI	6 6	$\gamma =17$
		Low C.D. Inactive	16.7 83.3	(3) (15)	6.4 90.4	(<u>8</u>)	6.4 92.2	(18)	5.8 92.8	(129)	100.0	(G)	(N = 549)
	Negative ^b	Violence High C.D.	1 1	66	11.1	90	7.5	ଚଳ ୦୦	3.5	⊝ ⊝ •	0.8	<u>6</u>	$\gamma =17$
		Low C.D. Inactive		<u></u>	11.1	33	7.5 85.0	(34)	5.8	(78)	4.0 88.0	(22)	(N = 160)
University	Positive ^a	Violence High C.D.	1 1	66	5.3	<u>6</u> 6	5.6	≎≎ 94	6.7	9 6 7		66	$\gamma = .10$
	,	Low C.D. Inactive	1 1	(3)	19.3 75.4	(11) (43)	27.8 66.7	(20) (48)	20.0 73.3	(11)	1 1	(E)	(N = 149)
	Intermediate	Violence High C.D.	1 1	(1)	1.4 14.9	££	0.9	(27)	2.4 16.5	(14)	12.5	(0)	$\gamma =04$
		Low C.D. Inactive	1 1	<u></u>	32.4 51.4	(24) (38)	37.1 50.2	(85) (115)	.29.4 51.8	(52) (44)	87.5	33	(N = 397)
	Negative ^b	Violence High C.D.	* "	<u></u>	42.9	<u></u>	1.9 35.4	(3)	2.7	(83)	13.7 61.1	(13) (58)	γ = .36
		Low C.D. Inactive	1 1	66	35.7 21.4	(3)	36.1 26.6	(57) (42)	31.5 20.7	(58)	5.3	(19)	(N = 451)

alncludes "Very Positive."
bIncludes "Very Negative."

Table 11. Aggressive Behavior Index as a Function of Policy Output Evaluation, Controlling for Political Support and Community Type

			00	50.	(N = 495)	71 " ~		(N = 538)	100	1 04	N = 160	COOT N	5	7 = .02	V = 140)	(C+T = NT)	5	5	V = 401)	(TO+ - N)		14.	$O_1 = A54$	(TCT I N
				•	<u>ت</u>	7	•	U	3	_	0	,	•	_	Ç	ٺ	•	•	•	ن	•	_	9	٠
	Very Negative	ર્ટ	(0)	6	66	6)	6	ଚିତ୍ର	(0)	6	<u></u>	(4)	60)	6	6	6	6)	6		(1)	(7)	30	9	6
	Very N	%	1	ł	l I	ı	ı	100.0	1	ŧ	ł	I	1	ŧ	ļ	ţ	j	ı	i	ł	16.3	8.69	14.0	1
	tive	દ્વ	(0)	6	(41)	6	6	(100)	6	⊛ •	((45)	6	6	(4	(12)	(1)	(15)	(14)	(36)	(11)	(84)	(51)	(20)
	Negative	%	1	ì	4.7 95.3	ļ	1 6	2.9 97.1	į	5.8	7.7	86.5	i	i	25.0	75.0	1.5	22.7	21.2	54.5	6.4	48.8	29.7	15.1
Evaluatio	diate	(S)	(0)	⊛)	(2) (2) (3) (3) (3) (3) (4) (4) (4) (4) (4) (4) (4) (4) (4) (4	60	ල ට	(254)	6	? `	(E)	(65)	6	6	(21)	(61)	(3	(31)	(101)	(129)	.	(08)	(20	(48)
Policy Output Evaluation	Intermediate	%	1	1.4	5.1 93.5	ı	2.1	8.5 89.4	ţ	2.9	4.3	92.9	f	8.9	23.9	69.3	1.1	11.7	38.3	48.9	1.9	38.5	36.5	23.1
Poli	ve	(F)	(0)	(2	8 2 2 2 3	(0)	€ ((118)	6	7	- (1)	(26)	6	(7	6	(30)	(1)	6	(22)	(32)	6	(10)	⊛ ○	(10)
	Positive	%	1	6.0	4.6 94.5	1	2.3	6.9 90.8	1	6.9	3.4	89.7	ŧ	4.9	22.0	73.2	1.5	13.4	32.8	52.2	ł	35.7	28.6	35.7
	sitive	3	(0)	(O)	<u> </u>	(0)	6	<u>9</u>	(0)	<u>(</u>	(Z)	(3)	60)	6	(1)	(3)	6)	6	6)	(2)	6)	6)	6	(3)
	Very Positive	%	1990	ŧ	100.0	i	í	100.0	ŧ	i	40.0	0.09	i	ı	ı	ı	i	ı	ı	į	i	ì	ŀ	ł
	Aggressive Rehavior	Index	Violence	High C.D.	Low C.D. Inactive	High C.D.	High C.D.	Low C.D. Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Inactive
The second secon	Political	Support	Positivea			Intermediate			Negativeb				Positive ^a			•	Intermediate				Negative ^b			
	Community	Type	Rural and Urban									-	University											

^aIncludes "Very Positive." ^bIncludes "Very Negative."

Table 12. Aggressive Behavior Index as a Function of Political Support Controlling for Political Trust and Community Type

	AND THE PROPERTY OF THE PROPER	and the second s				The state of the s	Political Support	Support					
Community	Polifical	Aggressive Rehavior	Very Positive	sitive	Positive	tive	Intern	Intermediate	Negative	ıtive	Very Negative	egative	•
Type	Trust	Index	%	2	%	2	%	3	%	3	%	3	
Rural and Urban Positivea	Positivea	Violence		60	ı	6	1	6	1	60)	I	60)	- - - - -
	•	High C.D.	3.1	(7)	1 (6	2.7	က် (11.1	((1	6	27.
		Low C.D. Inactive	89.1	(57)	5.3 94.7	(11)	89.3 89.3	(100) (100)	77.8	30	1 1	<u></u>	(N = 392)
	Intermediate	Violence	ı	6)	ı	6	ı	6	1	6)	i	6)	23
		High C.D.	ı	6	1.7	€ •	4:1	4	8.3	<u>(E</u>	i	6	10-1
		Low C.D. Inactive	100.0	<u></u>	2.9 95.3	(164)	6.4 92.2	(268) (260)	88.9 88.9	(33)	1 1	7 ()	(N - 510)
	Negativeb	Violence	4	6)	{	6	í	6		6)	*	6)	1
)	High C.D.	ļ	6	ı	6	1.3	(S	2.2	(2)	15.0	(3)	/c:=/
		Low C.D.	ł	-	1	6	5.2	@ 	5.5	ક	5.0	<u> </u>	(N = 318)
		Inactive	Į	(3)	100.0	(48)	93.5	(145)	92.3	(84	80.0	(16)	(OV)
University	Positive ^a	Violence	l	6)	ļ	6	1.3	(1)	ŧ	6)	i	6)	1
•		High C.D.	ı	6)	5.7	€ Э	16.0	(12)	41.7	(2)	1	(1)	+c: - 1.
		Low C.D.	12.5	<u> </u>	20.8	(E)	32.0	(54)	33.3	.	1	<u> </u>	(N = 150)
	-	Inactive	87.5	(3	73.6	(33)	20.1	(38)	22.0	(3)	1	6	(act in
	Intermediate	Violence	ı	6)		6	6.0	(2	2.1	(3	ì	6)	v = 47
		High C.D.	1	<u></u>	6.1	.	8	(27)	34.0	(49)	50.0	<u>()</u>	120-1
		Low C.D.	16.7	<u> </u>	28.8	(19)	37.1	(82)	37.5	(54) (54)	21.4	€ ((N = 459)
	•	macnve	65.3	(6)	7.00	(43)	30.7	(611)	4 .07	(96)	0.02	(+)	
	Negativeb	Violence		6	1	6	2.5	(2)	2.3	(3)	10.3	(15)	~ = 64
		High C.D.	i	6	6.3	(1)	16.1	(15)	37.6	(20)	62.3	(91)	·
		Low C.D.	ı	6	18.8	(3)	26.9	(25)	34.6	(46)	21.2	(31)	(N = 388)
		Inactive	ļ	6	75.0	(12)	24.8	(51)	25.6	(34)	- 6.2	6)	·

^aIncludes "Very Positive."

^bIncludes "Very Negative."

Table 13. Aggressive Behavior Index as a Function of Political Support Controlling for Policy Output Evaluation and Community Type

			ر ا ا	CT: _ /	(N = 417)	,	γ = .13	(N = 568)	()	749	(N = 200)	(007 - 17)		y = .40	OI = 145	(017 - 17)	1	7c. = Y	(0.55 - 140)	(noc - N)	7	co 1	(00C - IV)	(267 - 11)
	Very Negative	(N)	(0)	6	6 6 6 6	6)	(1)	6 8	(0)	(2)	(3)	œ)	6)	(3)	(1)	(1)	(2)	(23)	(10)	(7)	(13)	(74)	(24)	<u>4</u>
	Very 1	%	ł	i	1 1	ı	11.1	08.9	1	15.4	23.1	61.5	I	60.0	20.0	20.0	4.8	54.8	23.8	16.7	11.3	64.3	20.9	3.5
	Negative	E	(0)	(3)	(33) (33)	6	(1)	(3)	6)	(1)	(1)	(41)	6)	(2)	(7)	(15)	(3)	(57)	(99)	(41)	(3)	(40)	(33)	(22)
	Neg	%	1	6.3	84.48 4.4.4	i	1.6	4.9 93.4	1	2.3	2.3	95.3	i	26.9	56.9	46.2	1.2	34.3	39.8	24.7	5.0	40.0	33.0	22.0
Political Support	Intermediate	જ્ઞ	(0)	3	C134)	6	<u>(</u>	(254) (254)	6	6	(3)	(105)	(1)	<u>6</u>	(22)	(31)	(3)	(31)	(101)	(129)	(1)	(15)	(15)	(37)
Political	Interr	%	i	2.1	-6.2 91.8	i	2.1	8.5 89.4	f	i	7.8	97.2	1.4	13.0	31.9	23.6	1.1	11.7	38.3	48.9	1.5	22.1	22.1	54.4
	Positive	જ્ઞ	(0)	6	(175)	6		(10) (179)	6	6	67	(42)	6	6	6	(33)	6	6	(20)	(23)	6	=	(E)	(12)
	Pos	%	1	ı	2.8 97.2	1	1.6	5.2 93.2	١,	I	4.5	95.5	ı	5.0	22.5	72.5	ļ	7.6	25.3	67.1	1	6.3	18.8	75.0
	Very Positive	જ	(0)	(3)		6)	6	<u>3</u>	6)	6	6	=======================================	6)	6	$\widehat{\mathbb{C}}$	(4	6)	9	$\widehat{\mathbb{C}}$	(8)	6)	6)	6	6
	Very I	%	١	3.5	8.8	ł	I	4.5 95.5	i	ı	1	I	ı	1	20.0	80.0	1	I	11.1	88.9	i	1	ļ	1
	Aggressive Rehavior	Index	Violence	High C.D.	Low C.D. Inactive	Violence	High C.D.	Low C.D. Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Inactive
	Policy Output	Evaluation	Positive ^a		٠	Intermediate			Negativeb	ı			Positive ^a				Intermediate				Negativeb			
United States of Control of Contr	Community	Type	Rural and Urban										University					٠						

a Includes "Very Positive."

^bIncludes "Very Negative."

Table 14. Aggressive Behavior Index as a Function of Political Support, Controlling for Policy Output Evaluation, Political Trust, Ideological Commitment, and Community Type

			1.7	/ T' = /	(80 = N	(00 - 17)	1 2	000-1	(N = 109)	;	γ = .49	(N = 150)	***		(N = 92)		75	oc 1	(N = 218)		000	Y =39	(37.5 - IA)	(0/c = N)	(continued on next page)
	egative	2	(0)	6	6	6	6)	6	<u>e</u>	6	<u>(</u> 2	(2)	6	6	6	9	6)	6	6	(T)	6	6	6	(10)	inued on
	Very Negative	%	ı	ı	1	ı	1	ı	1 1	1	20.0	30.0 50.0	١	ı	١	1	i	I	ł	ļ	t	1	1	100.0	(conti
	tive	ર	6)	. 1	6	Ξ	6)	$\widehat{\mathbb{C}}$	(2) (3) (3) (4) (4) (4) (4) (4) (4) (4) (4) (4) (4	6	(S)		6	6	6	3	6)	6 •	33	(13)	6	6	9	(55)	
	Negative	%	ı	1	i	ı	1	6.3	12.5 81.3	1	9.5	4.8 85.7	ı	ı	i	i	1	i	13.3	80.7	i	ı	1	100.0	
Support	ediate	2	(0)	6	(4)	(14)	6)	(3)	(1)	6	ĵ.	(£0) (£6)	6	6		(77)	6)	(3)	6	S	6	(2)	(12)	(202)	
Political Support	Intermediate	%	ł	1	22.2	77.8	ł	7.0	2.3 90.7	1	1.3	13.3 85.3	ł	ı	4.3	73.1	i	3.5	7.0	67.5	ł	6.0	5.5	93.6	
	tive	2	(0)	6	(3)	(4 1)	6)	6	(3) (3)	6	6	.E	6	6	(6)	(40)	6)	6	(E)	(98)	6	(3)	.	(78)	
	Positive	%	I	ŧ	4.7	95.3	1	ı	7.1 92.9	ı	ł	2.4 97.6	ı	ı	5.9	74.1	ı	ı	3.0	D. / Y	ł	3.5	4.7	91.8	<u>.</u>
	ositive	3	(0)	(1)	(3)	(51)	6)	6	<u>9</u> 6	6	6	(E)	6	Ξ	(22)	(cr)	(0)	6	6	(cr)	6)	6)	6)	(7)	
	Very Positive	%	ı	4.0	12.0	84.0	ŧ	ı	100.0	1	į	1 1	i	2.6	11.1	00.0	į	l	1 6	100.0	i	•	1	100.0	
	Aggressive Rehavior	Index	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D. Inactive	Violence	High C.D.	Low C.D. Inactive	Violence	High C.D.	Low C.D.	паспле	Violence	High C.D.	Low C.D.	пасцуе	Violence	High C.D.	Low C.D.	Inactive	
ng for:	Policy and	Trust	Highc				Mediumd			Lowe			High)			Mediumd				$\mathbf{Low}^{\mathbf{e}}$				-
Controlling for:	Ideological	Type Commitment	Left ^a										Center and	Rightb)			·							
	Community	Type	Rural and	Urban																					

Table 14. (continued)

A- (1949)			02 - 70	1-12	(C) = (C)	(77 = N)	5	67° = 1	(N = 105)		1 2		(11)	(I + + + N)	i i		(N = 10)	(or - N)		7 = .10	(00)	(00 = N)	100	011	(N = 266)
	Very Negative	(S)	(0)	6	6)	6	6)	(4)	(5)		(15)	(06)	(21)	(e)	6)	6	9	6	6	6	6	6)	6)	6	≎ 9€
	Very N	%	ļ	ı	ı	ı	i	57.1	28.6	74.3	10.9	65.2	19.6	4.3	ı	i	ı	I	1	į		I	l	i	33.3 66.7
	ative	(S)	(0)	(1)	6)	(3)	6)	(3)	<u>@</u>		9)	(81)	(75)	(27)	6)	6	6	6	6)	(7)	<u>(T</u>)	(4	6)	(8)	(16) (32)
	Negative	%	j	{	ı	ı	ŧ	25.0	40.0	23.0	3.2	42.9	39.7	14.3	1	ı	ı	1	ì	28.6	14.3	57.1	ŀ	14.3	28.6 57.1
Political Support	Intermediate	3	(0)	.	. 4	(3)	(3)	8)	(S)	(07)	(3)	(25)	(45)	(32)	6)	6	6	9)	6	(2)	(10)	(32)	6)	<u>6</u>	4 4 4 8 8
Political	Intern	%	ı	40.0	40.0	20.0	3.6	14.3	46.4	23.7	2.9	23.8	42.9	30.5	ı	I	ı	100.0	1	10.6	21.3	68.1	ŀ	4.1	29.7 66.2
	Positive	3	6)	6	(1)	(2)	6)	4	(2)	(61)	6	.	(3)	<u> </u>	6)	6	(1)	(10)	6	<u>(T</u>	(10)	(18)	6)	(7)	(13) (36)
Annual of the same	Posi	%	l	ì	12.5	87.5	ı	18.2	22.7	1.60	ł	11.1	22.2	2.99	1	i	9.1	6.06	ı	3.4	34.5	62.1	ì	3.9	25.5 70.6
	sitive	3	(0)	0)	6	(3)	6)	(0)	66	6	6)	6	6)	(O)	6)	6)	(1)	(O)	6)	6	6	(2)	60)	6	⊖ ⊖
	Very Positive	%	1	ı	į	ı	l	1	1	I	1	*	ı	i	I		1	ì	i de	i	ł	100.0	ı	ı	20.0 80.0
	Aggressive Rehavior	Index	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Illacuve	Violence	High C.D.	Low C.D,	Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D.	Inactive	Violence	High C.D.	Low C.D. Inactive
ng for:	Policy and	Trust	High				Mediumd				Lowe				Highc				Mediumd				Lowe		
Controlling for:	[deological	Type Commitment	Lefta												Center and	Right ^D									
	Community	Type	University																						

ancludes "Extreme Left."

bincludes "Extreme Right."

cincludes "Very Positive" and "Positive" on both variables.

dincludes "Very Positive" and "Positive" on one variable, "Intermediate," "Negative," and "Very Negative" on the other.

eincludes "Intermediate," "Negative," and "Very Negative" on both variables.

Political Support is at its most negative level. By contrast, the inactivity rate among those in the university milieu *begins* at 66.7 percent for those with positive support, then drops all the way to under 5 percent, at the most negative position on the Political Support variable.

Turning to the non-Left we see that the direction of the relationship between Political Support and the Aggressive Behavior Index actually is reversed among persons in the nonuniversity milieu due to the fact that everyone on the non-Left who is disaffected from the system of government also is inactive on the Aggressive Behavior index. This phenomenon is quite unexpected from a theoretical standpoint and calls for a separate, "deviant case" analysis. The non-Left in the university milieu is not so deviant. Here the relationship between Political Support and the Aggressive Behavior index, while by no means as strong as that which holds among leftists, goes in the predicted direction at least, except for a small, deviant upturn when Political Support is at the most negative level.

Summary. We can summarize the relationship between the Political affect variables and the Aggressive Behavior index, taking into account ideology and community context, as follows:

- There is a specification effect due to incumbent affect, as predicted by the theory of the behavioral consequences of the incumbent-system distinction, but this effect is relatively minor compared to that which results from ideology.
 - a) Among the Left, regardless of the level of policy and trust, moderate-to-strong correlations are observed except in the instance of High policy and trust levels in the nonuniversity milieu.
 - b) Among the non-Left, regardless of the level of policy and trust, either (1) almost all persons are in the Inactive zone on the Aggressive Behavior index (there is too little variation on aggressive behavior to even compute a correlation) or (2) the observed correlation between Political Support and the Aggressive Behavior index is very weak or else has a theoretically implausible sign. The one exception is the instance of Medium policy and trust levels in the nonuniversity milieu, where the Gamma correlation between support and aggressive behavior reaches a strong magnitude.
- 2. Although ideology appears to have the major specification effect on the relation-

- ship between Political Support and the Aggressive Behavior index—inhibiting it (or producing a change in sign) among centrists and rightists, facilitating it among leftists—a slightly stronger strength of relationship is observed among leftists in a facilitative community context than in an inhibitory community context. More importantly, community context affects the likelihood of participation in aggressive action, given varying degrees of political support among those with a leftist ideological commitment.
- a) When support is in the positive range, leftists in a nonuniversity context are practically certain to score in the Inactivity zone on the Aggressive Behavior index. This is much less true of leftists who reside in a university milieu.
- b) When support is in the negative range, leftists in a university context are extremely likely to score at other than Inactive on the Aggressive Behavior index. This is much less true of leftists who reside in a nonuniversity milieu.
- 3. These generalizations apply to the Aggressive Behavior index as ranging from Inactivity (no participation in aggressive behavior and negative intention toward its performance) to High Civil Disobedience (participation in nonviolent aggressive action and positive intention toward political violence). Political violence occurs only under the following set of circumstances: (1) university context; (2) leftist ideological commitment; (3) low policy and trust; and (4) negative or very negative Political Support. But even under these conditions civil disobedience is a far more likely outcome than violent action.

One puzzle remains to be solved. Granted that aggressive political behavior is more acceptable to the Left than to the Center or Right, we have nonetheless seen that a facilitative community context counteracts the inhibitory effect of ideology to some degree: In the university milieu, under the low policy and trust condition, 26 of the 53 persons who score in the negative range on the Political Support variable also score at the civil disobedience levels on the Aggressive Behavior index. Yet, given the exact same conditions, only with community context inhibitory, all 65 of those who score in the negative range on Political Support also score at the inactive level on the Aggressive Behavior index. Is the Political Support variable for some reason irrelevant under

just this latter set of conditions? Table 15 provides an answer.

Centrists do not have much of an alternative if they are in an inhibitory context and are negative toward the political system. But the Right does, at least in Germany. This is the NPD, a party of the extreme Right which for a time displayed clear neo-Nazi coloration. In the nonuniversity milieu, 20.6 percent of those on the Right-and even 16.7 percent of those in the Center-who register in the "negative" zone on Political Support would like to see the NPD either represented or the majority party in parliament. Among those few on the Right who score at the most negative level of support for the political system, one-half would like to see the NPD represented or the majority in parliament. By contrast, the rate of support for the NPD among centrists and rightists who are positive toward the political system is virtually nil. Obviously there is a relationship, and among rightists it looks almost identical to the relationship between support and aggressive political behavior among leftists in an inhibitory context. The Right in an inhibitory context simply manifests its opposition in a different way. Finally, it is also worth noting that those on the Left who score at inactive on the Aggressive Behavior index are considerably more likely to express support for the German Communist Party (DNP) if their support for the political system is negative, than if their support is intermediate or positive.

Conclusion

To return to the questions which began this paper, our data indicate that the analytical distinction between affect for an incumbent administration and affect for the system of government is important empirically because it affords a more precise explanation of aggressive political behavior than would be possible if political affect were treated as a single entity. Affect for the system of government is a more powerful explanatory variable than affect for an incumbent administration. It is generally related to aggressive political behavior regardless of whether incumbent affect is positive or negative (though the relationship is depressed or heightened depending especially on whether ideological commitment is non-Left or Left). Incumbent affect is unrelated to aggressive political behavior when system affect is controlled for-except for modest correlations which appear under one or two special conditions. If we are to make causal inferences, it would appear that bivariate correlations between measures of aggressive political behavior and variables sensitive to incumbent affect actually represent either indirect or spurious correlation. This is due to the fact that incumbent affect also is correlated with what appears to be the more powerful and direct antecedent of aggressive political behavior, namely, affect for the system of government—Political Support, as we label it.

All indications point to the conclusion that trust in government is, as Stokes intimated some time back and as Citrin recently argued, more sensitive to incumbent than to system affect and should be classified in the same category as such variables as evaluation of public policy outputs and general approval-disapproval of the performance of an incumbent administration. Incumbent affect, while probably not of much consequence in any direct causal sense, does appear to condition modestly the relationship between system affect and aggressive political behavior such that when the incumbents are trusted and their policies are regarded in a positive light, the relationship is weaker than it is when the incumbents are not trusted and their policies are not regarded positively. The relationship between system affect and aggressive political behavior is most strongly specified by ideology: the relationship generally holds up well among persons with a leftist ideological orientation; but it is sometimes erratic and considerably weaker among persons with a centrist or rightist ideological orientation. However, among rightists, when one substitutes support of an extreme Right oppositionist party for participation in aggressive political behavior, then in place of a puzzling negative relationship between degree of antisystem sentiment and aggressive action, one finds a not inconsequential positive relationship.

The data indicate that the theory of the behavioral consequences of the incumbent-system distinction suggested by the work of Lipset and Easton must be qualified according to whether ideology and community context are inhibitory or facilitative. The theory should be reformulated to take into account the effect of ideological commitment and community context, for each has a separate role to play in the explanation of aggressive political behavior, and the nature of this role does not appear to be linear additive.

The Federal Republic of Germany today is a stable political system. In part this is because it has managed, in a relatively short space of time, to create legitimacy and also to perform effectively, at least in the eyes of most of its population. This is not so much the view seen by many of those at its universities, but those with an opposing view have not yet managed to convince the populace at large. Out of 1033 persons in a nonuniversity milieu whom we could score on all of our variables, only ten fell in the most volatile condition: leftist, low on policy evaluation and political trust, very negative toward the system of government. But half of these ten scored at the civil disobedience

levels on the Aggressive Behavior index; of the other five, three support the German Communist Party. These are large percentages of a small base. But very negative system support is an extremely deviant sentiment in the rural and urban communities from which our samples were drawn. In the rural milieu, only 2.4 percent registered at "very negative" on the Political Support variable. In the urban milieu,

Table 15. Relationship Between Political Support and Attitude Toward Oppositionist Political Parties Controlling for Ideological Commitment

Controlling for:	Party				F	Political	Suppo	rt				
Ideological	NPD Represented or Majority		ry itive	Pos	itive	Intern	nediate	Neg	ative	Ve: Nega		
Commitment	in Parliament	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	
Left	Yes No	_	(0) (2)	2.8 97.2	(1) (35)	5.6 94.4	(3) (51)	6 .2 93.8	(1) (15)	100.0	(0) (5)	(N = 113)
Center	Yes No	<u>-</u>	(0) (3)	2. 4 97.6	(1) (40)	6.9 93.1	(6) (81)	16.7 83.3	(3) (15)	- -	(0) (2)	(N = 151)
Right	Yes No	-	(0) (3)	2.9 97.1	(1) (34)	8.4 91.6	(8) (87)	20.6 79.4	(7) (27)	50.0 50.0	(4) (4)	(N = 175)
Ideological	DKP Represented or Majority	Ve Posi	ry itive	Posi	itive	Intern	nediate	Neg	ative	Ve Nega		
Commitment	in Parliament	%	(N)	%	(N)	%	(N)	%	(N)	%	(N)	
Left	Yes No	-	(0) (2)	10.8 89.2	(4) (33)	8.9 91.1	(5) (51)	29.4 70.6	(5) (12)	60.0 40.0	(3) (2)	(N = 117)
Center	Yes No	- -	(0) (4)	5.0 95.0	(2) (38)	7.6 92.4	(7) (85)	10.5 89.5	(2) (17)	- -	(0) (2)	(N = 157)
Right	Yes No	-	(0) (4)	2.9 97.1	(1) (34)	2.1 97.9	(2) (94)	6.1 93.9	(2) (31)	100.0	(0) (8)	(N = 176)

Table A1. Relationship Between Composite Aggressive Behavior Index and Index of Actual Participation

Actual			Ag	gressive	e Behav	ior Ind	ex				
Participation	0	1	2	3	4	5	6	7	8	9	10
High (3-5)	5	0	1	0	1	1	3	5	16	6	5
Medium (1-2)	24	9	50	41	67	43	35	13	0	0	0
Low (0)	1431	229	150	146	57	42	0	0	0	0	0
TOTAL	1460	238	201	187	125	86	38	18	16	6	5

Table A2. Relationship Between Composite Aggressive Behavior Index and Index of Intention to Participate

Intention to				Agg	gressive	Behavi	or Inde	×			
Participate	0	1	2	3	4	5	6	7	8	9	10
High (5-10)	. 0	0	0	15	49	84	35	17	13	6	5
Medium (1-4)	0	249	205	176	86	26	4	1	3	0	0
Low (0)	1526	0	0	0	0	0	0	0	0	0	0
TOTAL	1526	249	205	191	135	110	39	18	16	6	5

the "very negative" rate was 1.8 percent. Should "very negative" sentiment for the system of government become less deviant, the incidence of aggressive political behavior would likely increase. Should the incidence of aggressive political behavior increase, aggressive action itself would become less of a deviant form of political behavior. And if negative system support and aggressive political behavior both became less deviant in the nonuniversity milieu, this milieu would begin in all likelihood to lose much of its inhibitory effect on the relationship between system support and aggressive behavior.

Appendix. Analysis of the Components of Aggressive Political Behavior

As mentioned in the text, the aggressive behavior measure is built from responses about behavioral intention and actual participation in the following five behaviors: (1) refusal to pay rents or taxes; (2) participation in a wildcat strike; (3) takeover of factories, offices, and other buildings; (4) participation in fights (battle with police, battle with other demonstrators); (5) participation in a group which wants to dislodge the government by violent means. Refusal to pay rents or taxes and participation in a wildcat strike are examples of nonviolent civil disobedience. Participation in fights and involvement with a group which wants to overthrow the government by violent means are examples of political violence. The item dealing with seizure of buildings represents a behavior which could fall either way. If we include this item within the domain of violence, then participation in three or more behaviors would mean that a person must have participated in at least one violent action. Table A1 gives the relationship between the composite Aggressive Behavior index and an index of actual participation. The High category of participation is defined by involvement in three or more actions, of which one or more entails violence. The Medium category is defined by involvement in one or two actions. Most of these entail civil disobedience. (Of those participating in one or two aggressive actions, 46 percent were involved in rent or tax strikes, 39 percent were involved in a wildcat strike, 17 percent were involved in the seizure of a factory, office, or other type of building, 16 percent were involved in fights with police or other demonstrators, and 4 percent were involved with a revolutionary group.) The Low category is defined by lack of involvement in any aggressive action.

There are four distinct zones or levels on the Aggressive Behavior index when we look at the relationship between it and actual participation. The vast majority of persons who score from 0 to 1 on the composite index fall in the Low category of actual participation (98 and 96 percent, respectively). This is clearly an *Inactivity* zone.

A clear preponderance of persons who score from 2 to 3 on the composite index also fall in the Low category of actual participation (75 and 78 percent, respectively). A minority of these persons have participated in one or two behaviors—mostly nonviolent civil disobedience. This is clearly a Low Civil Disobedience zone.

The majority of persons who score from 4 to 7 on the composite index fall in the Medium category of actual participation (54, 50, 95, and 72 percent, respectively). This may be designated as a High Civil Disobedience zone.

Finally, all persons who score from 8 to 10 on the composite index fall in the High category of actual participation. This may be designated as a *Political Violence* zone.

The relationship between the composite Aggressive Behavior index and an index of Intention to Participate in aggressive behavior is given in Table A2. The Intention to Participate index is a summation of scores across the five intention items, where negative intention is scored "0," conditionally positive intention is scored "1" (might do it), and unconditionally positive intention is scored "2" (definitely would do it). To fall in the High category of intention (5 to 10) a person must have expressed at least a conditionally positive intention toward performance of one of the violent actions. The Low category is defined by consistently negative intention toward performance of all the actions. The Medium category represents conditionally or unconditionally positive intention toward performance mainly of civil disobedience actions.

All persons who score at 0 on the composite index have consistently negative intention toward performance of aggressive behavior and, although all of those who score at 1 on the composite index fall in the Medium category of intention, the vast majority of these express only a single conditionally positive intention (85 percent). Thus, the Inactivity zone is characterized by very consistent negative intention toward performance of aggressive behavior.

Nearly all persons who score from 2 to 3 on the composite index fall in the Medium category of intention, and the vast majority express more than one conditionally positive intention (87 and 92 percent, respectively). Thus, the

Table A3. Relationship Between Participation in Discrete Aggressive Activities and Composite Aggressive Behavior Index

								I	Aggressiw	Aggressive Activities	8							
Aggressive Rebarior	Ž	Ketuse Pay Kent, Taxes	Kent, 1	axes					Illega	Illegal Strike					Seize I	Seize Buildings		
Index	Neg	Negative	Trans	Transitional	Pos	Positive	Ne	Negative	Trans	Transitional	Pos	Positive	Neg	Negative	Trans	Transitional	Pos	Positive
	%	3	%	2	8	3	26	3	%	2	28	ટ્ટ	%	2	%	Z	%	3
Violence (8-10)	0	6	1.7	(10)	. 12.2	(11)	0	6	0.7	○	18.9	(23)	0	(0)	0.4	(2)	30.0	(24)
(4-7)	0.7	(12)	35.0	(200)	63.3	(88)	0.2	(3)	38.4	(235)	50.8	(62)	0.3	9	48.0	(243)	65.0	(52)
(2-3)	5.3	(66)	46.7	(267)	24.5	(34)	4.7	(82)	44.1	(270)	30.3	(37)	8.2	(155)	45.5	(230)	5.0	○
(0-1)	94.1 ((1663)	16.3	(36)	0	6	95.1	(1646)	16.8	(103)	0	6	91.4	(1718)	6.1	(31)	0	6
TOTAL	100.0 (1768)	(1768)	100.0	(572) $d_{yy} = .$	100.0	(139)	100.0	(1731)	100.0	(612) $d_{viv} = .81$	100.0	(122)	100.0	(1879)	100.0	(206)	100.0	(08)
	-			1						*						*	,	
								- 1	Aggressiv	Aggressive Activities	es							
		•			Figh	Fight Police, Demonstrators	Demon	strators			Use Vio	Use Violence Against Government	ainst Go	vernmen	Ħ			
		Aggressive Behavior	SSIVE	Ne	Negative	Trans	Transitional	Po	Positive	Negative	ıtive	Trans	Transitional	Po	Positive			
		Index		%	(S)	8	3	%	(N)	%	(S)	%	3	26	3			
		Violence (8-10)	109 10)	0.0	(1)	9.0	(1)	42.4	(25)	0.1	(2)	5.4	(13)	80.0	(12)			
		(4-7)		6.0	(137)	81.2	(125)	54.2	(32)	5.3	(117)	72.1	(173)	20.0	(3)			
		(2-3)		16.2	(398)	14.9	(23)	3.4	(2)	16.0	(353)	16.3	(33)	0	60			
		(0-1)	1)	77.7	(1759)	3.2	(5)	0	,	78.6	78.6 (1736)	6.3	(15)	0	6			
		TOTAL	[AL	100.0	(2265)	100.0	(154)	100.0	(65)	100.0	(2114)	100.0	(203)	100.0	(15)			
						$d_{\mathcal{Y}X}$	dyx = .89					d_{yx}	$d_{yx} = .85$					•
					-			*										

Low Civil Disobedience zone on the composite index is characterized by consistently positive intention toward participation in civil disobedience actions but generally negative intention toward participation in more violent action.

Those who score from 4 to 7 on the composite index manifest progressively greater positive intention toward participation in violent action, as evidenced by the increasingly larger proportions who fall in the High category of intention (36, 76, 90, and 94 percent, respectively). The High Civil Disobedience zone on the composite index is thus characterized by increasingly greater potential for participation in political violence.

Virtually all of those who score from 8 to 10 on the composite index fall in the High category of intention. The Political Violence zone is characterized by very consistent positive

intention toward participation in violent action.

Thus, combining of intention to participate and past participation into a single composite index gives a measure which is not only sensitive to actual past behavior but also is future-oriented and corresponds to a theoretically plausible conceptualization of the aggressive political behavior continuum, according to which, as one goes from low to high on the continuum, one moves from (1) inactivity to (2) potential for civil disobedience, to (3) actual participation in civil disobedience and potential for violent action, and finally to (4) actual participation in political violence. In the analysis reported in the text we collapse the Aggressive Behavior index into the four zones just defined.

The relationship between each aggressive behavior and the collapsed composite index is

Table A4. Zero-Order Correlations (γ) Between Index of Aggressive Behavior Participation and Political Affect Variables

	C	orrelation Bet	ween Actual Aggr	ressive Behavi	or Participation a	nd	
Politic	al Support	Politi	ical Trust	Polic Eva	y Output duation		umbent Iluation
γ	(N)	γ	(N)	γ	(N)	$\overline{\gamma}$	(N)
.56	(2215)	.37	(2376)	.36	(2301)	<i>.</i> 19	(2260)

Table A5. Correlations (γ) Between Aggressive Behavior Participation Index and Political Affect Variables, with Variables Controlled

Controlling for:	Correlat	tion Between Actual Agg	ressive Behavior Participa	tion and:
	Politi	cal Trust	Policy Outpu	t Evaluation
Political Support	γ	(N)	γ	(N)
Very positive	14	(92)	.33	(91)
Positive	.16	(553)	.23	(542)
Intermediate	02	(919)	03	(915)
Negative	07	(414)	.08	(417)
Very negative	.32	(182)	.16	(182)
		Political	Support	
Political Trust		γ	(N)	
Very positive		30	(81)	
Positive	•	.55	(441)	
Intermediate		.41	(954)	
Negative		.50	(541)	
Very negative		.64	(143)	
		Political	Support	
Policy Output Evaluation		γ	(N)	
Very positive		.70	(52)	
Positive		.49	(501)	
Intermediate		.45	(1104)	
Negative		.63	(436)	
Very negative		.61	(54)	

shown in Table A3. Scale-item correlations are very high.

For reasons mentioned above and in the text, we feel that the Aggressive Behavior index is superior to the index of actual participation. The following is a representative selection of correlations between the political affect variables under various conditions and the index of

actual participation in aggressive behavior. This Index's correlations with the affect variables are not markedly different than those with the Aggressive Behavior index reported in the text. On the whole, they tend to be slightly lower, but in no instance do they materially alter the interpretation of the relationships using the Aggressive Behavior index.

Table A6. Correlations (γ) Between Index of Actual Aggressive Behavior Participation and Political Affect Variables and Other Political Affect Variables

Controllin	g for:	Act	Correlation Lal Aggressive Beh	n Between avior Participation	n and:
	Political	Politi	cal Trust		Output ation
Community Type	Support	$\overline{\gamma}$	(N)	γ	(N)
Rural and Urban	Positive ^a Intermediate Negative ^b	.12 27 41	(504) (540) (156)	.24 16 44	(492) (530) (156)
University	Positive ^a Intermediate Negative ^b	02 .10 .29	(141) (379) (440)	.07 05 .23	(141) (385) (443)
	Political		Political	Support	
Community Type	Trust		γ	(N)	
Rural and Urban	Positive ^a Intermediate Negative ^b		.31 .09 .60	(381) (505) (314)	
University	Positive ^a Intermediate Negative ^b		.46 .29 .36	(141) (449) (370)	
	Policy Output		Political	Support	
Community Type	Evaluation Evaluation		$\overline{\gamma}$	(N)	
Rural and Urban	Positive ^a Intermediate Negative ^b		.38 .17 .41	(414) (558) (206)	
University	Positive ^a Intermediate Negative ^b		.31 .33 .45	(139) (546) (284)	

^aIncludes "Very Positive."

bIncludes "Very Negative."

Table A7. Correlations (γ) Between Aggressive Behavior Participation Index and Political Affect Variables, Controlling for Ideological Commitment

Control	ling for:	Act	Correlatio ual Aggressive Beh	on Between avior Participation	and:
Ideological	Political	Politi	cal Trust	Policy Evalu	Output ation
Commitment	Support	γ	(N)	γ	(N)
Left ^a	Positive ^c Intermediate Negative ^d	.33 .20 .27	(200) (298) (402)	.58 .09 .20	(198) (304) (406)
Center	Positive ^c Intermediate Negative ^d	.09 18 23	(251) (338) (86)	.10 .08 06	(245) (333) (84)
Right ^b	Positive ^c Intermediate Negative ^d	14 06 .08	(137) (206) (66)	.24 26 26	(135) (201) (68)
Ideological	Political		Political	Support	eranna radii fare dhafar Bullenia mba
Commitment	Trust		γ	(N)	
Left ^a	Positive ^c Intermediate Negative ^d		.75 .43 .33	(217) (377) (306)	
Center	Positive ^c Intermediate Negative ^d		.13 12 05	(199) (309) (171)	
Right ^b	Positive ^c Intermediate Negative ^d		.15 07 .50	(68) (181) (160)	
Ideological	Policy Output		Political	Support	
Commitment	Evaluation		$\overline{\gamma}$	(N)	
Left ^a	Positive ^c Intermediate Negative ^d		.68 .49 .40	(213) (454) (241)	
Center	Positive ^c Intermediate Negative ^d		10 08 .16	(187) (372) (103)	
Right ^b	Positive ^c Intermediate Negative ^d		.59 .19 06	(84) (213) (107)	

aIncludes "Extreme Left."
bIncludes "Extreme Right."
cIncludes "Very Positive."
dIncludes "Very Negative."

COMMUNICATIONS

An Apology to Alexander L. George

From Confusion to Confusion

TO THE EDITOR:

Since Alexander George has been exposed to unpleasantness as the result of his blameless compliance with a request of mine in connection with the Review, I believe I owe him an apology in these pages. As a great many busy scholars do each year, Professor George undertook to read and evaluate a manuscript at my request. Through a clerical error in our office, a copy of this manuscript containing annotations in his handwriting, but not identifying him in any other way, was passed on to James David Barber when I invited Professor Barber to prepare a comment on it for publication. Further conversation ensued between Professor George and Professor Barber, to which I was not privy, which establishes to my satisfaction that I failed to protect Professor George's anonymity as I should have.

Professor Barber's comment was published in the March 1977 APSR. Faithful readers of the Review will no doubt recall that this comment contains many statements that speak unkindly of my management of the editorial process and consequently I felt constrained from subjecting it to editorial modification. However, I welcomed Charles O. Jones' suggestion that he write Professor Barber with editorial suggestions; these were ignored. In the course of this activity I lost sight of my further responsibility to protect Professor George from the publication of untrue comments referring to him. If I had seen his interests clearly and promptly, I think I would have acted differently, either to seek the removal of the offending language, or to provide space for a rebuttal by Professor George in the same issue. After Professor George called the error to my attention, he did receive this space in the June APSR. I am very sorry, however, to have contributed to a situation that has caused him personal distress and especially so since this occurred as the result of his willingness to contribute his knowledge to the editorial pro-

NELSON W. POLSBY

University of California, Berkeley

TO THE EDITOR:

In the March, 1977 issue, Michael Margolis criticizes, among other studies of American voting behavior, my 1972 APSR article, "From Confusion to Clarity." While I welcome scholarly controversy, I would like briefly to answer his direct criticisms.

- 1. Margolis' major point is that my analysis is not based on the total number of respondents, but on a smaller group. The crucial question we analyze is the changing proportion of respondents who believe the Democrats are more liberal than the Republicans on a series of policy issues. Margolis here uses the entire sample; I use only those voters who see a difference of any kind between the parties. Even using his method, there is a trend evident toward greater perceived party differences from 1956 to 1968. The crucial point, however, is that my method is exactly the same as that used in The American Voter (p. 182 ff.), a book Margolis still regards as holy writ. Since I was arguing that the conditions described in that volume of the 1950s had changed, it is important to replicate its techniques exactly. By that exact replication-rather than the new techniques Margolis attacks-we find a change in voter awareness.
- 2. While conceding the trend to 1968, Margolis argues it does not continue through the 1972 election. However, if one continues to use the same methods as I did previously, we find the same voter awareness in 1972 as in other recent years. Thus, in an updated publication available to Margolis, I find that 86 percent of all voters, and more than 80 percent of each party identification group, agree that the Democrats are more liberal on issues of medical care and a federal job guarantee (Voters' Choice, Harper & Row, 1975, p. 172). These are the only questions available throughout the 1956-72 period, and they again show a definite increase in the electorate's consciousness. The same trend can be seen in perceptions of the parties' general ideological positions. In 1960, nearly half of the Democrats thought their party was more conservative or as conservative as the Republicans. By 1972, the propor-

tion was reduced to a fourth. Republicans and Independents followed the same pattern, "from confusion to clarity" (Voters' Choice, p. 181).

- 3. Margolis then introduces new issues into his analysis, those of integration in places of public accommodation, strong federal government, and Vietnam. Obviously, these are irrelevant to my analysis, which deals only with issues current from 1956 on.
- 4. At three different places, Margolis departs from his usual scholarly courtesy to take a "cheap shot" at the absence of the raw Ns on which the table percentages are based. Admittedly, it is good academic practice to include these figures. But Margolis knows these figures, since his printout comes from the same SRC/CPS data sources as mine. Moreover, he knows the Ns are substantial and cannot affect the statistical relationships shown. (To give one typical example: attitudes on aid to education in 1960 are based on Ns of 1704, 1439, and 792, in Tables 1, 2, and 3 respectively.) To stress the omission is to stress the form of tables over the substance of findings.

More generally, I believe Margolis has missed the major point of my work and that of others who have argued that voters are now responsive to issue-oriented politics. That point emphasizes the political context of voter behavior. Given appropriate electoral cues, persuasive leadership, and concerned parties, we believe the citizens of the United States can translate their issue preferences into electoral decisions. The movement to clarity in American politics is evident to virtually every observer. Hopefully the remaining confusion in political science will soon be relieved as well.

GERALD M. POMPER

Rutgers University

TO THE EDITOR

I too believe that, given the appropriate cues, citizens are capable of translating their issue preferences into electoral decisions. Professor Pomper and I disagree, however, about the degree to which citizens have succeeded in making this translation.

My argument concerning the lack of Ns is not a mere quibble over form. At various stages of his analysis, Pomper excludes from consideration those with no party identification, those with no opinion on the issues examined, or those who perceive no differences between the parties. As a result, he comes up with figures which in my judgment substantially exaggerate the extent of issue voting for the years

1956-68. Reporting the sample Ns and the Ns upon which his figures were based would have avoided this exaggeration. (See table 1 of my paper.)

My introduction of new issues serves to emphasize that the major issues of the 1950s and early '60s were not the same as those of the late '60s and early '70s. If issue voting has substantially increased, it would seem that voters ought to be able to relate their preferences on these new issues to their perceptions of the policy positions of the parties. In Table 3 of my paper the figures demonstrate that voters' perceptions of party differences regarding public accommodations and strong government in Washington became increasingly muddled rather than clarified between 1964 and 1972.

The figures Pomper cites from Voters' Choice regarding perceptions of party differences on the issues of medical care and job guarantee in 1972 evidently exclude the one-third to one-half of the electorate who have no opinions or no perceptions of the party positions on the issues. Unfortunately, we have no way of knowing for sure just how many voters have been excluded, for once again Professor Pomper fails to report the Ns upon which his percentages are based!

MICHAEL MARGOLIS

University of Pittsburgh

Indian Rights

TO THE EDITOR:

Professor Frances Svensson, in her review (APSR, Dec. 1976, 1306-08) of my book, The States and Their Indian Citizens (Washington, D.C.: GPO, 1972), says I looked to "the inevitability of the disintegration of community interests in favor of individual ones" rather than considering the "roles of individuals and groups in a democratic society."

In fact, I pointed out that Indian tribal leadership looks to developing self-sufficient Indian communities and individuals. Both are appropriate goals; not all Indians live in Indian communities. For those Indians living in communities with tribal governments there is the possibility of developing community self-sufficiency and integrating such communities into the U.S. political structure. In *The States*... (p. 153) the Menominee example of becoming a Wisconsin county is cited, and the possibility of considering the Navajo for our 51st state is mentioned. The genius of our governmental framework and our political system is that it is

flexible and can accommodate pluralism: "... the majority of tribal leaders where they now have tribal governments seemed to see no major difficulty in the Indian governments working with local, state, and federal governments to resolve problems as they arise.... There would seem to be little question but that Indian governments will continue to exist for as long as the Indian communities involved want them" (p. 153).

Also Svensson apparently missed my statement that the majority society should not try to change Indians' or others' pluralistic goals "except insofar as they interfere with the rights of others" (p. 128). I cite the Amish as being economically self-sufficient, leading their own lives in their own way, and enjoying maximum freedom as a culturally diverse group (p. 128). The Indians may do the same.

The reviewer raised the question of adequate treatment of Indian relations with the states, in particular, the tension between Indian groups and the states. There is no more tension in the Indian-state relationship than in the Arlington County-Commonwealth of Virginia relationship. The States cites examples of Indian desires to cooperate with the state and achieve the benefits of full participation in state programs (pp. 91–92). For example, 68 percent of the Indian children in states with federally recognized Indians are in public schools operating under state law.

The tensions cited by Svensson are rarely discussed in conjunction with all of the related facts. Neither the federal government nor Indian governments are equipped to provide certain services to Indian communities. Foster home care or specialized institutional care for dependent or handicapped children; institutions for the mentally ill, aged, or infirm; judicial procedures to place children in foster homes or state institutions; state correctional institutions: and institutions of higher learning-all these are examples of functions performed by state governments used by all citizens, including Indians in reservation communities (p. 74). Having separate law and order on Indian reservations, in fact, frequently leads to increased

House Concurrent Resolution 108 (termination) and P.L. 280 (permitting state law and order on Indian reservations) were specifically mentioned by Professor Svensson as tension producing. No termination ever took place without tribal consent, including Menominee (pp. 63-64; pp. 220-223). In most cases P.L. 280 jurisdiction was assumed by the state only if the Indians concerned concurred (pp. 36-37). In Nevada eight counties refused to

take jurisdiction even though the Indians therein petitioned them to do so (p. 36). These facts are conveniently lost in the theory and rhetoric of advocates of Indian sovereignty.

Professor Svensson does not address the book's discussion of "Who is an Indian?" There may be 10 million people in this country with some Indian blood (p. 139), but only 800,000 were identified as Indians in the 1970 census, of which 37 percent lived in metropolitan areas. Only about 500,000 have any federal reservation connection (p. 176). Is a person with 1/16 degree of Indian blood an Indian? Which "Indians" are the concern of the reviewer? I tried to discuss all aspects.

I did not "defend the argument that government-sponsored policies of enforced acculturation were ultimately in the best interests of the Indians" as alleged by Professor Svensson. On the contrary I stated my belief that "permissive acculturation" would have facilitated adjustment, but that it was impossible at the time of the Allotment Act of 1887 (p. 15). Svensson asks: "Why and how in 'pluralistic' American society did there come to be no alternative? Why wasn't the environment conducive?" I tried to convey the temper of the times, including Theodore Roosevelt's statement that the tribes had to be broken up (pp. 15-18). Footnote 42, p. 17, refers to Appendix I which treats the environment in more detail. Secretary of Interior Carl Schurz pointed out in 1881 that feeling against the Utes was running high in Colorado and allotment was the only device that saved a portion of their lands. Without allotment the Ute tribe would have been destroyed and all of the land taken (p. 225). Later the national attitude changed (pp. 19 - 26).

Professor Svensson and I are probably more in agreement than the above might indicate. The concept of culture change (p. 159 ff.) and Indian potential as I see it (p. 145 ff.) bear on the relation of the individual to the group (Svensson's concern) and the response of both to their environment. But I have some trouble with her statement that "the pre-eminence of communal responsibilities and interests over individual rights does not necessarily lead to tyranny, particularly within a tribal rather than a state political structure." Some of the Pueblo government practices are incompatible with the Bill of Rights. This is a sensitive and important issue and relates to the "human rights" pronouncements of President Carter. I agree with Professor Svensson's suggestion that additional study is needed, but, as she correctly says, I favor a state arrangement with its constitutional protection of individuals.

THEODORE W. TAYLOR

Arlington, Virginia

TO THE EDITOR:

I find it difficult to know where to start in replying to Mr. Taylor, since we do not even agree on the "facts" of many of the cases, let alone their meaning. At base, I suspect, we are simply speaking past one another. Mr. Taylor refers to the possibility of Indian communities "integrating ... into the U.S. political structure." What if they don't want to? His choice of the Menominee example is especially apt, since it is a classic example of a tribe which was coerced in the crudest possible ways by the federal government to accept termination and which, after a struggle of many years, has finally been able to regain some semblance of its former status. And let's face it, the Navajo are never going to be a state.

Mr. Taylor also remarks that he too is for Indian goals "except insofar as they interfere with the rights of others." He notes that: "Some of the Pueblo government practices are incompatible with the Bill of Rights." The issue is: who decides what a "right" is and how it should be recognized and whose conception of "rights" should have priority? Pueblo ways of doing things antedate American ones, and have been found quite satisfactory to the Indians involved. I refer Mr. Taylor to the hearings held in Pueblo country on the "Indian Bill of Rights" in 1968. The "right" of internal self-determination of Indian tribes has been reinforced in numerous treaties and court decisions (including those of Chief Justice Marshall). If Indians choose in some cases to emphasize a code of rights which does not exactly correspond to the U.S. Bill of Rights, they are entitled to do so. Where is all the flexibility and pluralism that Mr. Taylor ascribes to the U.S. political system?

Mr. Taylor's rosy perspective on relations between the tribes and the states, I must confess, boggles the mind. I simply refer him to the record on Indian reaction to H.C. 108 and P.L. 280. Mr. Taylor's gloss on "tribal consent" to termination makes no reference to the enormous pressures put on the tribes to go along; an example is the refusal of the federal government to release money granted to the Menominee by the Indian Claims Commission in compensation for lands illegally seized from the tribe until the tribe agreed to termination. And this was only one of many pressures placed on the Menominee alone: And I must correct

Mr. Taylor; a number of states did assume jurisdiction on Indian reservations under P.L. 280 without Indian concurrence. The original bill did not require Indian concurrence! (It was later modified.) While some tribes did desire it, many others did not. Is Mr. Taylor aware of the referendum which the United Sioux Tribes organized in South Dakota to reject P.L. 280? Speaking of tribal-state relations, does Mr. Taylor know what it is like to be an Indian in South Dakota? The "increased tension" that Mr. Taylor attributes to separate jurisdictions arises more from the outraged sensitivities of whites who refuse to submit to Indian jurisdiction than to Indians off the reservation who object to white jurisdiction there. Particularly good examples of the complexities of this jurisdiction question are to be found in Washington State, which assumed jurisdiction originally without concurrence from all the tribes (in violation of its own charter of incorporation) and which has been reaping the consequences in recent Indian-state confrontations on a number of reservations (e.g., Colville, Yakima, Puyallup, etc.).

The crucial issue underlying our divergent perspectives centers, I think, on our approach to history. Mr. Taylor's position comes down to: whatever was, had to be. My position comes down to: why was it that way and not another? I know all about the views of Andrew Jackson and Theodore Roosevelt, and for that matter Cotton Mather and John Wayne, on Indians. What I am interested in is why their views triumphed; why more balanced views of the requirements of justice and equity-and even decency-did not. And I am interested in what this analysis reveals about the American political process, about its "flexibility" and "pluralism," and about the problems of ethnic relations in multiethnic societies.

If anything, our exchange is useful as itself an example of the difficulties of communication across boundaries. Mr. Taylor's views seem generally to correspond to those of many persons involved in the formulation of Indian policy in this country, both in the B.I.A. and in other agencies and offices (including Congress). My views, on the other hand, are those of an Indian who has been involved in Indian affairs and organizations most of my life. Mr. Taylor prefers "a state arrangement with its constitutional protection of individuals" as the final resolution of the problem of Indian status. I prefer the right of Indian tribes and communities to make their own choice, which should include the treaty-guaranteed option of autonomous internal self-determination, whether or not its form corresponds to American political conceptions.

FRANCES SVENSSON

University of Michigan

Financing Elections

TO THE EDITOR:

In his review essay in the March 1977 APSR, David Adamany assessed my book, Financing Elections: The Politics of an American Ruling Class. Although he was kind enough to give credit where he saw it due, the reviewer unfortunately made a number of errors which demand correction in order to set the record straight.

Adamany said I overstated the degree to which the corporate rich give to both parties; but nowhere did I imply that more than a minority split contributions, and I reported a compilation by the authoritative Citizens' Research Foundation on the amount of split giving.

The reviewer said I gave incorrect data on the amount of big McGovern contributions; but what I really did was to *predict* that when the official figures were in they would show that McGovern raised no more than half his money from givers of \$100 or less, and this prediction has since been borne out.

The reviewer said I garbled the text of the

1971 Election Finance Law; on review I find the provisions were accurately summarized.

Adamany said I was unaware of studies of the decline in dues revenue by European parties; in fact I was quite aware of these, but was basing my argument that dues financing is the only really democratic method on the reversal of present trends, not on their continuation.

The reviewer said I contributed no new research; but my study of the economic backgrounds of the 1968 Cleveland metropolitan area big givers, reported in the book, constituted contributory "evidence on the sociology of givers"—something to which Adamany attached considerable importance elsewhere in his review essay.

Finally, Adamany views my argument about the structure of corporate power and its incompatibility with democracy as wrong, and is particularly upset about my analysis of the broadly conservative purposes of the election finance reformers. Now, this was the core argument of the book and can hardly be profitably reargued in the space of a communication. I must point out, however, that Adamany confused my analysis with elitist arguments like Domhoff's. In reality, my book was a Marxian analysis of power, with particular attention to the role of the electoral process.

DAVID A. NICHOLS

State University of New York at Albany

EDITORIAL NOTE

From time to time I will use this space to explain procedures and policies. We try to provide sufficient information in the "Instructions to Contributors" but on occasion it may be helpful to supply additional information.

An issue of current interest is that of critical analyses of and comments on a scholar's work or reputation. As with previous editorial staffs, we see it as the continuing function of the journal to encourage and promote professional exchange on major scholarly issues. The precise means by which this policy is implemented may, and should, be expected to vary from one editorial staff to the next. Whatever means are employed, however, we wish to reaffirm the following policy of The American Political Science Review: The discourse in scholarly exchange is expected to follow the commonly accepted standards of civility. Should these standards be violated for whatever reason, the offended person will be given an opportunity to respond. Where feasible this response will appear in the same issue.

Articles Accepted for Future Publication

- M. L. Balinski and H. P. Young, International Institute for Applied Systems Analysis (Schloss Laxenburg, Austria), "Stability, Coalitions and Schisms in Proportional Representation Systems"
- Paul Burstein, Yale University, "Social Cleavages and Party Choice in Israel: 'A Log-Linear Analysis'"
- Melissa A. Butler, Wabash College, "Early Liberal Roots of Feminism: John Locke and the Attack on Patriarchy"
- George W. Carey, Georgetown University, "Separation of Powers and the Madisonian Model: A Reply to the Critics"
- Arthur DiQuattro, University of Washington, "Alienation and Justice in the Market"
- Lloyd S. Etheredge, Massachusetts Institute of Technology, "Personality Effects on American Foreign Policy, 1898–1968: A Test of Interpersonal Generalization Theory"
- John W. Ferejohn and Roger G. Noll, California Institute of Technology, "Uncertainty and the Formal Theory of Political Campaigns"
- Morris P. Fiorina and Charles R. Plott, California Institute of Technology, "Committee

- Decisions Under Majority Rule: An Experimental Study"
- Mark N. Franklin, University of Strathclyde, and Anthony Mughan, University College (Cardiff, Wales), "The Decline of Class Voting in Britain: Problems of Analysis and Interpretation"
- Norman Frohlich, Joe A. Oppenheimer, Jeffrey Smith and Oran R. Young, University of Texas at Austin, "A Test of Downsian Voter Rationality: 1964 Presidential Voting"
- Fred M. Frohock, Syracuse University, "The Structure of Politics'"
- Norman Furniss, Indiana University, "The Political Significance of the Public Choice-Property Rights School"
- Douglas S. Gatlin and Michael W. Giles, Florida Atlantic University, and Everett F. Cataldo, Cleveland State University, "Policy Support Within a Target Group: The Case of School Desegregation"
- Dante Germino, University of Virginia, "Eric Voegelin's Framework for Political Evaluation in His Recently Published Work"
- James L. Gibson, University of Wisconsin at Milwaukee, "Judges' Role Orientations, Attitudes and Decisions: An Interactive Model"
- John G. Gunnell, State University of New York at Albany, "The Myth of the Tradition"
- Barbara Kellerman, Tufts University, "Mentoring in Political Life: The Case of Willy Brandt"
- Samuel Kernell, University of California, San Diego, "Explaining Presidential Popularity"
- James H. Kuklinski, Wichita State University, "Representativeness and Elections: A Policy Analysis"
- Richard D. McKelvey, Peter C. Ordeshook, and Mark D. Winer, Carnegie-Mellon University, "The Competitive Solution for N-Person Games Without Transferable Utility, With an Application to Committee Games"
- Manis I. Midlarsky, University of Colorado, "Analyzing Diffusion and Contagion Effects: The Urban Disorders of the 1960s"
- David W. Moore and B. Thomas Trout, University of New Hampshire, "Military Advancement: The Visibility Theory of Promotion"
- John M. Orbell, University of Oregon, and L. A. Wilson II, University of Nevada, "Institutional Solutions to the N-Prisoners' Dilemma"

- 18 Marie

- Charles W. Ostrom, Jr., Michigan State University, "A Reactive Linkage Model of the U.S. Defense Expenditure Policymaking Process"
- John G. Peters and Susan Welch, University of Nebraska at Lincoln, "Political Corruption in America: A Search for Definitions and a Theory, or, If Political Corruption Is in the Mainstream of American Politics Why Is It Not in the Mainstream of American Politics Research?"
- David E. Price, Duke University, "Policy-Making in Congressional Committees: The Impact of 'Environmental' Factors'
- Arlene W. Saxonhouse, University of Michigan, "Comedy in Callipolis: Animal Imagery in the Republic"

- Donald D. Searing, University of North Carolina, "Measuring Politicians' Values: Administration and Assessment of a Ranking Technique in the British House of Commons"
- Donley T. Studlar, Centre College of Kentucky, "Policy Voting in Britain: The Colored Immigration Issue in the 1964, 1966, and 1970 General Elbctions"
- Sidney Verba, Harvard University, and Goldie Shabad, University of Chicago, "Workers' Councils and Political Stratification: The Yugoslav Experience"
- Robert Weissberg, University of Illinois at Champaign, "Collective vs. Dyadic Representation in Congress"

On Values and Science: The Korean Decision Reconsidered

GLENN D. PAIGE University of Hawaii

All political scientists recognize the importance of values in science, but rarely do we have so clear an example of it in a single case. The Korean Decision, originally written from a violence-accepting standpoint is reviewed here from a nonviolent value position. It is argued that such a value shift calls for sharper analytical focus upon violence, for further decontamination of proviolent language, for more vigorous exploration of nonviolent alternatives, and for the creation of comparative actor-observer value profiles to assist awareness and control of biases in research. It is hoped that this reanalysis will assist the transition of political science as a violence-accepting and violence-legitimating social science discipline toward greater emphasis upon the creation and application of nonviolent knowledge.

Science itself is not a liberator. It creates means, not goals.... We should remember that the fate of mankind hinges entirely upon man's moral development. (Einstein)¹

In The Korean Decision² I tried to make a contribution to the scientific study of international politics by exploring in a first case study the decison-making approach to analysis that has been suggested by Snyder, Bruck, and Sapin.³ Since the period of research and writing that resulted in publication of The Korean Decision I have changed my personal value position toward violence from acceptance to rejection. The purpose of this essay, therefore, is to explain the principal differences this makes in the original Korean decision analysis.

This should interest both social scientists and critics of social science. We all profess awareness of the importance of values in social science research, but we rarely have so clear an example of it in a single case.

Original Violence-Accepting Approach

The intent of the original study was to describe the series of decisions that led to American engagement in the Korean War; to reconstruct them from the point of view of the decision makers; to analyze them in terms of the interaction of organizational, informational, and motivational variables; to evaluate them; and to seek guidance for coping with future

¹Otto Nathan and Heinz Norden, eds., Einstein on Peace (New York: Simon and Schuster, 1960), p. 312.

war-prone crisis situations. Thus I devoted two background chapters to explaining pre-decisional domestic and international conditions, seven narrative chapters to describing daily decision-making events from June 24 to June 30, 1950, an empirical analysis chapter to suggest correlations among the decision-making variables, a normative analysis chapter to evaluate the decisions, and a final chapter to suggest guidelines for "crisis management" in Korealike situations.

Underlying all-reconstruction, analysis, evaluation, and prescription-was my normative acceptance of the employment of violence in politics, both domestic and international, Although generally to be avoided, occasions could arise in which political violence would be inescapable, just, and even heroic. My views on violence coincided exactly with those of the American decision makers whom I studied and were reinforced by my adolescent political socialization during World War II and by a personal sense of just participation in resisting blatant Communist aggression as an antiaircraft artillery communications officer in Korea from 1950 to 1952. Such views on the conditional acceptability of violence were merely the dominant mode of thinking of the mid-twentieth century in which we lived. Almost all political leaders, revolutionaries, counterrevolutionaries; political scientists, and other citizens held essentially the same views. The main political arguments of the age were not about violence per se but rather about the ends of violence and, with the advent of nuclear weapons, increasingly about its scale.

The method of decision-making analysis that I employed did not explicitly require acceptance of a violent or nonviolent value position. Rather it took the form of a value-neutral set of analytical tools. Implicitly, however, in this case it encouraged the acceptance

²Glenn D. Paige, The Korean Decision: June 24-30, 1950 (New York: The Free Press, 1968).

³Richard C. Snyder, H. W. Bruck, and Burton Sapin, *Foreign Policy Decision-Making* (New York: The Free Press, 1962).

of proviolent value assumptions (a) by stressing that decisions ought to be understood primarily as seen through the eyes of the decision makers, and (b) by not containing methods for explicating researcher values, for comparing them with those of actors, for measuring their effects upon analysis, and for evaluating decisional outcomes. Although actor values were given explicit attention, observer values were not. They were left to vary with the professional conscience of the researcher.

Because I believed that social scientists should make explicit their value preferences as indicators of possible factual and interpretive biases in their scientific works, I recorded my personal judgment of the Korean decision: "The writer regretfully cannot accept the pacifist view that the North Korean attack should not have been resisted by the armed forces of the Republic of Korea and such international allies as they could muster."4 Although not made completely clear in the original text, this judgment rested upon two beliefs: (a) that violent extension of their domains by unjust regimes justified counterviolence, and (b) that the state of civil liberties in South Korea was better than that in the opposing North. I wrote in 1968, "Less than twenty years after the event, the Republic of Korea seems to be traversing a far more open and spontaneous path of development than that of its Northern counterpart.... Without the Korean decision, this would not have been possible."5 In short. American violence had contributed to peace and freedom in Korea. Therefore the decision to fight was good.

Nonviolent Value Change

It is not essential to accept or understand the reasons why I changed to a nonviolent value position in order to appreciate the effects of this change upon re-analysis of *The Korean* Decision, but since colleagues and students have expressed keen interest in them an explanation is necessary.

At the conscious level, I am aware of the converging effects of three principal factors: public commitment to a proviolent value position, realization that Korean conditions were developing contrary to the values taken to justify violence, and discovery that we Americans who were self-righteously committed to threats of violence in Korea were ourselves

obstacles to the creation of nonviolent alternatives in international relations.

By 1973 the repressive nature of the Republic of Korea political regime had become globally notorious, mainly through the activities of the Korean CIA at home and abroad. This included the drugging and kidnapping from Japan of opposition presidential candidate Kim Dae Jung, the persecution of the poet Kim Chi Ha and Catholic Bishop Daniel Chi, and the stifling of other voices of legitimate dissent in the press, universities, and the religious community. This has been accompanied by the progressive elaboration of violence-based laws and decrees that make the pacific transfer of power increasingly unlikely.

Growing awareness of increasing repressiveness in Seoul was combined with what was the startling discovery that the main obstacle to the establishment of peaceful cultural relations between Americans and scholars from North Korea was the United States government. Meeting in Paris with scholars from the North Korean Academy of Sciences in the summer of 1973, I invited them on behalf of the University of Hawaii to visit Honolulu. They were eager to come. How shocking it was for me to discover that the American ambassador in Seoul, the Washington Korean desk officer, and the Secretary of State were adamantly opposed to such a visit and refused to give assurances that entry visas would be issued. Although the Department of State was receptive to visits to North Korea by certain Americans such as Professor Jerome Cohen of Harvard Law School and Selig Harrison of the Washington Post, it was adamantly opposed to reciprocal American hospitality. This meant no aloha for North

Against this background, it was especially disturbing for me, during a visit to the Hiroshima atomic bomb Peace Park in August 1975, to hear on a portable radio a statement by the U.S. Secretary of Defense that the government would not give assurances that nuclear weapons would not be employed in an American response to a renewed outbreak of fighting on the Korean peninsula.

In effect the United States government was threatening nuclear war in defense of a repressive regime, while obstructing the development of peaceful relations between American citizens and those of a potential military adversary. These were definitely not the conditions of freedom and peace to which the wartime killing of 1950 to 1953 had been devoted.

For me, this represented an intolerable situation of cognitive dissonance. Violent means had proved inimicable to peaceful ends. I

⁴Paige, p. 352.

⁵Ibid., p. 354.

could attempt to change reality by further commitment to the value of violence; I could elect nonviolence and then seek reality change; or I could deny the conflict and withdraw. In actuality I experienced a profound change in attitude toward violence from acceptance to rejection.

Furthermore, I experienced this change in a general sense, not just in Korea-specific terms. Perhaps this was because I had always approached the study of Korean history and contemporary society as a social scientist seeking to understand the general from the particular. Partly because of the uniquely intense concentration of American, Chinese, Japanese, and Russian influences upon the Korean people over the past century I have always thought that this convergent experience offered extraordinary possibilities for global insight. Korea thus became for me a broken link in the chain of violence forged by human history, a chain in which the glorification of each preceding link becomes the justification for its successor. But let us examine the implications of such a value change for the scientific single case analysis attempted in The Korean Decision.

Implications for Background Reconstruction

Review of the two background chapters from a nonviolent value perspective creates awareness that the reconstruction of pre-decisional "givens" contained therein is biased in at least two ways: proviolent propensities are inadequately stressed, and nonviolent potentials are almost completely ignored.

To illustrate the first point, the chapters make no mention of the American decisions to drop atomic bombs on Hiroshima and Nagasaki (August 6 and 9, 1945) as part of the historical experience that may have preconditioned American decision makers in 1950 toward engaging in violence in Korea. This is especially important for understanding the aggressive aspects of Harry S. Truman's personality and of the presidential role. The Korean Decision cites President Truman's letter to his sister of August 12, 1945, to illustrate that "he was learning to live with difficult decisions." "Nearly every crisis seems to be the worst one," wrote Truman, "but after it's over, it isn't so bad..." However the narrative is silent upon the fact that this declaration of growing ability to make difficult decisions without tormenting afterthoughts came less than a week after decisions that had wiped out two urban communities with a horrendous immediate loss of 140,000 lives in Hiroshima and 70,000 in Nagasaki. Japanese violence had legitimated American counterviolence, therefore our consciences were clear.

In accepting counterviolence as justifiable, The Korean Decision also underplays the contribution that victims of violence may have made to its initiation. Thus we are told of Truman's disgust with Russia's commitment to power politics (e.g., "Unless Russia is faced with an iron fist and strong language another war is in the making. Only one language do they understand-how many divisions have you?" "-letter to Secretary of State Byrnes of January 5, 1946),8 but we are not told of Russian perceptions of American power behavior in this era of American atomic monopoly. Applied to Korea, the background analysis does not ask if American politics from 1945 to 1950 might have contributed to a North Korean decision that only violence could assure the attainment of Communist political objectives there.

On the other hand, the background chapters are silent on the leaders, ideas, and experiences, both domestic and international, that tried to contribute to a nonviolent world in the 1945 to 1950 period. This is an artifact of proviolent values plus method: seeking to explain justifiable American counterviolence to North Korean aggression we tend not to seek evidence that nonviolent alternatives might have been even remotely possible. This means writing violent history that suppresses awareness of human potentials for nonviolent futures. Not all Koreans, for example, both leaders and other citizens, considered it inevitable or necessary that Kim Il Sung send armies south or that Sygnman Rhee invade the north in order to reassemble the tragically divided nation. Who were they? What ideas did they have? How were they suppressed? What can we learn from them for a nonviolent Korean future? Furthermore, what

⁷These figures are taken from the report of an expert commission created by the mayors of Hiroshima and Nagasaki in 1976. The losses are estimated as of December 1945, with a margin of error of ±10,000 persons in each case. The Hiroshima figure includes an estimated 20,000 military deaths. By 1950, total deaths attributable to direct bomb exposure are estimated as "more than 200,000" in Hiroshima and "more than 100,000" in Nagasaki. Takeshi Araki, mayor of the City of Hiroshima, and Yoshitake Morotani, mayor of the City of Nagasaki, Appeal to the Secretary General of the United Nations, n.p., October 1976, p. 31.

⁸Paige, p. 54.

American domestic or international resources for nonviolent politics existed in the pre-1950 period? *The Korean Decision* is written as if the American Friends Service Committee and Mohandas K. Gandhi, among others, had never existed.⁹

In short, a nonviolent perspective in decisional background analysis should lead to enhanced awareness of both proviolent and nonviolent potentials in the decision makers and their environments.

Implications for Narrative Reconstruction

The principal methodological feature of the narrative chapters of *The Korean Decision*, aside from the effort to operationalize the variables of the decision-making approach, was the effort to "decontaminate" the description from the normative biases of the author. The intent was to treat normative issues independently of factual description. The narrative might be filled with normative judgments of the decision makers, but those of the reconstructing social scientist ought to be suppressed in that context. Review of these chapters from a nonviolent perspective, however, reveals several outcroppings of proviolent biases and the consequent need for further "decontamination."

For example, describing the Korean military situation just prior to the June 26 American decision to commit air and sea forces to combat, I wrote:

At this time the Korean Government was withdrawing from Seoul to Suwon, 20 miles to the south across the Han River, as the invaders continued their unrelenting advance. The armored column spearheading their drive in the Uijongbu corridor was voraciously chewing its way through the two full South Korean divisions which hopefully had gone forth to bring it to a halt. Along the invasion route to Seoul the blood of heroes and cowards together with the blood of those bewildered ones to whom circumstance did not provide a conscious

⁹Only after experiencing value reversal did I begin to seek out and seriously study the excellent literature on nonviolent political alternatives; e.g., Richard B. Gregg, The Power of Nonviolence (London: George Routledge, 1938); Barthélemy de Ligt, Conquest of Violence: An Essay on War and Revolution (New York: Garland, 1972), reprint of 1937 edition; Joen V. Bondurant, Conquest of Violence: The Gandhian Philosophy of Conflict (Princeton: Princeton University Press, 1958); Staughton Lynd, ed., Nonviolence in America (Indianapolis: Bobbs-Merrill, 1966); T. K. Unnithan and Yogendra Singh, Traditions of Nonviolence (New Delhi: Arnold Heinemann India, 1973); and Gene Sharp, The Politics of Nonviolent Action (Boston: Porter Sargent, 1973).

choice between courage or cowardice stained the damp Korean earth the same bright red [emphasis added]. ¹⁰

From a nonviolent position the author's gratuitous judgment of Koreans who killed as "courageous" and those who sought to escape killing as "cowards" is readily apparent. From such a position the judgment, if any were to be made here, could be exactly the opposite. Readers of *The Korean Decision* can further decontaminate the narrative simply by striking out the italicized sentence.

The cited passage contains yet another example of proviolent bias in its reference to the North Korean armored forces as "voraciously chewing" their way through the southern defenders. This imagery, implying in horror film fashion a mechanical beast devouring human victims, sets the stage for human heroes to vanguish inhuman foes. The same mood is conveyed by an earlier reference to "northern legions" that "swarmed" over southern hills. 11 Northern soldiers were neither the ghosts of long-dead Roman phalanxes nor insects; like their southern counterparts, they were mainly farm boys engaged in the task of killing. These passages thus can be decontaminated further by noting that the northern armored forces "murdered" their way through two defending divisions and that large numbers of North Korean soldiers advanced across the southern hills.

In the final paragraph of the narrative section, concluding the chapter on Friday, June 30, the proviolent bias of the author is made unmistakably clear. Referring to the efforts that would have been required to carry out the air, sea, and ground combat decisions that had been taken, the passage begins with the statement: "It would be no picnic." Then the bias emerges clearly in the form of a dramatic quotation in which an actor is found to express the method-suppressed view of the author: "As Republican Representative Charles A. Eaton of New Jersey, an ordained Baptist minister, expressed it: 'We've got a rattlesnake by the tail and the sooner we pound its damn head in the better!" Note again the employment of inhuman collective imagery-North Koreans are "a rattlesnake." Note also the implied religious justification for killing.

The passage concludes with two sentences that complete the effect of bias. First, "Most Americans wholeheartedly agreed." In support

¹⁰Paige, p. 157.

¹¹Ibid., p. 82.

¹²Ibid., p. 270,

of this contention I footnoted a Roper Poll of responses to the statement that "President Truman did the right thing in sending our troops into Korea" which showed 73 percent agreement, 15 percent disagreement, and 12 percent with no opinion. This poll, of course, provides no evidence of the degree of commitment implied by the word "wholeheartedly." In view of the abrupt swing of public opinion against the war, contributing to Eisenhower's victory in the 1952 presidential election, the depth of support is questionable. For the purpose of further decontamination let us simply strike the word "wholeheartedly" from the text.

In the final sentence immediately after the assertion that "most Americans wholeheartedly agreed," I wrote: "Not the least of these were those who were committed and were slain on the distant peninsula jutting down between the Yellow Sea and the Sea of Japan." Although the dead are beyond polling, I would now hypothesize that a study of letters written by them to friends and relatives from the combat zone would reveal views more diverse than implied by my gratuitous invocation of their opinion. The value of nonviolence simply raises questions about exaggerated portrayals of human acceptance of violence. In sum, decontamination of the narrative would be better served if we struck out the whole last paragraph of chapter 10.

Implications for Empirical Re-Analysis

Reconsideration of the empirical proposition-building chapter of *The Korean Decision* from a nonviolent value position produces a disquieting sense that the original analysis is somehow truncated, stunted, and cut off from lucid engagement with the central problem of the Korean decision: why violence emerged and why it was responded to in kind. Instead the analysis is first diffusely devoted to the effects of "crisis" as an independent variable upon organizational, informational, and motivational aspects of decision-making processes.

The primary emphasis in the original analysis was to take "crisis" as an independent variable and to treat "organization," "information," "values," "internal setting," and "external setting" as dependent variables. ¹³ Secondarily, I combined all these variables in a set of propositional statements that described four decisional "stages" that characterized response to crisis in

the Korean case.¹⁴ Interestingly, the word "violence" did not appear in this analysis: words such as "positive response" and "costly commitment" were used instead.

Since a violent or nonviolent outcome was not the primary focus of attention, I finally concentrated overall explanatory analysis of the Korean decision upon its most outstanding processual characteristic: it was a "high consensus decision." Thus:

The stronger the organizational leadership, the less the variability in decisional unit membership, the more the shared learning of unit members with respect to the issue for decision, and the less tolerable the decision delay—the less the variability of information and values supplied from within the unit, the less the articulation of alternative courses of action, and the greater the probability of single courses of action that are anticipated to win leader approval. ¹⁵

Combining the initial interest in crisis effects upon decision-making variables with the secondary interest in a high consensus outcome, the overall logic of the original Korean decision analysis can be summarized as: crisis affects decision-making variables that produce high or low consensus outcomes.

If an explicit concern for violence is introduced into the analysis, however, we obtain the following pattern of analysis: crisis affects decision-making variables that produce violent or nonviolent outcomes. From a nonviolent perspective we are challenged to focus attention more sharply upon the substantive content of crisis decisions. The Korean case thus needs to be perceived not only as an example of a "high consensus decision," but also as a "violence-accepting decision."

A complex propositional statement to sum up the violence-accepting aspect of the Korean decision may now be added to the text¹⁶ as follows:

The more the organizational influence of a violence-accepting leader, the more the decisional participation of members skilled in and accepting of violence, the more the past satisfaction with participation in violence, the greater the availability of instruments of violence, the greater the confidence in overall weapons superiority, the less the anticipated counterviolence, the greater the social acceptance of violence, the less the salience of

¹⁴Ibid., pp. 318-21.

¹⁵ Ibid., p. 321; emphasis in original.

¹⁶Insert after propositions (i.e., after line 22) in ibid., p. 321.

nonviolent alternatives, and the greater the belief that competing decision makers are motivated by a similar logic—the greater the probability of violent decisional responses to crisis in international politics.

At the end of the original chapter devoted to empirical analysis of the Korean decision, I briefly introduced three propositions intended to "link properties of decisions with problems of their execution by large-scale governmental organizations." These predicted a gap between intent and performance if the decision content is ambiguous; a link between the seriousness of expected counteraction and the degree of decisional specificity; and a tendency to delegate command and control functions to field commanders where severe counteraction is not anticipated. Again, the word "violence" did not appear in any of the statements.

Approaching the same problem from a non-violent perspective, it appears that decisions based upon the assumption of justified violence are apt to be ambiguous; that the acceptance of violence tends to preclude attention to complementary and possibly supplantive nonviolent coping alternatives; and that violence-based decisions are likely to be permissive of initial commander autonomy in a violent direction.

While a proviolent value bias in empirical analysis seems not to have repressed evidence of nonviolent alternatives considered by the decision makers, since they all seemed satisfied with violence, this does not mean that it had no analytical effect. For example, no effort was made (a) to develop nonviolent alternatives with which the decisions could be compared, (b) to question the degree to which each decision maker was committed to violence, and (c) to probe through interviews the existence of latent nonviolent alternatives or to obtain a more detailed understanding of why such alternatives were considered infeasible.

Implications for Normative Re-Analysis

The Korean Decision contains a chapter devoted to normative evaluation of the decision. Four different approaches were taken. First, a normative inventory of the case materials reviewed the judgments of the decision makers, domestic critics, external allies, and external critics. Second, some common criteria of international political evaluation were explored. The Korean example was found to fall in the category of good decisions; i.e., decisions

that pursued good ends by just means in a flexible, realistic way with beneficial long-range effects. Third, the conditional approbation of violence by major world religions was recalled and a pacifist perspective was entertained and dismissed. Finally I presented my own judgment

It is the latter which I wish to revise here. I now believe that the American decision to fight in Korea is not a decision worthy of moral justification by a social scientist, any more than that which produced the North Korean attack, that the American decision vastly increased the loss of life in Korea including later many Chinese, that confirmation of the decision by congressional resolution which I originally recommended would not have made it more just even if politically more tenable, and that the long-range effects of the Korean decision have not been beneficial for Korea as a whole or for international political life. The Korean decision did not realistically make international political violence less likely, as illustrated by the case of Vietnam, to which official American satisfaction with the Korean decision undoubtedly contributed. The international militarization to which the Korean War contributed did not make world peace more secure; witness continuing arms races, increased anxiety over American military security, and nuclear weapons proliferation.

The United States' decision to engage in violence in Korea, not "resist aggression" as in the title of a 1958 article jointly written by Richard C. Snyder and myself, ¹⁸ has contributed to the unprecedented militarization of both parts of Korea. In 1950, there were 286,091 men under arms in Korea (151,091, south; 135,000, north), ¹⁹ out of a total population of about 29,715,000 (20,167,000, south; 9,548,000, north)—or one soldier for every 104 persons. By 1975 this had risen to 1,092,000 men under arms (625,000, south; 467,000,

¹⁸ The "resist aggression" characterization, rather than "to intervene," was strongly advocated by Secretary of State Dean G. Acheson and accepted by me in Richard C. Snyder and Glenn D. Paige, "The United States Decision to Resist Aggression in Korea: The Application of an Analytical Scheme," Administrative Science Quarterly, 3 (December 1958), 341–78. One problem with the "resist aggression" formula is that it implies total evil of the aggressor and that only military measures offer hope of successful resistance.

¹⁹ Roy E. Appleman, South to the Naktong, North to the Yalu (June-November 1950) (Washington, D.C.: Office of the Chief of Military History, Department of the Army, 1970), pp. 10-11.

north)²⁰ out of a total of 50,350,000—or one soldier for every 46 persons. This increase was combined with vastly more destructive weapons, and with the high likelihood that both contending Korean governments will achieve independent nuclear weapons capabilities in the near future.

The threat of violence in Korea, based upon the ultimate acceptance of the possibility of a violent "solution" by both sides and their international supporters, has legitimized the suppression of political freedom in both parts of Korea, a value that the original "realistic" commitment to violence was intended to protect and enhance.

Thus my own judgment is that the Korean decision does not merit praise as a contribution to world peace and freedom. It should rather be judged as a stimulus to search for nonviolent alternatives to resolve human conflicts and to realize human aspirations then, now, and in the future.

Reassessment of Action Implications

In the final chapter of *The Korean Decision*, I tried to derive some lessons from the Korean case to guide future American policy makers in crisis situations. All based upon acceptance of violence, these suggestions were: not to underestimate potential enemy military strength; to be receptive to friendly critics so that "force" might be employed less dangerously and with more political support; and to set clear limits on the employment of force so that it might be employed with surgical precision.

From a nonviolent perspective, the best "lesson" to be learned from the Korean decision is that American policy makers should be encouraged to experiment with the assumption that American violence will not be applied in international politics, that American military supplies will not be provided to support the violence of others, and that policy makers should work positively toward nonviolent resolution of the grave domestic and international conflicts that threaten human dignity, economic decency, physical survival, and world peace.

A multinational nonviolent approach to pre-1950 conditions in Korea and to coping with violence if it erupted there would by no means imply that only military measures would be appropriate or effective, either in the short or long run. An extraordinarily versatile combination of political, economic, social, cultural, and communications means might be employed to prevent, resist, limit, and defuse armed aggression including physical resistance to the point of death with intent not to kill but to touch the hearts of the aggressors. A nonviolent policy approach to the Korean decision and its preconditions does not therefore imply passive acceptance of violence but rather more creatively vigorous efforts to end and avoid lethal conflicts than a violence-accepting approach would require.

Analytically we need to add to the repertoire of skills in decision-making analysis the caution that the more the agreement of the scientist with the values of the decision makers, the more limited the likely development of evidence and analysis that would support alternative courses of action. A collegial check upon such biases would be constant encouragement of value diversity among scientists. An individual check would be to prepare comparative actor-analyst value profiles and to seek deliberately to extend the range of congruence-predicted analysis of alternatives.

In conclusion, *The Korean Decision* needs to be re-examined not as a text on how to handle violence better but rather as a challenge to how to avoid it in the first place. If violence does occur, then the best crisis advice is to limit it, compartmentalize it, diminish it, weaken it, calm it, cool it, find alternatives to it, seek rewards to end it²¹—not to increase it, fuel it, supply it, justify it, praise it.

In an age of unprecedented potential for violence, the supreme task of political science becomes the creation and application of non-violent knowledge. It will benefit us little if our continued "realistic" acceptance and justification of political violence prevents us from creating alternatives to it. The Korean Decision thus needs to be reanalyzed as a contribution to this task and not allowed to stand as a scientific apology for the future continuation and possibly irreversible escalation of violence in international political life.

The original dedication of *The Korean Decision* was "To all who died in the Korean War, and to all who make and study political decisions." To this should now be added, "for a nonviolent future."

²⁰ International Institute for Strategic Studies, *The Military Balance*, 1975–1976 (London: The International Institute for Strategic Studies, 1975), p. 56.

²¹The remarkable experiment by Tsai, in which after 700 trials he got a cat and a rat to cooperate in obtaining food without a coercive security barrier between them, can serve as a stimulus to constructive thought along these lines. See Loh Seng Tsai, "Peace and Cooperation Among Natural Enemies: Educating a Rat-killing Cat to Cooperate with a Hooded Rat," Acta Psychologia Taiwanica, 3 (March 1963), 1-5.

BOOK REVIEWS

Political Theory and Methodology

Comparative Social Research: Methodological Problems and Strategies. Edited by Michael Armer and Allen D. Grimshaw. (New York: John Wiley, 1973. Pp. 473. \$17.95.)

The book is a collection of papers from an assemblage of social psychologists, methodologists, and other sociology subfield specialists who were invited to think about the methodological implications of their subject. The editors included the salient points recorded from discussions following the paper presentations; though some of the comments yielded further insights, most of the oral comments would have been better left unpreserved. Furthermore, the standard complaint about large edited collections-uneven quality and lack of coherence-is appropriate. However the collection does represent a significant effort on the part of sociologists to address the issues which confront comparative (cross-national) politics as well as social research specialists.

An atmosphere of déjà vu about the probability (possibility?) of solutions to the difficulties all comparative research specialists face, from concept equivalence to measurement and data analysis strategies, pervades the book—"All of sociology is a tough business.... Let those who cannot tolerate the uncertainties, the ambiguities, and the high probability of failure pursue one of the simpler sciences" (Hill, p. 464). The volume is an example of the burgeoning interest in comparative research and the need to grapple with the research-related issues. Why then the dejà vu? Before answering the question, I shall discuss the papers.

The papers may be grouped according to subject emphasis and methodological concern. Topics range from community power, illegitimacy, childrens' acquisition of language, the impact of law on social change to individual level studies of modernity, class consciousness, and national development. Methodological problems addressed include what is distinctive about comparative sociology and the comparative method, the sociology of cross-cultural research (field work interviewing, research acceptance by citizens of the country being studied, the assumptions brought to comparative research by the researcher and the researched), concept formation and measurement

issues, unit and level of analysis questions associated with ecological analysis.

If one views the volume as an effort to summarize the lessons a group of able comparative research specialists have learned from experience, it is a worthwhile purchase. Moreover, several papers are worthwhile contributions taken alone. Turk's discussion of the use of historical legal materials for the study of social change is a welcome addition. Armer and Schnaiberg critically evaluate the Inkeles individual modernity scale through a well-designed replication procedure. And Schwirian and Finsterbusch present a useful discussion of comparative ecological analysis strategies. However, one is no clearer about what is distinctively comparative sociology or what comprises the comparative sociological method after than before reading the book. We may address the feeling of pessimism which pervades the collection by drawing two distinctions.

The distinctions are between theory and method in social inquiry and strategy versus method in comparative (cross-national) research. The distinction between method and theory must be insisted upon because much of the confusion surrounding which method, technique, or data base-in combination with which unit and level of analysis) would fade if we had a theory of social change. By theory I mean models or systems of various degrees of completeness, parsimony, capable of generating specific-point predictions about variable states in the sociopolitical process of change. The absence of such a theory in social inquiry leads to the apparently endless debate over method which I take to encompass all steps in the logic of inquiry (the traditional set of research design steps or reconstructed logic). This is especially so in comparative research because of dilemmas suggested by the justifications usually offered for comparative research. Hill states that comparativists desire to examine a problem in a number of societies "to evaluate the importance of cultural or societal heterogeneity as an explanatory variable..." because they are either searching for universal laws or checking out the limits of hypotheses developed largely in the American research context. However, underlying this statement is a problem which highlights the issues comparativists face. To assume cultural or societal heterogeneity is to

assume a continuum or a process (presumably developmental) within which particular societies, or parts thereof, may be placed. Thus, unfortunately, in the absence of a theory of social change it is difficult to do comparative research because we have no way of knowing whether we have standardized comparative cases (Walton, pp. 173-188) or equivalent units in comparative research, i.e., we do not know where to place the units to be compared on the process or societal heterogeneity continuum. As it is we generate cross-sectionally based correlations in comparative ecological research (Finsterbusch, pp. 417-46) which lead nowhere because, in the absence of hypothetico-deductive theory, such work can only assume the stability among and between the independent and dependent variable systems under study, cross time. It is rare that this assumption is warranted. Cross time analysis (Johnson and Cutright, pp. 377-408) does not solve the issue either because in the absence of parameter estimates provided by a theory, the investigator is still unable to make point predictive estimates about the level and direction of R's cross time; moreover, there are no guides for predicting the amount of time needed for changes in relationships to occur.

The above basic epistemological point leads to the distinction between strategy and method in comparative research. The reason comparative social inquiry "exhibits very little by way of distinctive and rigorous methods" (Walton, p. 174) is because the claim that there is a distinctively comparative (cross-national) method is unwarranted. This collection of papers, like others of its type, comprises a set of more or less useful strategy suggestions about the way specific methods might be combined to attack the questions comparativists study. The reasons strategy discussion becomes confusing and contentious (as witnessed in the preserved oral remarks in this book) is because it falls in the "context of discovery" column where tacit level assumptions, images which condition what investigators think are appropriate questions, are dominant.

However, I do not wish to leave the impression that the argument for comparative research is not sound. Several of the authors suggest the limitations of system-specific (within system) studies. For, by inference, generalizations from these (largely American) reports must be regarded at least as cautiously as comparative research-based studies because of the Hill quotation and the point about our inability to place nation-states or sub-units thereof on the societal heterogeneity continuum or process of change. Unless we are satisfied that no further

social change will occur, we are not entitled to rest comfortably with system-specific findings—if further change occurs these typically unstable variable systems may change significantly, even reverse direction.

Read with the distinctions made here, the confusion exhibited in the papers over what unit of analysis is appropriate in comparative research, how explanations may be evaluated and compared, what, in fact, is meant by comparative methodology, is not only understandable but probably inevitable. However I know of no single volume in comparative politics which addresses the range of issues discussed in this collection and thus reiterate that it is a useful substitute volume which examines virtually all the problems that anyone dedicated to comparative research faces.

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Capitalism and the Permissive Society. By Samuel Brittan. (Atlantic Highlands, N.J.: Humanities Press, 1973. Pp. 397. \$12.00.)

It is now obvious, both to the outside world and the British intellectual elite itself, that British society is close to the ultimate throes of social collapse. National adversity, however, has produced a few concerned social thinkers who have begun to re-think the conventional wisdom of the British way of life from fundamentals. One such is Samuel Brittan. Mr. Brittan, not surprisingly in terms of British society, though it would be so in the United States, is not an academic political scientist or economist, but a financial journalist. As a social philosopher, however, he has the advantage of a first-class Cambridge economics degree, during which he was taught by both Joan Robinson and Milton Friedman, and a scholarly bent which has made him deeply versed in the philosophy of Hayek and the "Vienna School" of liberal philosophers,

The present book is for the most part a record of work in progress and minor variations on a major theme, in the form of short pieces on British politics and political personalities. These pieces are illuminating and penetrating; but they stop short of tackling head-on the fundamental problem in political science and sociology that the British case increasingly raises—namely, why political debate in Britain is couched in language and imagery that consistently avoid the rational treatment of policy problems (especially Britain's international relations). There comes a time when the appeal to

rationality must be recognized to be pointless by those who pin their faith on belief in the ultimate rationality of the democratic process. Mr. Brittan still has faith—naturally enough, for anyone born in and committed to any country of historical tradition and pride—but one occasionally senses a hint that the author is uneasily aware that no one is willing to listen.

This has, indeed, been the reaction in Britain to the main essay, "A Restatement of Economic Liberalism," which accounts for the first 150 pages of the book. This is a very carefully worked out and balanced essay, notable for its willingness to suffer fools patiently (if not gladly) and its philosophical sweep, as good as can be found in the way of a relevant modern restatement and extension of classical liberal ideas. If anything, Brittan concedes too much to the wilder superficial critics of capitalism for American tastes; but one has to remember that the universities of privilege in the United Kingdom stifle genuine intellectual enquiry and debate to an extent impossible in the political and cultural polycentricity of the United States. Nevertheless, this has not saved Brittan from savage reviewer attacks based on refusal to follow his arguments, and depiction of him as a mere beautician lackey of "the ugly face of capitalism."

There is a growing body of observers who fear that Britain today is a picture of what the United States is likely to become tomorrow. Few, however, if any, have extended their gloomy prediction from the economics into the politics of the welfare state. Brittan's work suggests a number of themes worth exploring in the context of contemporary American democracy.

HARRY G. JOHNSON

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Coalition Theories: A Logical and Empirical Critique. By Eric C. Browne. (Sage Professional Papers in Comparative Politics, Vol. 4. Sage Publications: Beverly Hills, Calif.: 1973. Pp. 95. \$3.00, paper.)

Coalition for what? What is the payoff? These are the questions that continue to plague decision theorists. This short, tightly written monograph examines the assumptions and logic of coalition models by William H. Riker, William A. Gamson, and Michael A. Leiserson and finds important weaknesses.

In order to compare the models, Browne tested them against coalitions actually formed in 13 parliamentary democracies over a quarter

of a century. The results, although quite a lot better than chance, do not give "us a very good chance of predicting an actual formation" (p. 26). The Gamson and Riker models were especially weak. Analysis by individual nations offered no encouragement for the predictive value of coalition theory.

Why do none of the models allow us to predict? Browne concludes, using some empirical evidence and a tightly reasoned argument, that the model builders depart from incorrect assumptions. In particular, he believes, "the value of belonging to a particular winning coalition is not related in any necessary way to its size alone" (p. 72).

Riker's famous "size principle" is irrelevant in most cases. Why? Because "in most political coalition games there is no payoff waiting to be divided" (p. 73). Browne therefore seeks to answer the question of why, then, coalitions are formed at all. Relying upon some recently published theoretical work (all of it in the 1970s) especially that of the Netherlander, Anthony De Swaan, he argues that players seek to maximize their coalitional benefits by minimizing the policy or ideological differences among partners in the winning coalition. If the coalition is not to be minimal, there is a problem of determining how large it should be, for the larger the coalition the greater are what Charles Press and I have called "dissonance" costs. (American Political Science Review, 62 [June 1968], 556-63). Browne offers suggestions for calculating optimal coalition size and membership, but the problems are formidable. In effect the goal is one of finding the winning coalition having the smallest dissonance costs.

Even if the Browne-DeSwann approach can be refined to the extent of providing satisfying predictability, it is still limited in concept to cabinet formations in parliamentary democracies. The ad hoc coalitions that occur in the United States Congress, for example relative to "revenue sharing" (positive) or foreign aid (blocking), could not be predicted by their method, in part precisely because while policy distances are small, ideological distances are great—members vote the same direction for different reasons. Alas, no general coalition theory is yet in sight.

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Ferdinand Toennies: A New Evaluation. Essays and Documents. Edited and with an introduction by Werner J. Cahnman. (Leiden: E. J. Brill, 1973. Pp. viii + 302. 64 guilders.)

This recent volume aims at a new evaluation of the sociology of Ferdinand Toennies through the combination of secondary analytical essays with newly translated excerpts from that author's work. The secondary evaluative essays include the following: (1) two older pieces (Albert Salomon's essay in memoriam and Talcott Parsons' note on the concepts of Gemeinschaft and Gessellschaft); (2) a series of new essays on various facets of Toennies' work (Albert and Gillian Gollin on the study of public opinion in Toennies, A. Oberschall on his empirical sociology, recent afterthoughts by Parsons on Gemeinschaft and Gesellschaft, p. Etzkorn on Toennies as a formal sociologist, and three wide ranging systematic treatments of Toennies' sociological system by E. G. Jacoby, R. Heberle, and W. Cahnman); and (3) an introductory statement by the editor locating the various contributions to the volume. Here, the essays by Cahnman on "Toennies and Social Change" and Jacoby on "Three Aspects of the Sociology of Toennies" are especially acute and discerning additions to our understanding of that German pioneer's wider contemporary relevance.

The newly translated material is accompanied by helpful commentary and notes by the editor and focuses on Toennies' relation to the work of Spencer, Marx, Durkheim, and Max Weber. An added bonus for the reader is the inclusion of a new translation of Emile Durkheim's own 1889 review of Gemeinschaft und Gesellschaft, along with Toennies' comments on that review and his own reviews of Durkheim's Division of Labor and Rules of Sociological Method.

Undoubtedly the most rewarding part of this book is the section of new translations. To read Toennies' modifications and criticisms of other pioneering figures in the sociological tradition is to understand more readily key differences between these masters as they saw matters themselves. The sections on Toennies' relations to Marx, Durkheim, and Weber are immensely helpful in this respect. Indeed, Toennies' criticisms and extensions of Marx, carrying many of Marx's assumptions and perspectives into the light of more systematic sociological conceptualization and historical empirical evidence, can be read with profit even today for their clarity, scope, and depth and will doubtless be preferred by many sociologists and social scientists in general to some more recent yet also more elusive treatments of that author. The wide horizons and conceptual penetration of Toennies' remarks will appear even more unusual when it is remembered that they appeared as early as 1908 (and later in more extensive form in 1921), well before more recent interpretations based on a fuller recovery of Marx's unpublished manuscripts. One is allowed to wonder whether the integration of Marx's theoretical and empirical contributions into the mainstreams of sociology might not have proceeded more smoothly, especially in America, had earlier sociologists been aware of Toennies' neglected contributions in this area.

More than a few perplexing questions arise from the comparison of Toennies with Max Weber, but this volume at least begins to provide some indispensable beginning materials for their resolution. Only three examples can be cited here. Toennies' remarks in this volume on Weber's Protestant Ethic thesis highlight the distinctive stresses of these two writers in their approaches to historical sociological matters. Indeed, Weber's more comparative historical differential approach to the use of ideal typical conceptions of the varieties of capitalism contrasts with Toennies' own stress on the "common characteristics of all kinds of capitalism" (p. 265; his italics). Also, the attempt by Toennies to see the rise of modern capitalism primarily in the light of his notions of Gesellschaft and rational will contrasts with Weber's need to distinguish more carefully among the various historical roots and typical constellations of the notions and processes of rationalism, rationality, and rationalization and to see his own study of the impact of ascetic rational Protestantism as one part of a wider understanding of the series of phases in the overall process of rationalization within the Western European civilizational setting. Finally, Toennies' recourse to the concept of reciprocity (p. 268) to help solve the perplexing problems of the relative degrees of causal efficacy of "spiritual" and "real" factors is frequently combined with an ultimate appeal to "economic factors" (social class, occupational activity, productive technology) as the key causal agencies. This volume sheds a good deal of light on these (and a number of other) differences between Weber and Toennies as well as between Toennies and other classical writers considered in these pages.

Despite a certain degree of thematic repetition, especially in the secondary essays on Toennies, this collection is a helpful addition to the growing literature on a sociological pathfinder whose importance is bound to increase as sociologists today try to map the wider dimensions of a more contemporaneously relevant systematic sociology of a larger historical range. As students of the history of social theory move more rapidly toward a fuller reappropriation of the widest perspectives and strands of analysis available in our "collective memory," volumes of this sort will hopefully appear more rather than less frequently.

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From Rousseau to Lenin: Studies in Ideology and Society. By Lucio Colletti. Translated by John Merrington and Judith White. (New York: Monthly Review Press, 1972. Pp. 240. \$8.50.)

Some potential readers of this volume could be misled, as I was, by the title, The title suggested to me a coherent, systematic study of a series of social theorists, beginning with Rousseau and ending with Lenin. In fact, however, the book consists simply of a number of essays by a leading Italian Marxist philosopher, heretofore unavailable in English translation, and these essays vary widely in theme, style, method, length, and quality.

The low point in the book is reached in the first essay, "Marxism as a Sociology," which is an example of Marxology—perhaps "Marxophilia" is a better word—at its worst. We find here all the marks of solemn Marxist exegesis carried to an extreme. The seminal texts, especially Capital, are quoted with reverence. The prose is dense and obscure. Innumerable words are italicized for no apparent reason. The author leaps from one line of analysis—"Darwin and Hegel"—to another—"Weber and Aspects of Contemporary Bourgeois Sociology"—without establishing a clear connection between both themes.

If one reads only this initial essay, one might get the impression that Colletti is simply a hack. One might get the same impression from the final two essays in the book, "Lenin's State and Revolution" and "Marxism: Science or Revolution?" Both of these brief essays are long on assertion and short on argument.

But Colletti is not a hack. There is plenty of meat between the slices of soggy bread. The essay on "Bernstein and the Marxism of the Second International," the longest in the book, is impressive as a concise account of the pre-World War I controversy over revisionism, illuminating in its analysis of the underlying issues of that controversy, and provocative in

its bearing on current disputes among Marxian socialists. Not surprisingly, this essay has a good deal to say about the heresies of Eduard Bernstein. But it is by no means an anti-Bernstein polemic. Indeed, Colletti maintains that Bernstein was in some ways more perceptive than Engels and the "orthodox" Marxists, especially Kautsky. But the interest of the essay does not lie primarily in its judiciousness but in its boldness. For one thing, Colletti maintains that Bernstein and Kautsky shared a (mistaken) view that Marx thought it possible to distinguish between (nonideational) economic phenomena and all other phenomena. For another, he argues here, as in other essays in the volume, that "dialectical materialism" is both unfaithful to Marx and intellectually indefensible. And for another, he offers a thoughtful and innovative interpretation of Marx's theory of value, which focuses especially on the notion of "abstract labour."

Two lengthy essays, "Rousseau as a Critic of Civil Society" and "Mandeville, Rousseau, and Smith," focus on the political theory of Rousseau. As Colletti himself acknowledges, he fails to integrate fully the twofold task of constructing a critical bibliography of recent writings on Rousseau and critically examining Rousseau's political theory. Accordingly, a number of his observations about Rousseau's political philosophy are hardly groundbreaking (e.g., that Rousseau did not conceive of the state of nature as an actual historical condition and that he did not advocate a retreat from civilization to savagery). Nevertheless, these essays are exceptionally interesting, at least from the standpoint of intellectual history. For example, Colletti provides some striking evidence and argument-and also a fascinating commentary-concerning Rousseau's influence on Adam Smith. And he advances the bold thesis that "revolutionary 'political' theory, as it has developed since Rousseau, is already foreshadowed and contained in The Social Contract" (p. 185). Unfortunately, he does not go on to say whether or not, or why, a satisfactory revolutionary political theory should, or could, advance beyond Rousseau.

By far the most provocative essay in this collection is the one entitled "From Hegel to Marcuse." The thesis of the piece—that Marcuse's writings are not those of a Marxist but of a Young Hegelian—is not new. What is new is the care with which the thesis is developed and the persuasive argument that the originator of "dialectical materialism" was Hegel himself. I find Colletti's critique of Marcuse compelling, as far as it goes. Its shortcoming is that it fails to address—unlike Marcuse, however inade-

quately—the question of whether or not the industrial working class is a plausible agent of revolutionary social change.

This book is so uneven that I find it impossible to give a general assessment. At its best it is very, very good, and at its worst it is horrid.

T. C. POCKLINGTON

University of Alberta

Self-Evident Truths: Being a Discourse on the Origins & Development of the First Principles of American Government—Popular Sovereignth, Natural Rights, and Balance & Separation of Powers. By Paul Conkin. (Bloomington and London: Indiana University Press, 1974. Pp. xi + 196. \$7.95, cloth; Midland Paperback, \$3.25.)

This is an ambitious book, both in terms of the complexity of the ideas and the range of the writers examined. It is divided into three parts, corresponding to the source, intended purpose, and form of American government. Parts one and two present a separate but parallel treatment of popular sovereignty and natural rights: their origins, their application in the colonial debate with England, and their development in American constitutions. Part three contains a chapter on the relationship between mixed government and separation of powers and another on America's checks and balances, called balanced separation. While these political ideas have a "beguiling personal appeal" to the author, he cannot embrace them fully. "It is difficult for me to believe that any normative theory is universal, that all rightminded people have to agree with it, that it fits all contexts and all societies" (p. x). It might be difficult for others to believe that so democratic and inflexible a test is appropriate for assessing political thought. The ambitious task and the author's posture toward thought may have affected the accuracy and thoroughness of his exposition, especially regarding religion and property.

Conkin draws on two jurists, Johannes Althusius and Enrich de Vattel, for the origins of popular sovereignty. Althusius wrote about a religious polity whose object was a "holy" as well as a "just, comfortable, and happy" life (p. 83), but Conkin is more interested in his distinction between the residual sovereignty of the people and government. The fully developed concept of popular sovereignty, found in Vattel, includes a sovereign people, formed by consent, establishing a form of government, and

retaining the right to change the fundamental law (pp. 25-26). Conkin later argues that popular sovereignty requires universal manhood suffrage for constitutional decisions but not for regular elections (p. 60).

Indicative of his legal approach to the subject, Conkin suggests that Althusius' distinction between sovereignty and government, which permits coordinate governments, could have resolved the colonists' conflict with England and would have prevented "a hundred years of foolish drivel about "divided sovereignty" in America (pp. 38-39). Then, in his discussion of the Federal Constitution's new federalism, Conkin prefers Calhoun's generation-after states rights' explanation to Madison's contemporary nationalism. The question is whether the people of the several states or the people of the United States are the source of constitutional authority. Conkin presents the nationalist interpretation, which he associates with Wilson and Hamilton, but, strangely, he thinks that "moderates such as Madison or Randolph" would have agreed with Calhoun's formulation and "might even have accepted . . . secession" (p. 63). Madison authored the Virginia Plan and argued strenuously for a national negative of the states, popular ratification, and proportional representation in both houses. The cautious Randolph refrained from signing the Constitution but later helped Madison secure Virginia's ratification. The Federal Convention is not Conkin's strong point; three prominent framers, Edmund Randolph, Roger Sherman, and William Paterson, are misnamed John Randolph, John Sherman, and James Patterson (sic) (pp. 63, 66, 131, 151, 178, 209, 210).

Conkin traces the evolution of natural rights from Calvin and the French Huguenots to Locke, who "fitted traditional Christian views of responsible ownership to new economic realities" (p. 95). Christian natural law, based on divine right, apparently evolves into modern natural right. The difficulty with this interpretation is revealed by a consideration of Locke's teaching on property. Locke's natural law conforms to rational self-interest. The only restriction on the accumulation of property in the state of nature is perishability. The invention of money eliminates this restriction by providing for the accumulation of durable wealth. Locke unqualifiedly supports this invention, because it encourages cultivation, and "the increase of lands and the right of employing of them is the great art of government" (Second Treatise, chapter v, section 42). For Conkin, Locke's teaching contains an objective standard for individual need, a disinterested concern for others, and does not "confer the

sanction of nature ... for any monetary wealth" (pp. 96-97). Conkin argues that the only form of property related to natural rights includes what is "needed for sustenance and used productively," as well as "the resultant products of one's labor" (p. 112). This excludes "speculative holdings" and "vehicles of commerce" as well as slavery (p. 112). Having attributed Christian restrictions to Locke's teaching on property, Conkin now celebrates agrarianism for expressing "in a peculiarly secular setting many of the ethical themes of Christianity" (p. 114).

In part three Conkin notes the difficulty of classifying governmental activities by function and that Montesquieu intended a separation and a balance to maintain political liberty. Conkin claims that complete separation "cannot cohere easily with legislative sovereignty"; but, unless a single executive shares the legislative power and possesses the appointing power, legislative supremacy results. Later Conkin notes that the earlier state constitutions achieved a functional separation of powers with weak executives (p. 173). Conkin's "balanced separation" reflects a victory for the strong executive advocates.

In his epilogue Conkin holds Hamilton responsible for the nonpreferred position of property rights today. Americans voted "against a propertied society, eventually paying the price of almost universal economic servility for the glory of national greatness and the solace of abundance" (p. 191). If the right of property comprehends the exercise of one's faculties in the unlimited accumulation of wealth, then we cannot separate our security and prosperity from that right. Almost against his intention, Conkin's book illustrates the difference between a religious polity and a polity limited to securing individual rights. For that reason, and for Conkin's full account of writers from the Protestant Reformation to the Puritans, students of political thought will wish to consult this book.

MURRAY DRY

Middlebury College

The Political Philosophy of Luis de Molina, S.J. (1535–1600). By Frank Bartholomew Costello, S.J. (Spokane: Gonzaga University Press, 1974. Pp. xxviii + 242. \$15.00.)

The Jesuit writer, Luis de Molina, is better known as a theologian than as a political theorist. His position on the relation of free will and divine grace ("Molinism") involved him and his order in a conflict with the Dominicans whose great theologian, Domingo Bañez, had given greater prominence to the infallible action of God's grace than did Molina who wished to emphasize the freedom of the human will and the individual decision to cooperate with God's grace. The controversy lasted until 1608 when it was officially terminated—but not resolved—by papal action.

Molina also wrote a five-volume treatise, De Justitia, which developed out of his lectures on Aquinas' Summa and what he considered to be the inadequacies of its treatment of important moral questions. Father Costello uses the treatise as his primary source, comparing Molina's political theory with that of his contemporaries, analyzing the content of his thought, and—not altogether successfully—attempting to link his political views to his theological position.

Costello gives particular attention to Molina's view of the role of the community in establishing and limiting political authority. Molina shares with contemporary Spanish Jesuit writers the theory that the community transfers political authority to the ruler and can limit it, but unlike the better-known Jesuit, Francisco Suarez, he denies that the community can exercise political power in itself. Costello notes that in contrast to the position adopted a century later by John Locke, Molina never concedes that the individual has the right of execution of the law of nature, which, for Molina, can only be carried out by those acting on behalf of the political community. Here there would have been an opportunity to analyze the relationship of Molina's emphasis on free will and his denial of individual decision making on natural law, but Costello never develops this point. Nor does he draw the obvious parallels with the thought of Molina's English contemporary, Richard Hooker.

The study examines Molina's theories on church and state-and devotes considerable attention to the distinction between the "direct" and "indirect" theories of the temporal power of the papacy. John of Paris, whom Molina read, provides a useful point of comparison and Molina is described as adhering to a less naturalistic view of the state than does John, even granting the church an emergency power to intervene coercively in temporals for spiritual reasons. Costello describes Molina's position as "vacillating" but that is probably not the correct word for a theory which, despite its rejection of the extreme papalists' theory of the direct power of the papacy in temporals, grants unlimited coercive power to the papacy (and

even to bishops) if the good of the church requires it.

The attention which the just war theory has received in recent years makes Molina's analysis of this theme of considerable contemporary interest. It turns out to be a fairly straightforward reworking of the classic Thomistic conditions with some additions to take account of Spanish and Portuguese imperialism. Molina justifies a war which is fought to prevent pagans from impeding the spread of the gospel, a reason that could be and was used for the Spanish conquest. He rejects the conquest of the infidels simply because of their idolatry, barbarity, or vice but allows for enslavement (even of women and noncombatants) as a result of defeat or capture in a just war, and explores the extent to which this justifies or limits the Portuguese slave trade.

In general, Molina emerges from this study as a rather conventional synthesizer of the scholastic moral tradition. He is less original than Vitoria on whom he depends for much of his thinking, and less relevant and influential than Suarez and Bellarmine who followed him. For an introduction to the thought of the Spaniards, Bernice Hamilton's Political Thought in Sixteenth Century Spain (Oxford: Clarendon Press, 1963) is both more comprehensive and more readable than this rather specialized and pedestrian study of a thinker whose real contribution was to theology rather than to political theory.

PAUL E. SIGMUND

Princeton University

Locational Approaches to Power and Conflict. Edited by Kevin R. Cox, David R. Reynolds, and Stein Rokkan. (New York: Sage Publications, Distributed by Halsted Press, 1974. Pp. 345. \$17.50.)

Most political scientists by now realize that they should keep themselves aware of what is going on in other social sciences—aware of new developments in theory, in methods, and in data resources. Attention has been focused by substantial numbers of major scholars on work in sociology and psychology, as reflected in branches of our discipline now devoted to political sociology and political psychology, and on many aspects of economics, as in the field now devoted to problems of collective choice as well as the new political economy. Even if political scientists are unable to keep up with all these new developments, presumably they recognize the importance and potential

contribution of these developments to their own direct concerns.

Relatively little attention, however, has been paid to work in geography. Perhaps this is because we still think of geography as it probably was taught to us in school: heavily concerned with physical characteristics of climate and terrain, making extensive use of maps and, where concerned with societal phenomena, overwhelmingly descriptive rather than theoretical or systematically explanatory. In other words, it does not seem to be a social science.

'Tis pity if 'tis true, because in fact the image is no more correct than an image (still not uncommonly held, however) that the study of politics is primarily descriptive and atheoretic. Not only can geographers learn from us, but there is a lot we can learn from their now very analytical and mathematical discipline, and we can discover a lot through joint enterprises. A number of efforts in recent years, epitomized by several readers in political geography, have attempted to let scientifically oriented geographers know some of the potential contributions lying in wait among political science works. In this volume the editors-two geographers and a sociologist-political scientist-do a little of that, but, more importantly, they try to promote a reverse flow from geography into political science. This is a book that should prove useful to people working in a variety of subfields in our discipline, such as electoral behavior, public opinion, comparative politics, international relations, and of course methodology. As Editor Rokkan notes at the outset, territory, space, and location "have been key concerns in the study of politics since Aristotle" (p. 9)-but they have rarely been addressed with the range of scientific tools on display here.

The papers by Cox and by Cox and Reynolds will hold special interest because of their application to problems of spatial organization of concerns from the public choice literature: externalities and jointness and distribution efficiencies. Although several chapters present useful literature reviews. I found it disappointing that, of 11 chapters, only four represented systematic empirical studies. Of these, to me the most stimulating was Reynolds' application of models of spatial contagion to patterns of election returns. I strongly suspect that various contagion models will prove enlightening for the study of such other phenomena as opinion change and, in comparative and international politics, sequential changes of governments across borders ("coup contagion," as some work has labelled it). Some of the most promising models have been derived from work in epidemiology and public health, where spatial phenomena are surely relevant but often secondary. It seems doubtful to me whether in highly developed societies, where so much communication is by advanced technology rather than face-to-face, spatially oriented contagion models will be the most useful ones. But Reynolds' work, with other variants of contagion models, deserves careful attention.

The remaining papers manage to cover phenomena ranging from urban to international politics. In total, the contributors number nine geographers and four political scientists; there is no point in trying to summarize all the papers, but the reader may consult the book's good introduction for an overview. The volume stems in part from a session at the 1969 Annual Meeting of the American Political Science Association. Although several contributions were written after that, the combination of publication lag and reviewing lag means that this is no longer the place to go to find out what is newest and most exciting at the juncture of political science and geography. But it is a good place to begin.

BRUCE M. RUSSETT

Yale University

Crime, Rape and Gin: Reflections on Contemporary Attitudes to Violence, Pornography and Addiction. By Bernard Crick. (Buffalo, N.Y.: Prometheus Books, 1974. Pp. 96. \$7.95.)

If the success of a book is to be measured by how well the author achieves his stated purpose, then Professor Crick's small volume is quite successful. The aim set forth at the outset is to clarify the problems connected with violence, pornography, and drugs. The central issue in the author's mind is tolerance, and that issue binds together the three "sermons" (the author's own playful designation) that make up the book. Professor Crick's manner is unpretentious and discursive; his observations are often amusing and are informed with thought and common sense.

The views developed might be briefly summarized (although the author carefully refrains from stating grand conclusions) in terms of a twofold doubt—that social order is threatened by violence, pornography, or drugs, and that liberty is threatened by every governmental regulation designed to deal with these problems. Professor Crick is skeptical of the cries of alarm raised both by conservatives who see

contemporary society as disintegrating and by libertarians who discern the onset of totalitarianism in every limit imposed on individuals. This two-edged doubt makes an effective weapon for defending toleration. It serves to hold off both these enemies of toleration who believe that it imperils order and those dangerous friends who would burden it by turning it into an absolute.

It must be asked, however, whether a reviewer is bound to judge a book strictly in terms of the author's stated purpose. That question arises in the present instance because the philosophical principles underlying the social commentary do not seem quite so strong as the commentary itself. The author seems often to make do with common sense in place of theoretical rigor. In his behalf if must be emphasized that he is not trying to formulate a theory of toleration. He does, however, more than once remark that he is a political philosopher and that the clarification he is seeking is of the kind a political philosopher is presumably in a position to provide. Hence it may be appropriate to accord at least brief attention to philosophical underpinnings.

One of the major principles the author brings into play is a strong one, namely, that toleration does not imply approval; on the contrary, it carries with it an obligation to view social behavior with discrimination and with a willingness on occasion to condemn. He insists on distinguishing between toleration and permissiveness. The efficacy of this distinction is suggested in his statement that "those who wish to be their brother's keeper had better really try to do so, to do so themselves with all the trouble, time and care involved, not to agitate for the police and the courts to do it for them" (p. 89).

The author has no doubt that we occasionally have a right to call on the police and the courts, however, and it is in trying to answer the old question "When?" that his basic principles-so far as they are manifest in this bookbegin to seem not quite as clear and consistent as those of a political philosopher should be. Mill's distinction between self-regarding and other-regarding actions is explicitly rejected and the legitimacy of limiting freedom for an individual's own good is affirmed. But when it comes to defining the criterion of intervention. the author does little more than appeal casually to the likely effect of an action on "sociability." Censorship, for example, can be defended only in cases involving a "reasonable, wellfounded likelihood of danger and damage to sociability" (p. 57). But what is sociability? This is not made clear. Yet the term is a very

broad one and it might readily be defined in a way that would render Professor Crick's position, contrary to his own intentions, practically the same as Mill's.

Not only does Professor Crick seem to have in mind no clear principle for deciding when toleration may be limited, he also provides no clear reason why it should be preferred where regulation is not imperative. He seems to favor toleration merely on the commonsense grounds that it is, better not to become involved in telling people what to do unless you have to. The reader gains little sense that toleration is a positive good.

These lacunae are particularly noteworthy in view of the fact that the author identifies himself as a socialist. Is toleration fully compatible with social justice? If not, which is to be preferred? There is no sign that the arguments of Wolff, Moore, and Marcuse, in A Critique of Pure Tolerance, have been seriously considered.

In sum, Crime, Rape and Gin is not in all ways a book one would have expected from a political philosopher; it is, nevertheless, a wise and pleasant book.

GLENN TINDER

University of Massachusetts, Boston

Machiavelli and the Nature of Political Thought. Edited by Martin Fleisher. (New York: Atheneum, 1972. Pp. 307. \$10.00, cloth; \$4.95, paper.)

This book came to me very late to review and I have been sitting on it for a long time, and have needed two reminders to get it out; and probably the person before me just did not do it. Alas, one sees why. Nothing academically has cheered me more in recent years than the formation and fluorishing of the Conference for the Study of Political Thought. But that is not to condone the terrible itch to publish conference proceedings, regardless of coherence and quality. I want to use this review as an excuse to say something about this bad intellectual habit; but first to the book itself.

All but one of the papers printed here were presented at various meetings sponsored by the Conference for Political Thought. Most of them were presented at York University, Toronto, in 1969 to celebrate the five hundredth anniversary of the birth of Machiavelli. The first two essays in the book have nothing to do with Machiavelli at all, Sheldon Wolin's by now famous and much reprinted essay, "Political Theory as a Vocation," and Alan Ryan's "Two Concepts of Politics and Democracy: James and

John Stuart Mill." The "and" in the title of the book turns out to be the disjunctive, not the conjunctive "and." "And if our fellow citizen about to be hung doesn't wish to address all those here assembled in a dying speech, I'd like to say a few words on the tariff question."

Wolin's few words are very good words, about the necessary involvement of political theory in politics, about the fundamental politics of perceiving what we are, what we could and should be doing: not that narrow "realism" which cannot see beyond the end of its own nose, except when it lifts its head in rhetoric to universalize its own local values. It is concerned with questions about "the quality, direction, or fate of public life." What is interesting is that most of the debate that followed the publication of Wolin's essay, in this journal and elsewhere, has taken the form of the old empiricist and behavioral battle against both the importance of ideas as explanatory factors and as rational moral critique. But the more interesting thing about Wolin's argument is now it touches the New Left. They are so right to demand relevance to human purposes rather than to the interest groups of the Fifteenth Precinct (or the real attempt, as Nixon tried in America and Wilson tried in Great Britain, to treat a nation as if it is a precinct or a constituency), but are so wrong, he shows, to think that any one theory is always relevant. The interplay between theory and practice is much more complicated than that. Political theory is not specific to a single social context: the range of theory is the whole range of memories and possibilities contained in our historical human culture. Putting things in a Marxist, a Christian, or a positivist context is all very well, but the contexts are only selfcontained by the assumptions of the theory. What Wolin is really saying, it seems to me, is that political theory is a philosophical and historical critique of political doctrine. As in Oakeshott's thought, the process is continual and without end; but unlike Oakeshott, Wolin has no contempt for doctrines, only a humane awareness that all are attempted, at some time or other, but that none prove final.

Alan Ryan's essay on the two Mills is interesting and important (but what is it doing here?). It shows how far John Stuart was able to break from the strict utilitarian and market economy philosophy of his father, J. S. Mill, the libertarian, the "free spirit," the putative social democrat and communitarian. Within liberalism, there is not simply (as Louis Hartz used to argue so eloquently) one tradition, but rather two—"not obviously compatible" as Ryan modestly concludes.

The five essays on Machiavelli, with some snippets of conference commentary, range widely. They are oddly overly-specialized for such an anniversary assessment; none raises quite those central issues of political theory that are found in Wolin and Ryan's essays, although it would have readily possible to do so, even without committing oneself, simply to show how and why Machiavelli has appeared central to so many people and stirred up so many diverse reactions—as Isaiah Berlin did so cleverly in his "The Originality of Machievalli" in the celebratory studies edited by Myron P. Gilmore, Studies on Machiavelli (Sansoni, Florence, 1972).

Fleisher argues in "A Passion for Politics" that Machiavelli sees reason as solely concerned with technical means to pursue political life and struggle, not with ends. Perhaps, but if so, then he could be dismissed more easily. His implicit values are, however, much more interesting. Why does he say that evil actions may be needed to preserve the state? What sort of state was he thinking of? Certainly not something that justified itself simply and circularly in terms of power to retain power. Everything we know about the man and his life points in a humanist direction. To unmask hidden assumptions is not always to discredit, often the contrary.

If Sheldon Wolin can be thought to have played the broadening game, then J. G. A. Pococ, plays the narrowing game of political thought. And in any case the same chase appears more profoundly and at greater length in his recently published The Machiavellian Movement. Machiavelli is set firmly in his context, and no more than a rather eccentric late medieval thinker emerges. Pocock applies to politics, quite explicitly, the Kuhnian notion of a scientific paradigm, and then explores what paradigmic structures of political thought were available to Machiavelli-and reaches his narrowing conclusions by walking that reverse spiral. But if one doesn't accept that there are such complete and exclusive paradigms in political thought, if such "paradigms" are more likely to be symbolic than structural, then this is a wild goose chase.

Robert Orr, somewhat an Oakeshott disciple, would also wish to see ideas set firmly in their context, but has no such rigid ideas about context as Pocock. His essay on "The Time Motif in Machiavelli" (also published elsewhere) is excellent. Machiavellian politics emerges from this as the politics of adaptability and flexibility, spanning the whole range of time from the instant reaction to attack, to the hope for personal immortality through fame. Harvey

Mansfield writes on Machiavelli's Florentine Histories, a very stimulating, original but unsound conference paper: "obviously a stage in thinking about a wider problem, just the thing for a conference but not ready for publication. Finally, there is an essay by Neal Wood on "The Value of Asocial Sociability" which traces the theme of the positive value of social conflict from Machiavelli to Sidney, Montesquieu, and finally (if arguably) to Kant.

What I really want to say, however, is that perhaps rising printing costs and the state of the economy can save us from ourselves, that conferences can return to their pristine innocence as places for the discussion of ideas and. if for the presentation of long papers at all, then only of work in progress, not papers prepared for publication of which, perhaps, a few lines will be changed before publication. I even attended a conference a few years ago on a special theme of political thought that really needed thinking about in which it was simply assumed, not even said in the invitations or agenda, that papers would be published; and therefore the knowing ones brought papers ready prepared, in a technical sense, for publication, and defended them-which was the end of the free play of critical thought at the highest level about important matters, the end of the very thing for which Sheldon Wolin argues. Political thought is essentially an oral, critical, dialectic, discussing tradition and activity. Publications should be fewer and far more considered. Conference organizers should not seek for monuments, and conference sponsors should always-I suggest-make it a condition "that the grantees will think and, if they must have conference papers as a trigger to discussion, will not publish them together; and if they publish them at all then not in a form recognizably the same as when first delivered, otherwise we will judge the funded activity to have been a waste of time and will sue for our money back."

BERNARD CRICK

Birkbeck College, University of London

Can Social Science Help Resolve National Problems: Welfare, a Case in Point. By Leonard Goodwin. (New York: The Free Press, 1975. Pp. x + 214. \$8.95.)

This book begins with the premise that, despite the lip service paid to the notion of making social science more relevant to national problem solving, this has not resulted in major new research or problem solving efforts. Good-

win attributes this deficiency largely to the Galilean-Newtonian approach to science where there is no place for what he calls "the reflexive quality of human beings." This reflexive quality is essentially the ability of individuals to become aware of knowledge about themselves and to make self-consciousness an integral part of their approach to science.

Central to Goodwin's critique of contemporary social science methods and the assumptions undergirding these methods is his argument that there is an absence of social science interest in becoming involved in the problem-solving process. Goodwin contends the philosophy of science traditions on which social scientists are reared encourage concern with research techniques and not with understanding social problems which would thereby advance social problem solving. Goodwin devotes a large portion of his argument to the development of a philosophical approach to social research which he calls "experimental social research." Goodwin is not arguing for the application of experimental design principles and methodology to social problem solving. What Goodwin means by "experimental social research" is the involvement of individuals in the design of research efforts directed at social problem solving, a critical examination of their own basic beliefs about the problem, and the formulation of policies and implementation of social programs for the resolution of these problems. This arrangement has already been made, of course, by numerous psychologists, sociologists, and even a few enlightened political scientists.

Goodwin's earlier research on work orientations of the welfare poor and middle class demonstrates that policy makers maintain certain assumptions about the nature and cause of the welfare problem. In the current text, he further analyzes these data and contends that the beliefs held by policy makers about the welfare problem lead to their formulating solutions that are congruent with these beliefs but ill-suited to dealing with this social problem. Goodwin's experimental social research approach would overcome this deficiency because it would include the following individuals in the problem solving effort: policy makers (the donors), recipients of services (the clients), and social scientists (the researchers). The interaction of these three groups, Goodwin contends, would lead to greater understanding about the nature and cause of the social problem and therefore result in social policy that was more adequate to the task of addressing the particular problem.

The crux of Goodwin's argument centers on his recommendation that a Public Research Corporation be chartered by the U.S. Congress. This research corporation, using the experimental social research approach, would constitute appropriate client, policy-making, and research groups to address specific social problem areas. Only by employing this strategy does Goodwin feel that social science can help resolve national problems.

Goodwin's book is a thoughtful, if tedious, polemic and suggests this man's genuine concern for making social scientists significant actors in the problem-solving and policy-formulating process. Those who disagree with his arguments and recommendations should give evidence of their concern not by attacking Goodwin, but instead by developing their own thoughts and explicating them for students, scholars, and policy makers.

FRANK P. SCIOLI, JR.

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Handbook of Political Science, Volume 1.
Political Science: Scope and Theory. Edited
by Fred I. Greenstein and Nelson W. Polsby.
(Reading, Mass.: Addison-Wesley, 1975. Pp.
414. \$13.95.)

This is a first-rate book, instructive to those who seek a philosophy of political inquiry or who want to rethink the one that they already have. The book includes five chapters by different authors. Dwight Waldo puts the discipline in historical perspective. J. Donald Moon writes on explanation and interpretation. Dante Germino argues the relevance of the classics of political philosophy. Felix E. Oppenheim discusses central concepts and their explication. And Brian Barry and Douglas W. Rae together analyze the problem of political evaluation.

The book is well conceived. Given the number of contributors, it is surprisingly coherent. As a "handbook," it is better described as reflecting the state of the discipline than as exposing weaknesses or calling attention to needs or breaking new ground; but some new ground gets broken nevertheless, especially in the chapters by Moon and by Barry and Rae. Since the book reflects the state of the discipline, it gives reason for both pride and dismay.

Waldo's "primary method," as he himself says, is "historical-interpretative ... used as a means of understanding contemporary political science in the United States" (pp. 2-3). He sketches relevant developments in Europe from Plato on, and then focuses on American political science in successive periods. The account is

essentially reportorial and rather uncritical, and it cannot help but cover much of the same ground already so well covered in Somit and Tanenhaus, The Development of American Political Science. The account is no doubt an accurate reflection of the state of the discipline in that it takes the state system for granted and raises no question about the tacit assumption of most American political scientists that the discipline is essentially concerned with domestic politics, and that comparisons are to be at the domestic level rather than between the domestic and international levels. This is indicated on the very first page, where the definition of political science that is offered is Easton's-referring to the authoritative allocation of values for a society; it is obvious that Easton in formulating the definition and Waldo in quoting it did not have a world society in mind. International politics gets its first mention, a brief one, in the section on political science since World War II. Neither the International Studies Association nor the International Studies Quarterly gets any mention at all. It is interesting and paradoxical-though I suspect a faithful reflection of the tacit assumptions of most political scientists-that Waldo uses World War I and World War II in marking the periods of the study of political science, yet pays little attention to the question whether the reasons for or causes of such wars are or should be studied. To be sure, everyone knows that international politics is there-but it is politics with a difference. Real politics is domestic, and international politics, when remembered, has to be treated on the side.

Moon focuses on explanation, deliberately leaving to others the treatment of the normative aspects of political science. He analyzes two models of explanation: the naturalistic or scientific model, with its emphasis on generalization based on laws and empirical theories, and the interpretative model, with its emphasis on particular events and on the intentions and beliefs that guide individual actors. He is respectful of both models, each in its place. Moreover, he seeks to synthesize them. He does this by pointing out that the actions of many individuals sometimes combine in such a way as to produce consequences that none of them intend, and that explanations of the unintended consequences may be quasi-causal. The quasicausal explanations rest not on knowledge of specific, individual actors but on knowledge of man-in-general-on a "model of man." The dominant if not the only model now used is the rational choice model, which obviously assumes that people choose rationally and which depends on knowledge of the values and beliefs that guide them in the choices that they make; it depends on knowledge concerning attitudes about ends and means.

Germino describes himself as making a case for the importance of political philosophy. To him this means substantially a case for the classics and a focus on essences: What is man? What is society? What is history? These questions are abstruse. Germino sketches answers given by what he calls theocentric, anthropocentric, and metastatic humanists, but the sketches are necessarily brief, and those relating to the essential nature of society and history are either obscure or indistinct from each other. Moreover, the obscurity of the answer to the question, "What is society?" has a bearing on Germino's later statement that "the primary purpose of a political philosopher is to explore the individual's relationship to society..." (p. 259). He treats the question parochially, despite the contrast he makes between a philosophical and a parochial political science. His "society" is evidently the population of a state. and the population is evidently homogeneous. He ignores the fact that "societies" are sometimes segmented and that politics goes on both among the segments and among the "societies" themselves. Surely the human relationships that count in political philosophy occur not only between the individual and society but occur also within and among groups of a number of kinds-national, racial, linguistic, religious, etc.-and the philosopher should seek out rules concerning the right and the good pertaining to these relationships the world around.

Oppenheim's declared concern is with "the explication of basic concepts." He seeks "to illustrate the more general problems of conceptual analysis." Moreover, he wants to acquaint political scientists with "the relevant trends in analytic philosophy" (p. 284). Starting with a focus on concepts, he speaks in effect of real and nominal definitions, divides the nominal into the stipulative, the explicative, and the reportative, and then limits himself to the explicative-that is, to the clarification of concepts taken over from the language of everyday life. The analysis that follows is insightful and stimulating, but the effort to do several things at once blurs the focus. Questions about the definition of words get mixed up with questions of methodology and epistemology. Are empiricism and behaviorism really methods of explicating concepts? If one emphasizes Verstehen, is the concern primarily with the explication of individual concepts or with the formulation of correct statements? Should one announce in a heading (p. 309) that the focus is on "categorical and comparative concepts" and

then in the first sentence under the heading speak as if the adjectives really refer to methods? What is one to make of an exposition that (1) assumes and asserts (e.g., p. 320) that concepts-as distinct from the statements in which they appear-can be classified as either descriptive or normative; (2) says that legitimacy, for example, is a normative or moral concept, but that it can nevertheless also be used in a descriptive-legal way (pp. 320-21); and (3) says that "normative concepts had best be used only to express the advocacy of some political action or goal..." (p. 327)? The components of the chapter are all significant, and much about them is perceptive and instructive; but the various purposes are pursued in such a way that the clarity and the potential forcefulness of the analysis are impaired.

The chapter by Barry and Rae is notable. Its very inclusion marks a rejection of the view that the purpose in political science is explanation. More affirmatively, it marks the acceptance of the view that political scientists should concern themselves not simply with structures and processes but also with policy outcomes; or, as Barry and Rae say, it marks the acceptance of the view that, along with the study of how decisions are made, political scientists should also take up the question which decision ought to be made. "...[A] withdrawal from engagement with the problems of evaluation is untenable" (p. 337). The question, "What shall we do, and how shall we live?" was not initially directed to political scientists; but political scientists, in their teaching and research, should aim to contribute to the answer nevertheless. The problem is that the chapter leaves one somewhat uncertain about the policy issues that are appropriate to political science and about the criteria for selecting them.

Whatever the issue, "the ultimate ground of evaluation lies in the well-being of individual human beings ..." (p. 390). The consequence for well-being is the touchstone for all political decisions. Barry and Rae find that predicting consequences is often beyond human capacity, and they suggest three simplifying mechanisms: "focal descriptions, duties and obligations, and partial goods." Descriptions are focal when their characterizations are evaluative-e.g., when relationships are indicated between what is described and the public interest, justice, equality, freedom, or democracy. Duties and obligations are indicated, for example, by the French Declaration of the Rights of Man and by declarations of human rights promulgated since World War II. "Partial goods" are goods that are less than general or universal; actors pursuing them accept a division of labor and concern themselves primarily with the good of those with whom they have a special relationship rather than with all human beings.

Barry and Rae do a little more than the other authors to recognize the existence of international politics. They acknowledge that groups as well as individuals have claims that need to be evaluated—claims to freedom, for example, and in some cases even claims to sovereignty. They bewail the fact that "only a tiny fraction of the energy that has gone into the improvement of domestic conditions has been applied to the prevention of war" (p. 375). Moreover, some of their prescriptions for evaluation can be applied to politics at every level. Nevertheless, the pervading orientation is toward domestic politics.

Reasons for both pride and dismay are fairly clear. The volume reflects substantial achievements in human thought and provides a philosophy of inquiry that, on the whole, is commendable. It is distorted, just as American political science is distorted, in the undue orientation to the domestic and in the corresponding widespread failure to think of individuals and groups in relationships with each other and with a world society.

VERNON VAN DYKE

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Handbook of Political Science, Volume 2: Micropolitical Theory. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. vii + 389. \$13.95.)

This volume contains five essays (on personality and politics, socialization, recruitment, group theories, and organization theory) included on the basis of their relevance to micropolitical theoretical concerns. However, parceling out the discipline into categories admits of no single solution, and the reader should be aware that other volumes in this series (especially volume 4) address related topics. While each contribution has value, the chapters lack a consistent framework which renders the volume as a whole neither an overall review of the current state of knowledge nor a well developed blueprint for future research.

Fred I. Greenstein's chapter on personality, which begins this volume, draws heavily on his earlier *Personality and Politics: Problems of Evidence, Inference, and Conceptualization* (Chicago: Markham, 1969). While this reduces its utility, nonetheless this essay is an excellent example of what one hopes to find in a handbook—a concise review of the critical

theoretical issues, and a literature review which integrates a large and diverse body of material so as to elucidate the underlying theoretical concerns and also provide a useful substantive introduction to the field. Greenstein's discussion of the criticisms of personality theory succeeds admirably in clarifying the nature of the debate, but only partially in resolving the problems he so lucidly discusses. Perhaps most valuable is his insistence that personal and situational qualities influence political behavior in a joint, interactive, contingent manner.

David O. Sears, writing on political socialization, and Moshe M. Czudnowski, on political recruitment, each adopt an approach focused more heavily on the substantive content of the field. Sears organizes his essay so as to identify the replicated findings on the central dependent variables and to address what is perhaps the most pressing question in political socialization, the persistence of preadult learning. He stresses the danger of confusing "partisan" responses (i.e., attitudes toward the authorities, groups, ideologies, and the like) with system attachment, an invaluable point.

What is not included is extensive discussion of socialization agencies or processes, the comparative utility of various theoretical perspectives (e.g., social learning theory), or more than a brief sketch of common criticisms of the field. He concludes with a well-taken but generally stated argument that socialization research needs greater analytic depth and a cogent case for clarifying the measurement of independent variables to move beyond descriptions of children's attitudes to causes.

While hardly a comprehensive review of the field, the above approach is more useful in Sears' case than in Czudnowski's because of the "sprawling" (p. 93) character of socialization research and its recent proliferation. Czudnowski deals chiefly and most successfully with the major variables which tend to dominate recruitment research. The graduate student can find here a useful summary and discussion of the major works in this area. However, this is the least ambitious of the chapters, perhaps because of the more limited nature of the field it surveys.

Czudnowski advocates a "microtheoretical" approach (p. 232) focused on identifying the "sociopsychological profile" (p. 230) of social positions likely immediately to precede initial recruitment, an interesting idea for avoiding the problems of studying only the successful recruits. However, neither the discussion in this chapter nor the field of political recruitment in general has enough to say about the relevance of recruitment to elite behavior in office or

system characteristics.

The last two chapters, J. David Greenstone's article on group theories and Dennis J. Palumbo's on organization theory, place much greater weight on articulating a point of view than on surveying the field in a comprehensive manner. Greenstone, for example, neglects any systematic discussion of the long pluralist-elitist debate in discussing group theories. While his essay cannot serve as a general introduction to the field, he elucidates the theoretical relationships among five major contributors to the field, concentrating on the concepts of group and interest.

Greenstone's central conclusion which he argues cogently and convincingly is that, because of the way in which interest is conceptualized, group theory cannot adequately deal with the sudden emergence of novel political demands such as those of the civil rights movement. However, he is less convincing in arguing that the solution to this problem is to incorporate "objective" measures of interest.

Palumbo's essay, like Greenstone's, is organized around a particular theoretical argument rather than designed to present a general overview of the field of organization theory. It stands along in this volume, however, in arguing against the utility of its topic's approach. Palumbo concludes, after sketching the elements of the political model and extensively and skillfully discussing selected theory and literature of rational choice, "that a political model . . . seems to be a better way to explain the empirical world of organizational behavior than the pure theory of rational choice" (p. 361).

He argues that most decision making in political organizations (which typically have multiple goals and malevolent opponents) takes place under conditions of uncertainty; further, that this uncertainty cannot be practicably reduced to decision making under conditions of risk; and hence rational choice theory must be so modified as to amount to a rejection of the rational model. Palumbo presents a clear introduction to the debates concerning the applicability of organization theory to political science which should usefully orient readers whether or not they accept Palumbo's conclusion.

Overall, this volume is a valuable addition to political science reference works. While not providing as comprehensive an overview of both the theoretical issues and major substantive findings as it might, it is nonetheless a useful introduction to several important areas of political science for the advanced student and a convenient way for the professional to update

her/his knowledge of fields not commonly monitored.

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Handbook of Political Science, Volume 3: Macropolitical Theory. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xiii + 620. \$16.95.)

Volume 3 of the Handbook of Political Science is entitled Macropolitical Theory, It includes six essays contributed by recognized authorities in their fields-Huntington and Dominguez on "Political Development," Dahl on "Governments and Political Oppositions," Linz on "Totalitarian and Authoritarian Regimes," Michael Taylor on the "Theory of Collective Choice," Tilly on "Revolutions and Collective Violence," and Stinchcombe on "Social Structure and Politics." These constitute a reasonably comprehensive set of system-level themes. Two of the essays-Dahl's and Linz'sdeal with varieties of political systems; another two-by Huntington and Dominguez and Tilly-deal with political change. One by Stinchcombe deals with social structure and politics; and the one by Taylor reports the results of formal-logical analysis of the collective political outcomes of postulated individual choices. Notable thematic omissions in a volume entitled Macropolitical Theory are the relations of regime types to policy outcomes, and the relationships of the international system to varieties of political systems and developmental patterns.

An evaluation of the volume has to concern itself with how well these essays cover their topics, assuming an audience of preprofessionals-undergraduate majors and first-year graduate students. Three of the essays, those by Huntington and Dominguez, Juan Linz, and Michael Taylor, were written especially for the Handbook and are good surveys of the literature and evaluations of the "state of the art." Dahl's contribution is a condensation of his Polyarchy and his contributions to his two edited volumes on oppositions. The contributions of Tilly and Stinchcombe, though written especially for the Handbook, represent rather inconclusive work in progress and are hardly appropriate as introductions to the theory of revolutions or the relations of social structure to politics.

Of the six essays, the two by Huntington and Dominguez and Linz are important new contributions, and by themselves would justify owning a copy of volume 3 of the Handbook. The chapter on political development begins with an historical account of the political development literature, and a summary of the hypotheses as to the relations among political, economic, and social modernization. It then proceeds to a discussion of the conditions and factors which may affect the processes and outcomes of political development, treating such questions as the impact of the domestic and international environments, the historical timing of developmental processes, the sequences of challenges or problems confronting political systems, and the rates of various aspects of social change. It then examines political development as a set of dependent variables admirably summarizing the research literature on such aspects of political development as political culture, political participation, political and governmental institutions, and national integration and cohesion. A final section discusses emerging theoretical trends and research foci.

In a contribution that occupies more than a third of the volume, Juan Linz presents what is easily the most comprehensive treatment of nondemocratic regimes to be found in the literature. It is in the tradition of Weberian historical sociology in which the specifics of individual cases and ideal-typical classification of varieties of authoritarian regimes are in acute tension. Linz resolves the tension in favor of spelling out the specifics of subvarieties and individual cases. This makes it difficult to come away from his more than 250 pages of factpacked discriminations with a clean and parsimonious typology of nondemocratic regimes. He seems to impute an almost sacred quality to the richness and complexity of historical reality, and errs on the side of including too much.

His contribution begins with a long essay on totalitarianism, easily the best analysis in the literature. He deals with the origins of the notion, the pros and cons of its use as a typological concept, and an examination of regimes which have been characterized as totalitarian and of its varieties. While he acknowledges that no contemporary regimes can be characterized as totalitarian, he points out that the experiences of having been totalitarian, or seeking to become totalitarian, are sufficiently formative to warrant two subcategories of authoritarianism.

He then turns to surviving traditional and semitraditional regimes including some societies in North Africa, Southeast Asia, and SubSaharan Africa, and the Candillist and Caciquist regimes of Latin America in this category of surviving traditionalism. Under the heading of authoritarian regimes properly speaking, he differentiates some six varieties: Bureaucratic-Military Authoritarian regimes, Organic-Statist Regimes, Mobilizational Authoritarian Regimes in post-democratic societies, Postindependence Mobilizational Authoritarian Regimes, Pretotalitarian and Posttotalitarian Authoritarian Regimes. The categories reflect the diachronic-synchronic aspiration of his typology. The prior history helps explain the principal operating features of the regime.

Having said all of these favorable things about Linz's contribution, it would be dishonest not to admit to exasperation over his prolixity, his refusal to summarize, his failure to draw things together. Historical sociology, to be sure, invites elaboration, differentiation, careful qualification, and acknowledgment of exceptions, but the greatest of historical sociologists, Max Weber, has shown that it is possible to combine this with sharp definition, useful abstraction, and lucid formulation of hypotheses. Perhaps the second edition of this Handbook will contain a leaner and more muscular version of this contribution.

It is more difficult to comment on Dahl's chapter. His theories of polyarchy and of oppositions are contributions of great importance to empirical political theory, but they do not constitute a handbook-like treatment of empirical democratic theory. Dahl's Polyarchy is concerned with the conditions which are favorable or unfavorable to democracy. His polyarchy scale enables one to locate regimes on the two dimensions of participation and competition, with "polyarchies" being those regimes that are high on both dimensions. Thus we can distinguish among real countries according to whether they are more or less polyarchic, and whether their shortfall is on the participation dimension or the "contestation" dimension, but he does not discriminate among relatively persistent historical types of polyarchy. For the purposes of Dahl's own work there was perhaps no need to do this; for the purposes of a handbook contribution on the empirical theory of democracy this is a serious omission. The work of Lijphart and others on varieties of democratic systems deserved to be treated.

Perhaps more serious is the omission of any treatment of democratization, or references to the literature dealing with it. Dahl's own discussion of historical sequences is a formal one and surely the weakest part of his poly-

archy theory. It is surprising not to find any discussion of Moore's Social Origins of Dictatorship and Democracy, Rustow's Transitions to Democracy, the Binder et al. volume on Crises and Sequences in Political Development, as well as a number of other serious contributions to the topic of democratization.

Michael Taylor's review of the literature on the theory of collective choice is an excellent summary of this new genre of macropolitical theory, in which collective political outcomes are generated from the logical manipulation of the postulated utilities and resources of political actors under assumed sets of decision rules. As he himself acknowledges, this is very far indeed from being empirical macrotheory. He points out that this formal deductive work has not as yet come to grips with the problem of time. In these studies, he acknowledges, no provision is made for dealing with "changes in preferences, in perceptions of the alternatives, in voting strategies, or in expectations about other individual's preferences and behavior: no changes take place as a result of persuasion; there is no accumulation and reinterpretation of experience, no learning, no adaptation" (p. 469). One misses in this thoroughly honest review of the collective choice literature some estimate of the value of this approach to hypothesis generation as compared with inductive approaches, or mixed inductive-deductive strategies. One also misses some appraisal of the limits of mathematical modeling of political reality.

Charles Tilly, in "Revolutions and Collective Violence," reviews and criticizes some of the literature on revolution and violence and then presents his own theory. He is not kind to the competition, as his metaphor of the old-time doctor sniffing chamber pots suggests. His two principal straw men are Chalmers Johnson and Ted Gurr, He disposes of Chalmers Johnsons' sociological system-dyssynchronization theory of revolution as too general to be of value in research; and as failing to separate out the political realm in which revolutions occur from the society or social system as a whole. He disposes of Gurr's essentially psychological explanation of collective violence as the consequence of "relative deprivation" under specified conditions, on the grounds of the poor quality of its data and some questioning of index construction. He is kinder to Barrington Moore and Eric Wolf because of their emphasis on social class and class coalitions, though surely their work would warrant some criticism as well.

Tilly concludes that the theory of collective violence and revolution is now a new ball game,

and that powerful theory will emerge from the collective history movement in which he is playing a prominent role. Rewriting history "from the bottom up" using modern research methods makes eminent sense, but Tilly's own contribution to the theory of revolution seems to rest only in small part on such a research program.

Tilly deals with collective violence and revolution separately and never quite succeeds in relating the two in theoretical terms. One is at a loss to explain why he sought to offer a theory of both simultaneously. It would have made more sense to deal with the one or the other as a major theme, bringing in the minor theme in some appropriate way. His one contribution to the theory of revolution is a specifically political one, the positing of a set of proximate conditions of revolution which are four in number:

- 1. The appearance of contenders or coalitions of contenders, advancing exclusive alternative claims to the control over the government currently exerted by the members of the polity.
- 2. Commitment to those claims by a significant segment of the subject population (especially when those commitments are not simply acknowledged in principle, but activated in the face of prohibitions or contrary directives from the government).
- 3. Formation of coalitions between members of the polity and the contenders advancing the alternative claims.
- 4. Incapacity or unwillingness of the agents of the government to suppress the alternative coalition or the commitment to its claims (p. 521).

Conditions 1, 2, and 4, he argues, are necessary to revolution, The third is neither logically nor empirically necessary but facilitates the fourth condition. Now, if one reflects on these three necessary conditions of revolution for a moment, they appear to come perilously close to the primitive paradigm that all things have a beginning, a middle, and an end, or to the proposition that revolutions are revolutions. In fairness, he elaborates these conditions in a section in which he draws on historical examples, and then looks at the French Revolution from these perspectives in greater depth.

In view of the relatively undramatic dénouement of this contribution, one is led to ask why it was necessary to demolish Chalmers Johnson and Ted Gurr at the beginning, particularly since it would seem that a good theory of revolution would have to have sociological, psychological, and political components. Finally one is led to ask why Charles Tilly, with his

rich fund of historical and contemporary knowledge, fails to provide his readers with some straightforward discussion of varieties of revolution, and their conditions and consequences.

Stinchcombe's "Social Structure and Politics" is hardly the introduction to political sociology one would be led to expect under such a title. He provides us with no intellectual history of political sociology within which his contribution can be located. He fails to define either social structure or politics. And although he tells us that he has "put on blinders" and presented an oversimplified theory, he doesn't specify how he has gone about his oversimplification, what he has left out, and what the costs and benefits are of his particular strategy of explanation. Finally, in his conclusion he doesn't tell us what he has accomplished and what research programs might follow from his analysis.

The method that he employs is a combination of rational choice and exchange theory, but unlike the better exemplars of these approaches, he does not present his propositions as heuristics, but as empirical explanatory propositions, checking them illustratively by occasional real examples. One might best describe this effort as work in progress and as inaccessible to most readers who would consult this *Handbook*.

These various criticisms notwithstanding, the editors of this *Handbook* have performed a useful service in publishing this volume on *Macropolitical Theory*. This reviewer has found the volume eminently usable for teaching purposes at the incoming graduate level. Perhaps a second edition will remedy some of the shortcomings suggested here.

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Handbook of Political Science, Volume 4: Nongovernmental Politics. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xviii + 285. \$12.95.)

The title of volume 4, Nongovernmental Politics, is a bit strange, but the contents are familiar enough. The intended coverage, especially viewed in relation to volumes 5 and 6 (on governmental and policy-output aspects, respectively) is of the more informal, extragovernmental activities often lumped together as "political inputs." Chapters on political parties (by Leon Epstein) and interest groups (by

Robert Salisbury) predictably appear. Beyond this point jurisdictional claims, especially between volumes 2 and 4, are not so clear. Individual voting behavior might be fitted into such a volume, but might equally fit into volume 2 as one of the more successful efforts at "micropolitical theory." And the same would apply to the topic of individual political participation. Both in fact appear in this volume, the former sharing a chapter with the topic of public opinion. This rounds out the volume, which is slimmer and, at least in three of the four chapters, less empirical than one might have expected.

The generation schooled in the Gabriel Almond input functions might inquire for the location of chapters on political recruitment and political socialization; however, they are in volume 2. More mystifying is the absence, both in volume 4 and the entire series, of a chapter dealing explicitly with political communication, press, and other media. Since at least one of the series editors has a long-standing interest in this area, this omission suggests that at least one potential contributor may have missed the final deadline. Fortunately, there is a recent alternative publication with extensive coverage of this area (Ithiel de Sola Pool et al., Handbook of Communication, 1973).

For the chapter on "Political Participation" the editors turned, not surprisingly, to Norman Nie and Sidney Verba, for whom the assignment must have come with almost perfect timing. Nie and Verba have wrestled with the topic for most of a decade, had recently published their book on Participation in America, and were in the midst of further analysis of extensive cross-national materials. The Nie-Verba emphasis on four distinctive modes of participation, rather than a single continuum, seems well on the way to acceptance as a dominant research paradigm. But it is recent enough-in contrast, for example, to the pioneering work of the Survey Research Center on individual voting-that it is not yet encrusted with layers of criticism, extension, or modification.

Given this propitious timing, Nie and Verba launch into their topic with verve and enthusiasm. And with substantial data. Their chapter presents some 11 tables and 15 figures; the balance of volume 4 includes only 3 additional tables. The organization of the material follows lines worked out in their earlier book on the United States, but with a considerable extension of cross-national comparisons of the various modes of participation. Of course the richness of primary data and coherence of presentation are achieved only at a certain cost:

chiefly an acceptance of the overall Nie-Verba view of the topic. This is a broad but not unlimited view, since it excludes nonlegal activities, subjective orientations, participation mobilized by government (rather than autonomously directed toward the government), and participation in other aspects of society. The survey data range widely across the United States and Western Europe (including Yugoslavia), but also extend to Japan, India, and Nigeria. The time-frame, however, is strictly contemporary.

If sales of volume 4 outrun those of some other volumes, much of the explanation will lie in accumulated professional interest in what a Phil Converse (or Donald Stokes if he had not gravitated into administration) would have to say about recent changes in American political behavior and various "revisionist" interpretations of such behavior. Converse undertakes this task in a long chapter (94 pages) dealing with the related topics of public opinion and individual voting behavior. In contrast to the data-oriented presentation of Nie and Verba, Converse faces the task of organizing and evaluating a vast and varied body of literature. The result is a closely reasoned and heavily footnoted summary in which the author only rarely permits himself the luxury of presenting new data (there are three tables, presenting descriptive material of a comparative sort). For converse-the data analyst nonpareil-work on the chapter must have represented a sacrifice as well as an opportunity.

Converse analyzes public opinion largely in terms of information level, motivation, and degree of attitude-structure of coherence. These are of vital concern for students of electoral behavior, but they tend to narrow the topic to a marked degree. A full chapter on the topic might have explored how public opinion contributed to the resignation of a president from office, the effects of recessions or limited war on popular approval or disapproval of administrations, or how legislators seek to mobilize specific "issue publics" (to borrow a felicitous Converse term). On the topics treated we have Converse's measured judgment that low information levels remain highly important (acquiring new information is most likely for the minority with a substantial body of previous information, or for those with unusually high motivation). He sees increases in the consistency of mass "belief systems" as sweeping, but probably not indicative of nearly as marked a shift in actual ideological sophistication. He presents some cross-national evidence which hints that the level of attitude-constraint for the U.S. in the 1964 and later period may be above the norm for modern industrialized countries, rather than the 1950s reflecting a period well below the norm.

On the central puzzle of the determinants of voting choice, Converse concentrates his attention on the difficulty of apportioning the explanatory influence due to issues and to party when-as is usual-the two are substantially colinear. At this point one faces a choice of tactics. One can formulate a multiequation model, incorporating effects of party on issueorientation as well as on the vote and possibly extend this to estimate reciprocal effects of issues on party. Converse opts for the other route of turning to various bits of "side information" which are suggestive of causal and temporal ordering. Predictably enough the "side information" that he values most suggests that party identification is, for all practical purposes, remarkably stable in the short-run (though subject to gradual strengthening over the life-cycle). By contrast, he sees issues as quite unstable. Hence the judgment that most of the "overlap" in explained variance should go to party; hence also the enthusiasm for the table-oriented "normal vote" concept.

He continues the long and fascinating discussion with briefer consideration of macrolevel changes in the electorate. Cohort analysis of the 1930s suggests the absence of any single "critical election," but rather a gradual realignment resting mostly on heavy Democratic gains among new voters. This is congenial with Converse's view of the stability of party identification. The final section considers the design of "representation" studies involving both elite and mass interviews (a procedure, incidentally, pioneered by the late Samuel Stouffer).

The third chapter, "Interest Groups," takes up a topic on which conceptual schemes and typologies rather outrun verified generalizations. Salisbury thoughtfully reviews the Mancur Olson arguments that "selective benefits" (and not the collective policy goal) are crucial. If, however, significant numbers of people take. the "golden rule" or Kant's "categorical imperative" seriously, the Olson problem dissolves. It is sad to note the almost total lack of hard evidence on this issue to date. Salisbury emphasizes the importance of group organizers, or entrepreneurs, a factor rather neglected in David Truman's classic account. And mention should be made of William Gamson's (The Strategy of Social Protest, 1975) intriguing effort at a systematic analysis of a random sample of "challenging groups," published since the Handbook went to press.

The final chapter, on "Political Parties," is the briefest. The subject served as intellectual midwife to many of the topics dealt with at chapter length elsewhere. Presumably there does not remain that much to be said about contemporary parties per se. Leon Epstein skillfully spells out a modest comparative view of parties and their functions. Once viewed as threatening and seditious, parties tend now to be ignored or perhaps criticized for contributing to a false harmony. Even in Europe the trend seems to be to the "catchall" party. Politics can indeed be pursued by other means. One hopes it is not just an act of faith to conclude that the presence of contested elections between organized parties is, in part, important also for the indirect effect it has on the efficacy of other means of influencing governments.

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Handbook of Political Science, Volume 5: Governmental Institutions and Processes. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xx + 459. \$15.95.)

It is almost impossible to do justice to this volume of the *Handbook of Political Science* in the limited space available. Each of the six essays is a comprehensive analytical survey of a broad topic that falls under the general rubric of "institutions and processes." The contributions include: "Constitutionalism," by Harvey Wheeler; "Federalism," by William Riker, "Executives," by Anthony King; "Legislatures," by Nelson Polsby; "Courts," by Martin Shapiro; and "Bureaucracies," by Mark Nadel and Francis Rourke.

Harvey Wheeler's discussion of constitutionalism is a tour de force that covers much of the history of Western political thought from Plato to modern social science as viewed through the development of one of its central conceptual and institutional features. Wheeler will draw fire for his claim that American constitutionalism is outmoded because of its mechanistic nature and for his assertion that the United States needs a more "systemic" approach built on the rule of law and the institutionalization of civil disobedience. He does not make this argument with sufficient clarity or on the basis of empirical evidence. Wheeler also underplays the influence of Locke and Montesquieu on the development of American constitutionalism, and he pays insufficient attention to the idea of constitutional balance. The concept of the balanced constitution has been a persistent and recurring theme in American constitutional politics and is more than mere political Newtonianism. But these are minor quibbles; Wheeler's essay is a sheer delight to read and it sets a tone for the volume that the subsequent contributions manage at times to match but never to exceed.

William Riker's consideration of federalism is a refinement, updating, and restatement of his 1964 critique. Although less polemical in tone than the earlier work, his argument is no less devastating: as an institution federalism does not make much difference in the substance of a nation's policies but it can alter the style of its politics. Federalism's effects are at worst to give advantages to minorities at majority expense and at best to be so trivial (a "minor nuisance") that it is not a significant issue.

Riker grounds his attack in a scientific theory of federalism based on empirical examples. The essence of federalism lies in the political, not economic or social, bargain that creates it and in the distribution of power in political parties which shapes the mature federal structure (p. 141). There are two necessary conditions to every federal bargain, (1) the existence of a threat or the opportunity for aggrandizement, and (2) strong provincial loyalties. The structure of a federal system is correlated with its party system; centralized federalism is accompanied by centralized parties. All else relating to the operation of federal systems, e.g., constitutional and administrative arrangements involving the areal distribution of power, are "accidents."

Riker's essay will probably draw more critics than the rest of the volume as he is highly critical of a cherished American institution. Federalism does not agitate him as much now as it did in the mid-1960s only because the race issue has been nationalized and federalism's current beneficiaries are no longer southern racists but economic conservatives seeking to evade centralized regulation. Those who challenge Riker will confront a carefully constructed argument. It seems to me that he is vulnerable on at least two points: failure to account satisfactorily for the sustained vitality of successful federations in which regional and national loyalties coexist over time and a too abrupt dismissal of constitutional and administrative questions, such as those recently raised in National League of Cities v. Usery, 49 L.Ed 2d 245 (1976).

Anthony King is the only non-American contributor to this volume, and his examination of executives contains a distinctively British flavor. Nevertheless, he draws heavily on studies of the American presidency to flesh out his

essay. King's focus is research, or the paucity of it, on executives. He limits his treatment to heads of government and their top subordinates and concentrates on four topics: recruitment, the dynamics of executive institutions, executive-legislative relations, and psychological approaches.

King's survey of research on executives reveals that it is heavily concentrated on the United States and, to a lesser degree, on Britain. He attributes the general paucity of executiverelated research to the difficulties of obtaining access to information and of quantifying that which is obtained. Although these are serious obstacles, King argues vigorously that they should not be allowed to deter future inquiry. Specific research needs include cross-national recruitment studies that establish the extent of the relationship between selection processes and subsequent behavior in office and a psychological approach that reveals the interplay between individuals and institutions. King calls for a research agenda on executives that is detailed, comparative (as between similar systems), and which tries to answer specific, answerable questions (pp. 243-44).

Nelson Polsby defines legislatures in terms of their distinguishing characteristics which include official status, legitimacy, multiple membership, formal equality of members, collective decision making, deliberateness, and subsequent accountability to the people. Polsby bases his analysis of legislatures on a typology that employs two variables: access to the political system (open/closed) and specialization of governmental activity. He devotes most of his attention to legislatures in open systems having specialized decision-making institutions, i.e., parliamentary democracies. These he places on a "continuum of legislative power which expresses variations in the legislature's independence from outside influences" (p. 277). At the end of the continuum are arenas for conflict between power centers located elsewhere, as typified by the British Parliament. At the other extreme are transformative legislatures with an independent capacity to make laws, for which the American Congress is the prototype.

In studying arenas, external institutions are of primary importance while internal structures are the central focus in the examination of transformative institutions. Polsby argues that the degree to which a legislature is transformative depends primarily on the character of its parliamentary parties. Thus, American legislative parties are "extremely coalitional, decentralized, and flexible," while their British counterparts are "somewhat less coalitional, hierarchical, and fixed" (p. 292).

Polsby is at his best when he recounts the development of systematic empirical study of Congress by American political scientists and when he examines the modernization of Congress as an institution. His conceptual framework has substantial value for taxonomic purposes and it provides considerable explanatory leverage.

Martin Shapiro's essay on courts rejects as "fruitless" and without approximation in the real world the conventional prototype involving an independent judge applying pre-existing legal rules following adversary proceedings and resulting in a decision in which one party is found to be right and the other wrong (p. 321). Instead, Shapiro offers as a more appropriate model a triad involving two disputants and a third party. The "triad for purposes of conflict resolution is the basic [and universal] social logic of courts" (p. 322). It is maintained in all societies by consent and, as societies become more complex, by law and office.

Tension arises in courts when they function as conflict resolvers because of the need to convince the disputants of the neutrality of the judges and of the laws they have chosen to apply and because of the introduction of a third interest, that of the regime. This tension increases when courts go beyond the functions of conflict resolution and social control and engage in law making. In fact courts are not independent institutions, but an integral part of the regime.

Shapiro sees the courts as major political institutions that perform multiple functions "ranging under varying circumstances from bolstering the legitimacy of the political regime to allocating scarce economic resources or setting major social policies" (p. 365). That this is not widely recognized by the public is due, in part, to the damage that such recognition would do to the basic logic of the courts (the triad) and the consequent development of guises, myths, and conventions that surround the judicial process. Political scientists have long been aware of the policy-making role of appellate tribunals, especially through the exercise of judicial review, but they have for the most part ignored the essential political nature of the entire judicial process. Shapiro performs a valuable service by staking the discipline's claim on the courts. This opens challenging areas for inquiry that we have hardly begun to explore.

The final contribution, by Mark Nadel and Francis Rourke, examines the political role of bureaucracies "as instruments of power or as sources of policy" (p. 373). It is a synthesis of

the major themes found in the scholarly writings of the authors. Bureaucratic influence derives from technical expertise and is manifested through advice bureaucrats give to officials, the capacity of bureaucratic organizations to carry out essential and complex governmental tasks, and the exercise of discretionary authority by bureaucrats.

While public bureaucracies are essential policy-making institutions in all political systems, their role is especially crucial in the United States. Extensive political entanglement of government agencies occurs primarily because American political parties do not function effectively as linkage mechanisms between public opinion and public policy. Agencies cannot look to parties for aid in developing policies and as sources of support for programs. Instead they turn to organized private interests. The result is an aggravated problem of insuring administrative responsibility.

Nadel and Rourke analyze the controls for achieving bureaucratic responsibility through a typology which varies along two dimensions, location and formality. The controls are either external or internal to the agencies and bureaucrats, and they are exercised either formally or informally. All four types of controls are essential, no single set of controls can guarantee responsible bureaucratic performance, and the full range of controls cannot eliminate the tension between accountability and effectiveness.

To sum up, this is a most useful reference volume. The essays are comprehensive, thoughtful and often provocative, and theoretically well grounded. All are of high scholarly quality but they vary considerably with respect to their controversiality, imaginativeness, and their conceptual frameworks. Graduate students will find them invaluable in preparing for prelims, and scholars will do well to take them as starting points in preparing new courses and when undertaking research projects.

If there is one aspect of the essays that is disturbing it lies in the great extent to which they depend on the United States for illustrations and thus reflect the culture-bound character of American political science. (Anthony King's is a partial exception.) This may not appear to be a defect to persons interested mainly in American politics, but it is a severe limitation for those who seek to achieve scientific rigor in their analyses of political institutions. However, as long as an overwhelming proportion of the world's political scientists are Americans this condition is likely to continue. It does seem that minimally, more conscious

comparative studies of institutions and processes are required if research is to advance much beyond single case analysis and a cumulative body of knowledge is to be developed.

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Handbook of Political Science, Volume 6: Policies and Policymaking. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. xviii + 494. \$15.95.)

Handbooks in any scholarly field present special problems in finding the relevant standard of evaluation. The highest criteria of scope, skill, and theoretical subtlety would doom reviewers to perpetual disappointment. Between the useful and the useless, however, this volume tends toward the satisfying.

The reason for such caution is partly that the weaker articles in this collection are quite poor. One expects diversity of viewpoint, style, and achievement; one finds all of that here, with the upsetting inclusion of inferior materials. All of the articles focus on governmental actions and their impacts, taking us rightly away from the older case studies that stressed the origins and enactment of policy. The subject is handled with largely American materials-except for Fried's comparative urban materials-and the discussion of substantive cases of American, foreign, and domestic policy is accomplished with a minimum of quantitative material and statistical reasoning. Within those constraints, our knowledge of many fields is enhanced. Sapolsky brings the reader the themes of science policy in a readable, interesting way, the very model of what a handbook should do. Bernard Cohen and Scott Harris on foreign policy, Robert Fried on urban policy, John Grumm on policy impacts-all bring together the political science research on these topics in an informed, scholarly, usefully summative way. Duane Lockard's article on race policy and Charles Gilbert's on welfare policy are also comprehensive.

The problems with the volume are twofold, only the first of which the editors could have easily remedied. That would have been to jettison the essay by Lassell (on the intellectual and appraisal 'functions' in policy research) and Pechman's account of the role of the economist in the making of economic policy. Not only are these the weakest chapters; they are also the first two.

Lassell's effort is simply out of place, a windy repetition of overarching generalizations written elsewhere about why sensible people should think hard about policy problems. It is practically unreadable, a claim supported by the frustrations of two graduate students assigned to review it. Pechman's essay is readable, but inappropriate in focus. Neither about the politics of economic policy making-a very important topic-nor the conflict of roles for the professional economist, the essay is a simplified account of Samuelson-level public finance and a memoir of the Council of Economic Advisors since the war. A number of political scientists could have treated the politics of economic policy-Robert Eyestone, James Andersen, Douglas Hibbs, for instance. And a number of economists write well about the politics of economic advice-Charles Schultze, Alice Rivlin, Sam Peltzman, to name just three.

But criticism of some of its parts would not do justice to the chief problem with this volume. It is in the aggregation of these articles as a scholarly handbook that one realizes how far we are from a paradigmatic science or even an accepted body of knowledge. The individual articles at their best provide good introductions to their subjects and excellent bibliographies of available works, but they do not constitute a coherent approach to "policies and policy making." Indeed, the subareas are defined by the "industry" studied, not by the political science approach to it. It would be as if an economics handbook brought together studies of trucking and income maintenance policies and failed to mention the distinctions among cash and in-kind programs, capital and human investment government efforts. Despite theoretical articles to open and close the volume, we are no closer to a paradigm for policy studies at the end. The articles do not add up to anything more than the sum of the parts. And that raises the question of why some areas were covered and others not: why were the military, business, tax, health, and environmental fieldswhich account for the bulk of any modern government's activity-sacrificed to the politics of science, urban life, and foreign policy?

One answer is suggested by the editors at the outset. Political scientists, they say, are early socialized into the view that their field is ill-defined, heterogeneous, and amorphous (p. iii). In this volume, art is imitation of reality, as amorphous heterogeneity triumphs over order to reflect ill-defined subjects. Were the standard of scientific advance held, the volume would be disappointing. As a collection of articles about political science and American public policy,

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the book contributes some helpful, substantial overviews.

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Handbook of Political Science, Volume 7: Strategies of Inquiry. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. 458. \$13.95.)

The first two chapters of this edited volume cover, respectively, library sources and sources of quantitative data, the first by Clement Vose and the second by Jerome Clubb. Vose's objective is "to discuss some of the areas concerning the use of sources that political scientists are likely to find valuable," and to view in depth "a number of systems or sets of materials that are important for their own sake," in order "to develop principal points to be applied to other sets" (p. 2). He does this by reviewing for us various examples of errors in standard sources, and by enumerating various catalogs, indexes, encyclopedias, yearbooks, directories, and handbooks. He sensitizes the novice to factors such as the political and financial motives of authors and publishers and guides us through the maze of overabundant sources. I cannot resist noting, however, that Professor Vose provides three political science examples of how the Library of Congress Card Division often fails to list books under appropriate subject headings; his three examples include books by Greenstein, Polsby, and Vose. Whether intentional or not, this reflects the somewhat in-house flavor of the entire Handbook series.

Clubb's purpose is to provide a "minimal starting point" for the student interested in computer technology and supporting resources for social science research. He provides a discussion of all major data archives, followed by discussion of the operation and use of archives, archival development, sources of machine readable data, and the addresses of 39 social science data archives. Like Vose, he provides a useful if not startling chapter.

In chapter three, Harry Eckstein makes a strong argument for case studies as means of validating theories, rejecting the prevalent position in our discipline that case studies are useful to discover interesting questions but that theory can only be tested by using the comparative method. His case is presented clearly and persuasively, and his chapter is probably the best organized and readily intelligible of the

eight in this volume. Professor Eckstein presents a careful introduction which defines his terms, a brief overview of the possible arguments regarding the relative role of case studies and comparative study, a somewhat longer discussion of various types of case studies, and then rebuts potential objections to his argument. His argument is a sophisticated one, as he distinguishes between ordinary case studies, which he does not necessarily encourage, and crucial case studies, which he sees as efficient alternatives to comparative studies, differing only in expense but no in logical status. Although examples of crucial case studies are few, Professor Eckstein delineates them well enough that careful readers will "know one when they see it." I highly recommend this chapter for use in all introductory graduate seminars in scope and methods; it is clearly written and provocative.

Chapter five is a plea for more experimentation in political research by Richard Brody and Charles Brownstein, while chapter six is an overview of the uses of survey research by Richard Boyd and Herbert Hyman. I find both chapters to be good overviews and cogently presented reviews of current work in these fields. I believe both are useful for introductory graduate seminars in research design, and invaluable for courses in experimental design or survey research, respectively.

Brody and Brownstein are advocates of simulation and experimentation. They provide a brief discussion of the place of experimental method in scientific research-its advantages in terms of controlling extraneous variables and of manipulating the independent variables-as well as presenting some of the details of experimental designs and of several applications in political science, particularly in international relations and political behavior. Their chapter is cogent and well argued, with just the right balance of overview and detail, of review and advocacy. Like all good advocates, Brody and Brownstein present the weaknesses of experimental methods-focusing mostly on external validity-and unlike most advocates, even good ones, they take these weaknesses seriously. The result is an excellent addition to the literature.

Boyd and Hyman address two general questions: "What types of belief and behavior can be measured with surveys?" and "What purposes do surveys serve?" Their intended audience includes those "examining the potential of survey methods for the first time." Their purpose is a general introduction to the uses of surveys, and a review of political science applications. Unlike Brody and Brownstein, they do not address carefully the objections to survey

methods, such as the control of confounding variables and the problems of determining cause and effect, although they do review briefly problems such as the impact of question wording and the nonattitudes controversy. Given their purpose, however, they succeed for 40 pages and fail for 30 pages. The first half of the paper is a nice overview-introduction to the major studies that can-and have-been conducted using survey methods. Their unabashed enthusiasm for survey research is evident and ought to whet the appetite of even the most skeptical graduate student. Unfortunately the chapter ends with a whimper, because the last 30 pages contain a too-detailed history of survey research. Their purpose in this history is to emphasize the growth of survey research "in an environment that inextricably mixed policy and scholarly interests," but the detail is unnecessary to the point.

There are two technical chapters, chapter four on polimetrics by Hayward R. Alker, and chapter seven on formal theory by Gerald H. Kramer and Joseph E. Hertzberg. Alker has as his purpose "to help political statisticians to be more critically and creatively aware of the possibilities and limitations of measurement procedures," and to "enrich the analytical tools we can use to make qualitative and quantitative political descriptions." He assumes only one semester of social statistics, although he notes that for most people with that background, the material covered will be novel. Alker presents a brief overview of polimetrics, followed by four sections on representational measurement procedures, aggregative approaches to intentionalist assessment practices, situational and structural measures in intentional assessment, and the individual case in intentionalist assessment. The latter four sections consist mainly of a few general points and arguments followed by examples of recent works which happen to impress Hayward Alker.

My own reaction to Alker's effort is mixed, though more negative than positive. I for one would not assign this chapter to students (with one semester of statistics) in a graduate course in measurement. His examples are not detailed or clear enough to provide a good introduction to any of the topics he reviews. For example, two of his illustrations of aggregative approaches are the Carroll-Chang individual differences scaling and the Baas-Brown application of intensive factor analysis. Alker is unable to devote sufficient space to this discussion to provide comprehensible introductions to these topics. Unless one has read the original articles, one is unlikely to learn much from Alker's discussion, and if one has read them carefully, his discussion adds little. It is my own judgment that the chapter would have been more useful if Alker had written the entire article on his first topic, an overview of polimetrics. A general discussion of statistical theory and practice would be precisely the kind of overview most useful for the first year graduate student who has recently struggled through the incrementalism of introductory statistics. Alker's effort, I fear, is much too idiosyncratic to be useful for most of us.

The Kramer and Hertzberg chapter on formal theory is a competent nontechnical overview and introduction to political economy and game theory. Most of the discussion is cogent, even for the uninitiated, and the use of several extended examples helps clarify abstract points. My only comment is editorial: This chapter is about 45 pages long and introduces far too many complex concepts. The volume as a whole would have been better served by cutting Alker's 70-odd pages and Boyd and Hyman's 80-odd pages and by expanding Kramer and Hertzberg's discussion to allow elaboration of what is at present a dense presentation. The last chapter is by Herman Kahn on futurology.

JOHN SULLIVAN

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Handbook of Political Science, Volume 8: International Politics. Edited by Fred I. Greenstein and Nelson W. Polsby. (Reading, Mass.: Addison-Wesley, 1975. Pp. 450. \$14.95.)

If volume 8 of the Handbook of Political Science is a faithful reflection and adequate analysis of the state of international relations within political science, its students have no reason to rejoice. For this collection of six essays by seven distinguished authors displays everything that is wrong about the field—along with some of its history and accomplishments.

A reader wandering in from outside would be bewildered by the fragmentation. One of the essays, the fine survey of national security affairs by Richard Smoke, deals largely with the evolution of American thinking and policies in the realms of strategy and arms control; the other essays place themselves at the systemic or global level of analysis. There is little coherence between, on the one hand, the overview of the world political system by George Quester, who focuses on power configurations and on threats to peace, and, on the other hand, the discrete studies of strategy (Smoke), of the dynamics of international economic interdependence and integration (Keohane and Nye), and of the

difficulties faced by international law in a world as troubled as the present one (Leon Lipson). Moreover, large chunks remain outside: there is little on the historical sociology of, and political thought about, international politics, little on the relations between domestic politics and foreign policy, or on studies of the foreign policies of specific states or groups of states, little on international organization; there is almost nothing on foreign contributions to the field (in this respect, the volume is intensety parochial; it could at least have asked why the discipline has "taken off" so much more in the United States than elsewhere). There is a fragmentation of approaches as well, in two ways. One concerns the writing styles of the authors. Quester is discursive; others are incisively analytical (and, like Keohane and Nye, perform a most useful service in clarifying concepts and critically reviewing the literature in the field); Lipson's piece is really a think piece on the different intellectual perspectives about contemporary international law. More serious is the opposition between methodologies. Four of the essays are resolutely "literate" and traditional. One—the longest by Dina Zinnes is a thorough survey of quantitative international politics (she prefers to call it the scientific study of international politics, thus showing once again that the champions of one particular set of methods not merely confuse science with it, but believe that they are the only ones willing "to move from the unique to the general" and committed "to search for patterns of association" between classes of events or types of entities: a rather arrogant annexation, unjustified by the meagre intellectual results of the effort, if not by the material and human resources devoted to such labors). The remaining essay (actually the first, by Kenneth Waltz) dismisses in a few sentences all the research so painstakingly reviewed by Professor Zinnes: for, says Waltz, it merely amasses "coefficients of correlation . . . without asking which theories lead one to expect what kind of a connection among which variables." Waltz, whose assignment was the theory of international politics, and whose criteria for theory are as rigorous and "scientific" as those set up by the self-styled "scientists" of quantitative methods, does not even consider their work! Obviously, all the fireworks of the 1960s about contending approaches have led to no rapprochement among the contenders.

An innocent reader would also, in all likelihood, be scared away from the field by the conspicuous absence of any broadly accepted conceptual framework, into which all these fragments could fit. This is true at two levels. At what might be called a constitutive level, there is no paradigm for international relations. The paradigm which prevails in the history of political thought, and in the works of most of the authors discussed by Waltz as well as by Waltz himself-the "realist" image of the state of war, or troubled peace, among sovereign units in a state of nature-is now under sharp attack, as the Keohane and Nye essay demonstrates. Quester's chapter does not even consider the issue, a central one for students of world politics. At the level of explanation and prediction, there is no satisfactory theory—this is the point made by Waltz in his ambitious essay with his customary mix of brilliance and perversity.

No student of international politics can afford to ignore Waltz's analysis. He shows why a theory cannot be derived inductively from the data which it is supposed to explain; he defines the steps which testing a theory requires; he states that "most theories of international relations are so imprecise that expectations of outcomes cannot be stated in ways that would make falsification possible"; he concludes that the only theory without which international politics cannot be understood is balance-of-power theory—but it does not tell us nearly enough. And yet, contemporary theorists have done no better; he proceeds to tell us why by skewering several of them (including me).

I have no quarrel with his skeptical findings, but I have two chief criticisms of his position (I will not dwell here on some trivial disagreements with his readings, or misreadings, of my own writings). First, concerning his definition of system, the rigorous distinction he makes between structure (defined as a "purely positional picture" of international society) and interacting units is far less helpful than he believes. He defines system change as the result of a major change in the distribution of capabilities among major actors; but this assumes a far more homogeneous and calculable conception of state power than is justified, and it leaves cut important changes (such as the shift from the Old Regime to revolutionary France) which have their origin not in a shift in the distribution of capabilities but in the internal make-up of the units. Recent work by Nye and Keohane has shown the degree to which the outcomes of international processes cannot be explained by structure, as well as the complexity of "structure" once one ceases equating it with the distribution of military might. Waltz's relentless rejection of what he had earlier called "second image" explanations (what he now calls reductionist theories that search for causes within the units)-his desire to

separate completely unit level and system level changes and to reduce the study of the international system to that of the relations between the structure and the interactions of the unitsleads him, in the name of theoretical rigor, to leave out many of the most important aspects of the reality he wants theory to explain (he criticizes Morton Kaplan for conceiving of "subsystem dominance": it is the negation of a systems approach, says Waltz; but is it any more absurd than, in "domestic" political science, talking about a political system dominated by one of its components, such as a single party or a strong presidency?). Waltz may be right that definitions of the international system which are less exclusive than his own result in a loss of theoretical rigor. But his own approach makes him incapable of appreciating fully some of the highly original features of the present international system (the coexistence of military bipolarity and moderation, the effects of transnational relations).

Secondly, I suspect that Waltz demands of theory in international relations more than the field allows. He does not address himself to Raymond Aron's argument, in Peace and War (and several essays), about the impossibility of achieving a "scientific" theory of indeterminate behavior along the lines of economic theory: the concept of power differs from that of money; goal-oriented behavior differs from instrumental action; and while the permanence of a contest among units introduces elements of calculation and order, they do not suffice to provide the field with a rigorous theory. We can describe, in skeletal ways, the logic of state behavior, the main patterns of interaction and regulatory mechanisms, but making the field intelligible is not the same as disposing of an explanatory and predictive theory.

Leon Lipson ends his essay with advice to international lawyers to "curb their zeal, contract their horizons, and Think Small." The advice one might want to give to students of international politics, after one has read this volume, is more complex: on the one hand, Think Small, and try to find out more and to be more rigorously analytical about all the areas about which we still know, in fact, very little (this includes much of the past). But on the other hand, Think Large-not Big: try to understand the interrelations between functional or geographic areas, try to ask important questions about the field as a whole (those which traditional philosophers have asked, and for which endless collections of data and correlations are no substitutes); but forget about a master key or about the mirage of grand theory.

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Community: A Critical Response. By Joseph R. Gusfield. (New York: Barnes & Noble, 1975. Pp. xvii + 120. \$11.50.)

This book offers a comprehensive critical interpretation of the way in which the concepts of community and society are used as contrasting ideal types in sociological thought. It covers such central sociological themes as the relations between traditionalizing and modernizing processes in societies, the progressive and regressive aspects of urban-industrial societies, tribalism and nationalism as social processes, and finally communes and communitarianism as responses to mass society. These are difficult themes to discuss in a short book and Gusfield deserves congratulations for developing coherent and often original positions on these and other related issues. He is especially skillful in pinpointing ideological positions, including those disguised as value-neutral social science. While noting how ideologies sometime distort objective and subjective realities, he also pays sharp attention to their role in providing necessary dramatic and critical perspectives on social issues.

An important original contribution of the book lies in the way in which it delineates a model for thinking about communal and societal structures that views them as time and space bound resultants of human choices rather than as the fixed outcome of irreversable social trends. Using materials drawn from India, Gusfield shows how entire castes accomplish collective social mobility and thereby deploy communal resources to accomplish effectively what are usually conceived of as societal purposes. He analyzes the extensive literature on communal organization, stratification, and national development contrasting "systematic" theories about traditional resistance to modernization with a more complex and more accurate version of the multiple possible relations between these variables in diverse settings. American ethnic patterns are briefly considered and a remarkably flexible framework for thinking about associational life, class, and urbanindustrial trends is set forth.

It appears ungrateful to ask more from a book which does so much to such good effect as does this one. My only wish would be to hear more from Gusfield about every topic he takes up. His thinking is broad and deep; all students of community at any level have much to learn

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from him. This book provides a model of concise, original, and fair discussion of a key concept in social science. Would that every concept we use could receive such excellent treatment.

MAURICE R. STEIN

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Public Goods and Public Welfare. By John G. Head. (Durham, N.C.: Duke University Press, 1974. Pp. ix + 291. \$12.50.)

This monograph consists of 12 essays. Eight of them examine one or more aspects of the concept(s) of public goods, two discuss welfare economics, and two discuss Musgrave's concept of merit goods. The essays were written over the period 1962—1971. Eleven of the 12 essays originally appeared in European journals.

The chapters, with a few exceptions, are surveys of the literature Professor Head deems relevant to the topics examined. Footnotes are used generously. This feature makes the book useful for those who wish an introduction to the topics examined. Almost all contributions are patted on the head; the highest plaudits are reserved for James Buchanan, Richard Musgrave, and Paul Samuelson. That Buchanan happens to espouse a variety of political anarchy whereas the latter two economists are enthusiastic supporters of government intervention does not seem to bother the author.

Only after checking the original published papers did I learn that two chapters are reproductions of replies, written in response to criticisms (one by Harold Hochman and one by Charles McLure). Unless one reads the criticisms, the replies are difficult to appreciate. Either the comments of the two critics should have been reproduced or the replies should not have been included in the volume.

Of the three main topics, the welfare chapters are rather good commentaries. The two chapters on merit goods are not very useful. I agree with McLure that Head has misinterpreted Musgrave. The major portion of the work is devoted to the examination of concepts of public goods.

Head is one of many enthusiasts for the self-styled "modern" approach to public finance—a position that among other features places public goods as the main actor in the new scenario. The vast literature that has grown around this topic has a large payoff in promotions for academic economists; its payoff in terms of selecting and scaling public programs may be only slightly greater than zero. Head,

like many others, has no trouble finding numerous illustrations of public goods. The ploy here is to list supposed illustrations, with little or no justification. But he has almost nothing to say about how actual public programs should be selected and scaled. There are promising applications of the public-good approach to local government as illustrated by the works of Trebout, McGuire, and Oates, among others. Head did not choose, however, in the present essays to push very deeply into that literature.

Viewed as surveys, Professor Head's study has considerable merit especially but not only for novices in the fields covered. Many of the deficiencies of the book could have been remedied by rewriting the papers and by eliminating duplication. The excess verbiage seriously handicaps the reader in trying to understand Head's message. Nevertheless if one perseveres, there are many worthwhile observations to be found.

EARL R. ROLPH

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On Liberty and Liberalism: The Case of John Stuart Mill. By Gertrude Himmelfarb. (New York: Alfred A. Knopf, 1974. Pp. xxiii + 345. \$8.95.)

Among the great number of recent writings on Mill, this book is distinguished by its originality, and even where there is disagreement, one is refreshed by an encounter with an informed and provocative and politically relevant argument.

Professor Himmelfarb argues that there are two incompatible sides to John Stuart Mill's thought, one found in the essay On Liberty (and in a few related writings on the women question) and the other, reflecting intellectual traditions that are incompatible with the doctrine of the essay On Liberty, which is found in most of Mill's other writings, whether they were early or post-1859, whether primarily reflecting Benthamite teachings or written during the period of rebellion against Benthamism. Thus we have the more widely known Mill of On Liberty and the "other Mill." The doctrine of On Liberty, it is argued, is fundamentally radical. Despite superficial similarities to traditional understandings of liberty, it was radical even for Mill, who in his other writings "represents a quite different mode of liberal thought" (p. xi). The other Mill "belongs to an older liberal tradition, the tradition of Montesquieu, Burke, the Founding Fathers, and Tocqueville" (p. xxi). So different are the two traditions that

one becomes the basis of criticism of the other; indeed, the "other Mill" is the best critic of On Liberty. The implication (drawn out at the end of the book) is that the "other Mill" would disapprove of many developments that have taken place in our time which have been justified by arguments found in the essay On Liberty.

What, it should be asked, are the distinguishing characteristics that give the essay On Liberty its radicalism and, in the context of Mill's thought, its uniqueness? First of all, our attention is directed to Mill's defense of what he called "one very simple principle" that should "govern absolutely" the dealings of society with the individual with regard to compulsion and control; this principle provided that interference in the liberty of action of any individual can only be justified by self-protection. The emphasis, Himmelfarb notes, is on the singlemindedness of the doctrine-and she points to Mill's use of such terms as "one," "sole," "only," and "absolute" (pp. 14-15). The consequence is to set up an opposition between the individual and society and to make the claim of the individual absolute and uncompromising, whereas society is left unprotected as its claims on the obligations of individuals are greatly diminished. In addition, it is noted that for Mill the obstacles to liberty were not the traditional ones located in an oppressive political and legal system; on the contrary, public opinion, custom and tradition, religion and moral beliefs were regarded as the instruments of oppression. Thus society was the individual's chief antagonist, and as a consequence, the doctrine encouraged deep distrust of society, a contempt for convention, and extreme antiauthoritarianism. Furthermore, Himmelfarb notes that the diverse society that Mill wanted, which maximized spontaneity, individuality, and "experiments in living," also encouraged an unhealthy isolation. And by not providing support for morality, which usually came from social affections and public authority, a diversity in conduct is allowed to be so great that there is a problem of social cohesion.

The history of political philosophy contains many theoretical positions that would be critical of this understanding of liberty. But Himmelfarb argues that the "other Mill" offers the best critique of this view of liberty. Suggesting that we too easily assume a compatibility between the doctrine of liberty presented in 1859 and his comparable pronouncements in other writings, she has systematically made this comparison. She finds that whereas the Mill of *On Liberty* absolutizes liberty and defends it in terms of a single principle, the

"other Mill" is part of a tradition "that can accommodate liberty together with such other values as justice, virtue, community, tradition, prudence, and moderation" (p. xxii).

Himmelfarb reminds us that, far from welcoming fully free expression, the "other Mill" held that permanent political society required allegiance; he said that there had to be "something which is settled, something permanent and not to be called in question" (p. 46). Mill was quite explicit: the core idea for the state had to be "above discussion"; there had to be "some fixed point; something which men agreed in holding sacred" (p. 46). These passages (appearing in "Coleridge" •[1840] and repeated in A System of Logic) are a far cry from the essay On Liberty where even the sacred is held subject to completely free discussion. In addition, instead of celebrating the ideal of autonomy and self-determination, the "other Mill" often emphasized the need to give up the exercise of private judgment as one deferred to the authority of an educated elite. Furthermore, far from idealizing a diverse society, there is evidence that Mill thought extreme diversity contained the germ of civil war (pp. 46-47).

Himmelfarb also points to differences between the two Mills on man's capacity for virtue. Whereas On Liberty reveals no concern that fully free expression might lead to a proliferation of amoral and immoral conduct, elsewhere Mill was worried that by acting on passions, without regard for moral rules, people would behave immorally and would not perform their obligations to others. Whereas in the essay On Liberty conscience was an obstacle to individuality and freedom, the "other Mill" advocated education for the individual so as to produce habits "of subordinating his personal impulses and aims to what were considered the ends of society; of adhering, against all temptation, to the course of conduct which those ends prescribed; of controlling in himself all the feelings which were liable to militate against those ends" (p. 78). Here Mill called for a "restraining discipline" that is contrasted by Himmelfarb to the words and spirit of the essay On Liberty. Furthermore, such self-control was not only a matter of education and internalized norms; it was to be supported by the resources of society-laws and institutions and education and public opinion (pp. 73, 78, 106-07).

So much for Himmelfarb's claim that there are two Mills. Is it correct? In view of Himmelfarb's impressive marshalling of evidence, we may conclude that there are important differences between *On Liberty* and other parts of Mill's writings. However, while there are differences between the control of the

ferences, the two Mills are not as separate as she makes out, for some of the argument of On Liberty may be found in that portion of Mill's thinking that was a by-product of St. Simonian influences-a part, according to Himmelfarb, that was an essential ingredient in the "other Mill." There is evidence of this in Mill's discussion of transitional eras-periods of history characterized by great intellectual diversity and therefore by the need for liberty. During transitional eras old doctrines had become obsolete, new ones had not yet been acquired. Authority and deference ceased to exist, and the various elites (intellectual, religious, political) disagreed and came into open conflict; and this led to the erosion of the authority on which the social system depended. During transitional eras, Mill agreed, liberty had two important functions. The first was negative, and it is exemplified by the Benthamite use of liberty, which was to discredit, undermine, and even destroy old doctrines and institutions that were vulnerable to rational criticism. The second function of free discussion was to discover and diffuse true opinions; in other words, to provide for the competitive struggle of the varied creeds that were in conflict during the intellectually and morally chaotic transitional period. Out of such rivalry would emerge a new doctrine that would make possible an escape from the unsatisfactory transitional era. This way of understanding liberty was different from the rationale for liberty that prevailed among either traditional constitutional theorists or the Benthamites, both of whom thought of liberty as a means of informing public opinion and providing security against abuse of political and legal power. And one can see Mill's speculations about the function of liberty in transitional eras as anticipations of that part of On Liberty that was concerned with liberty as a way of discovering new truths and improved social arrangements.

This understanding of the function of liberty during transitional eras was not confined to the brief but important early period when Mill was most intensely interested in St. Simonian ideas. It formed the immediate background to his thinking during the 1830s and '40s when he cultivated an eclecticism that was intended to comprehend diverse versions of the truth; after encountering one another, these versions were to be synthesized into a yet more comprehensive understanding. With this outlook Mill evaluated and sifted the "half-truths" from both Bentham and Coleridge, democracy and elitism, empiricism and deduction, laissez-faire and socialism. The evidence of his attempts to synthesize such partial views into a new philosophy is to be found in many of his essays, in the *Political Economy*, and in the *System of Logic*. And this road to the truth assumed an understanding of the function of liberty that originated during the St. Simonian period (1829-31) and carried over to the time when he wrote *On Liberty*. In other words, it was a view of liberty shared by the Mill who wrote *On Liberty* and the "other Mill." Thus it may be concluded that either of the "two Mills," despite their differences, would not have regarded the other as alien.

There is another part of this book that should be mentioned. Himmelfarb argues persuasively that Harriet Taylor played an important part in the inspiration and in formulating the ideas for the essay On Liberty. For Himmelfarb this explains how an alien ingredient was introduced into the body of Mill's work. But acknowledging that Harriet had a large role does not require that we agree that the two Mills were separate. For the comprehensive framework of Mill's thought, especially that part of it that had been shaped by the ideas of the St. Simonians and by German romanticism, included a place where her ideas could fit, not as the result of intrusion but as a matter of affinity. In passing it should be said that Harriet's ideas are seriously analyzed in this book for the first time.

In the last section of her book, Himmelfarb points to the dangers of Mill's doctrines in On Liberty. She argues that he does not adequately provide for principled limits to the assertion of liberty, so that there is no boundary to the extension of liberty, even if it carries one to such revolutionary lengths as to undermine the social basis of liberty itself, to say nothing of other values. In other words, liberty is absolutized, making it impossible to defend any kind of morality or authority. She also argues that this absolute liberty allows for the exploration of not only man's higher nature but also his capacity to be depraved. And she points out that Mill's doctrine justifies a moral revolution which, although it has been under way for more than half a century, has become visible only in our time when it has become democratized.

Without disagreeing with this analysis, one may dispute Himmelfarb's view that since the two Mills are quite separate and incompatible, the "other Mill" is the best critic of the doctrine of On Liberty. She holds that the "other Mill's" ideas reflected an older tradition that recognized the multiplicity of ends and therefore the need to prudently combine them, so that liberty will be balanced against concerns for morality, stability, fairness, and com-

munity. In other words, the "other Mill's" ideas reflected the "Whiggish mode of liberalism" that "prized liberty as one of several virtues, that had a healthy respect for social institutions and conventions, that was willing to use the resources of society and the state to further the public good, and that above all distrusted the application of simple formulas to complicated affairs" (p. 299).

The difficulty here is that in the thinking of the "other Mill" there were ingredients that were incompatible with the "Whiggish mode of liberalism." First, there was his early Benthamism, which, with its Hobbesian origins, established an opposition between liberty and law that is incompatible with an understanding of liberty that allows for its prudent combination with morality, stability, and authority. In addition, the "other Mill," because he had incorporated St. Simonian ideas, was committed to a philosophy of history and a doctrine of progress that encouraged the visualization of vast changes which, though not involving physical violence, were still revolutionary and therefore inimical to the older Whiggish mode of liberalism-which was distinguished by a concern with gradualism, continuity, and moderation. Furthermore, the "other Mill" consistently expressed irritability with his own country's inclination to compromise; yet such compromise was at the heart of the older Whiggish tradition, whether found in Burke's observations on prudence, or in more traditional defenses of moderation, or in eulogies of British practice. In fact, the "other Mill," having confessed to "youthful fanaticism," rejected Whiggish moderation. Indeed, much of his political activity during the 1830s was a sectarian pursuit of doctrinaire goals, and, it should be added, throughout this period he was bitterly hostile to the Whig party and to the idea of Whiggism. And finally, as symptomatic of these themes, there was Mill's consistent criticism of those who were spokesmen for traditional Whiggism. Consequently, Mill was not likely to be an adequate spokesman for the Whig critique of On Liberty. Indeed, since the doctrine of On Liberty in important ways originated in the ideas of the other (and early) Mill, we should not expect to find an adequate basis for evaluating On Liberty in those ideas.

Learned and original, this book is a notable contribution to Mill scholarship. Despite one's disagreements with parts of it, the book is welcome, not only for the way it has stimulated healthy controversy and exploration, but also because it exemplifies how investigations in history and political philosophy can be relevant

to our politics.

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Organized Social Complexity: Challenge to Politics and Policy. Edited by Todd R. La Porte. (Princeton: Princeton University Press, 1975. Pp. ix + 356. \$16.50.)

During the 20 year period from 1948 to 1968, a number of articles were written on the general topic of complexity that have become interdisciplinary classics. The term "classic" should not be taken simply as hyperbole. Their authors are a distinguished lot-Christopher Alexander, Albert Ando, Franklin Fisher, G. A. Miller, Herbert Simon, John Platt, and Warren Weaver_among others-and in each case their examinations of complexity proved to be characteristically insightful and contained profound, if as yet commonly unrecognized, implications for model building, data analysis, and theory construction in the social sciences. An attempt to integrate, criticize, and extend this body of work is long overdue, and it is a credit to Todd La Porte and the other contributors that they have attempted to remedy the situa-

This volume consists of eight loosely related papers on organized social complexity plus an introductory and concluding chapter by the editor. In the Preface, La Porte tells us that the book grew out of a small seminar at Berkelev that had been convened to study the topic. Six of the chapters are revisions of papers prepared for that seminar; two others, those by Garry Brewer and Daniel Metlay, were added later. For the most part, the chapters reflect their origin. The majority of those which were generated in the seminar are self-consciously discursive and are primarily interested in raising provocative questions and airing assorted speculations. Given the importance of familiarizing more political scientists with the many issues involved this may be a wise strategy and, collectively, the articles should generate reflection on the part of individuals adhering to a variety of methodological and theoretical persuasions.

It should be noted, however, that occasionally this discursive approach tends to sacrifice some of the theoretical and prescriptive benefits that might have accrued if the authors had chosen to take a closer look at some of the verbal and mathematical metaphors they employ and points that they raise. One example of this is the La Porte-Metlay treatment of com-

plexity in the introductory chapter and its technical appendix. Central to this discussion in the statement that the complexity of an organized social system is a "function of the number of system components, the relative differentiation or variety of these components, and the degree of interdependence among these components" (p. 6). Unfortunately, this definition and the sociometric exchange theory terminology in which it is embedded, make it unclear whether the degree of interdependence refers to the number of interconnections, the pattern which they form, or the process of information and resource flow. A focus on the latter, taking off from Simon's "The Architecture of Complexity," would probably have proved the most fruitful but it is the aspect of interdependence least developed here.

La Porte and Metlay proceed to explore system complexity solely on the basis of an influence graph and its matrix representation. The principal drawback of this approach is that it permits only limited inferences about the nature and impact of complexity since it considers the existence or nonexistence of interconnections but not their character (nonlinearity, instability, etc.) or the processes that they represent.

In addition, there also appear to be a number of technical problems. To be more specific: On a technical level much is made of the fact that complex systems frequently take the form of "semi-lattices." However, such a mathematical structure possesses a number of formal properties (e.g., that there exists a least upper bound for every two elements) which are not shared by those graphs to which the term is applied here (e.g., Figure 1.3 on page 9). Nor is it true that a complete lattice is equivalent to a complete and symmetric diagraph, as is claimed on page 9. Along the same lines, the statement "If the ones in the matrix are replaced by numbers representing, in some sense, the weights of influence, the rank will either increase or remain the same" (p. 33, fn. 37) is false for matrices larger than 3 x 3.

Also suspect is Metlay's assertion that rank of an adjacency matrix can be used to represent the degree of system complexity. Rank is a property of linear transformation and can only be applied meaningfully to a matrix that represents one. It is not clear that an adjacency matrix of a diagraph has any such relationship to a linear transformation. Further, rank fails to have the properties that Metlay himself (pp. 26–27) claims to be desirable for a measure of complexity. Rank is not a monotonic function of the number of units, the degree of differentiation (whatever that is), and the number of

interconnections; and it is not inversely related to decomposability. Individually such points may seem relatively insignificant but collectively they make the reader skeptical of how successful the authors have been in their attempt to overcome the formidable difficulties involved in relating mathematical models to social phenomena.

More serious are the theoretical and conceptual limitations that stem from the decision to depict complexity and investigate its properties with a simple influence graph and its matrix equivalent (an adjacency matrix of ones and zeros). Aside from the fact that such a representation seems in danger of robbing complexity of its fundamental property of nonsimple interaction among elements, the problem with this approach is that the elements in the matrix describe states instead of processes. This is a problem because a number of authors want to interpret successive powers of such a matrix as future states of the system in order to study the impact of complexity over time and the manner in which a complex system evolves. Yet because the original elements of the matrix contain no process data, as in an authentic input-output or Markov model, higher powers of the matrix yield no such information. In fact, it is doubtful that the resultant matrix has any interpretation other than an esoteric combinatorial one.

Naturally, such comments do not accurately characterize each of the eight chapters. There are a number of thoughtful observations in virtually all of them, particularly those by Taylor and Brewer, and this book would be a useful addition to the reading lists of a variety of graduate courses ranging from political theory to model building. On the other hand, the observations above have not been made gratuitously. All too frequently in these chapters a lack of focus and development has obscured the insights that are present and postponed the discovery of others.

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Percepts, Concepts and Theoretic Knowledge: A Study in Epistemology. By Harold N. Lee. (Memphis, Tenn.: Memphis State University Press, 1973. Pp. 257. \$10.50.)

The idea of "the furniture of the world" epitomizes a fundamental misconception according to Professor Lee, namely, the notion that the world is constituted of discrete, static, enduring objects. These static objects, as con-

ceived by the erroneous view, become the subject matter of knowledge in everyday experience and science, and their existence and character is independent of human activities involved in the attainment of knowledge. Lee finds this error at the heart of numerous misguided efforts in philosophy and science. The error is found in Descartes' metaphysical perspective; it generates mind-body dualism and solipsistic problems like the problem of the external world and is presupposed in phenomenalist epistemologies like sense-datum theories. The same mistake encourages the idea that science is passively descriptive rather than interpretive, that contemplation is the highest form of knowledge, and it even lurks in dispositional accounts of belief and in behaviorism. Borrowing "ready-made" from Lee's many descriptions of this focal error, we can call it the fallacy of ready-made objects.

Lee avoids all these consequences of the alleged fallacy with an epistemological system ("a conceptual scheme or categorical scheme" in his terminology) based on the metaphysics of process. Events, change, flux, ongoing activity, these are fundamental, while objects along with concepts and generalities come into our thinking only as a consequence of divisions and selections made in the seamless continuum of process in the course of human cognitive activities.

The development of all this is a large undertaking, and Lee tells us in his introduction that the undertaking is speculative. Academic philosophy today tends to deploy scholastic techniques in arid and narrowly conceived investigations, and we are deficient in projects of great scope and imagination, so a work like Lee's is welcome. Speculative though it is, however, it is not wholly satisfying and that is because Lee relies on the mere repetition of general metaphysical formulas and falls into a shallow honorific use of the rhetoric of flux and continuity on many of his main points. The attractions of process philosophy have been known to philosophers since the time of Heraclitus, and since that time process metaphysics has seemed to carry with it its own paradoxes.

The single greatest fault of Lee's work is its failure to offer any substantial guidance concerning these problems. For example, Lee presents a naturalistic account of mind as

everything will be all right in the philosophy of mind if we think in terms of continuity and flux rather than of static ready-made objects.

Stretches of the primordial flux are called "episodes," and episodes interact with one another. "Sensitivity" is measured by the degree to which an episode "responds" to its environment of other episodes. Then somehow "experience" is generated, although presumably at this primitive level it is not the experience of anyone. "Experience is the result of participation, or being part of the flux. Hence those episodes that are more active and sensitive constitute a richer and more varied experience than those that are less active and sensitive" (p. 51). To go along with Lee here, we have to accept the idea of flux without raising the question, What is in flux? And then we are led, mostly by means of gradual shifts in vocabulary, from the mere idea of a flux to "response," "sensitivity," and "experience" and, thence, to "proto-generalization," "perception," and "consciousness."

We can certainly endorse Lee's conviction that the philosophy of mind ought to be naturalistic and his critical claim that the modern empiricist tradition has not provided an adequate understanding of mentality. Though his view is in the right spirit, however, it does not really give us anything definite enough to be called an articulated philosophy of mind. The same mysteries that beset the idea of consciousness in the epistemologies that commit the ready-made object fallacy will reappear in Lee's process metaphysics. Why are we entitled to think of one part of the flux as "responding" to another? How does an episode "participate" in the flux, rather than merely being part of it? Why do interactions deserve the title "sensitivity"? These questions raise the same difficulties as the question, framed in the metaphysics of objects, "How can causal interactions between the nervous system and outer things eventuate in conscious perception of those things?"

Lee gives us no guidance on these matters and we need much more to judge his account of mind than the loose metaphorical story he tells. Again, we are repeatedly warned that the existence of objects is never independent of our own cognitive activities. By way of illustration, Lee discusses the existence of subatomic particles (pp. 207-08), which is a very correspondent

human cognition? An explanation is owed the reader here. Without it we are free to think, as indeed we do think, that some of the furniture of the world does exist without reliance on our cognition and, if that is so, then the ready-made object fallacy is not a fallacy after all.

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The Pseudo-Science of B. F. Skinner. By Tibor R. Machan. (New Rochelle, N.Y.: Arlington House Publishers, 1974. Pp. 224. \$9.95.)

This book brilliantly fills the need for cerebral dialogue on the ideas of Burrhus Frederic Skinner-a need which derives a little from the visceral verbiage that emits from both those who agree and disagree with Skinner and very much from his premises and their profoundly dangerous implications. The premises include the easy manipulability and the rightness of using most people as instruments of the purposes of a few people. These premises have been shared by those dedicated to the service of a church or God (like John Calvin and the Spanish Inquisitors), those dedicated to the service of the state or society or humanity (like Hitler, Marx, Lenin, and Stalin), those who would have most people subserve the ends of a few gifted humans (like Nietzsche, Ayn Rand, and other "Renaissance" individuals who argue for the glorious fulfillment of talented individuals and neglect the cost of this fulfillment in the exploitation of untalented individuals).

Machan's book penetrates to these premises as they prevail in Skinnerism, with a deep clarity, intelligence, and open-mindedness that contrast markedly with the superficial clarity and deep muddiness of Skinner's writing. Perhaps because he denies the existence of consciousness, Skinner seems to be unconscious of the implications of his writings. Perhaps because he makes consciousness central to his critique, Machan makes the implications clear. But then Machan shows his own lack of awareness of the implications of his own values.

Skinner argues that human beings can and should be manipulated to ensure the survival of culture. Just what aspects of culture merit survival are not clear because Skinner innocently maintains that his moral standards are morally neutral and scientific. Presumably the culture he wants to survive is not the street culture of theft, mayhem, rape, murder, and assassination. But Skinner does not say.

Machan argues that indeed humans in some circumstances need conditioning. But the ends

that Machan specifies are not abstractions like divine will, humanity, the state, or even a social class. Rather, they are the ends of the individual—more exactly, it appears, the extraordinary individual, and, more particularly, the economic entrepreneur.

Both Skinner and Machan, so considered, emerge as elitists. Skinner is the scientific elitist, basing his advocacy of human manipulation on the supreme value of maintaining culture, and making stimulus-response psychologists the manipulators. Machan is the Renaissance elitist, justifying the subservience of society to gifted individuals on the implicit grounds that individual humans are the only inherently valuable entity. Contrary to Kant, he does not consider whether every human individual, even if ungifted, is an entity of inherent value. He quotes Ayn Rand at least twice for saying (p. 177) that "individual rights are the means of subordinating society to moral law." Taken at face value, this statement sounds like the highest morality. Taken at what Skinner would call the operant level, it means that those humans who are not extraordinarily gifted are to be used as instruments of those who are.

What both Skinner and Machan have unwittingly neglected is one very basic aspect of human nature and a basic moral derivation from it. All people wish to be regarded in some ways as equal to all other individuals. This demand for equality in worth, for dignity, is so universal that it can only be deemed part of human nature. People do not in all ways want to be subordinated to the purposes of others, even to elitists who rationalize their ends in moral or scientific terms. The morality of Christ and Kant is based on the inherent value of each individual, regardless of ordinariness or extraordinariness. The morality of Marx is based on the inherent value of each ordinary individual. The morality of Nietzsche, Rand, and Machan is based on the inherent value of each extraordinary individual.

There does not seem to be much fundamental difference between the ideologies housing those who would make people altogether into instruments of some "higher" good, whether it be the race, the state, society, a class, or even humanity and the ideologies housing those who would make ordinary people subserve the purposes of extraordinary people, whether psychologists or entrepreneurs. May be the latter are more candid.

A plague on both their houses. Both kinds of elitists need the positive reinforcement of reflecting on the teachings of Christ, Kant, and the Marx of *The German Ideology*. If Skinner and Machan were voluntarily to condition

themselves thus, they would find that they like themselves and others better, but also that others liked their ideas better for having nonparochial foundations. For this lack of arrogance and for making the individual the valued entity, Machan is more likable than Skinner. For their elitism, both are equally likable to elitists.

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From Affluence to Praxis: Philosophy and Social Criticism. By Mihailo Markovic. (Ann Arbor: University of Michigan Press, 1974. Pp. xviii + 265. \$12.95, cloth; \$3.95, paper.)

"I find myself in profound disagreement with most of the people who describe themselves as Marxists" (p. xiii). The Yugoslav philosopher Mihailo Markovic, however, who makes this statement in the preface to his book From Affluence to Praxis, is just that, namely a Marxist. Belonging to the prominent Praxis group, so-called after their famous and, since 1975, by party decree prohibited journal, Markovic's future is somewhat in jeopardy. Praxis friends of his at the University of Belgrade have been fired for ideological reasons. Reacing, therefore, Markovic's book, which is based on earlier essays and lectures given at the University of Michigan in 1969/70, turns out to be an introduction into the vicissitudes of intellectual survival in Yugoslavia.

Having been an active member in the Yugoslav resistance movement during the German occupation of Yugoslavia, Markovic, however, is familiar with the lifestyle of a partisan in the literal and figurative sense of the word. Even though he does not refer to these personal experiences—Erich Fromm provides the reader with the biographical information about the author in his sympathetic foreword—the figurative meaning of the partisan circumscribes beautifully the thrust of his Marxist humanism.

Markovic retraces his humanism to the early writings of Marx. Here he finds, like so many other intellectuals on the Left in our century, the genuine spirit of a revolutionary Weltanschauung. Without trying to divorce a 'young' from an 'old' Marx he, nevertheless, blames the ideologues of the Communist movement for having reified the 'old' Marx and, thereby, making it impossible to invoke the radical criticism of the 'young' Marx against Marxist dogmatism and orthodoxy. The Belgrade philosopher wants to overcome reified Marxism, to restitute the basic activism of Marx's

thought, and then to live the way of praxis: "A philosopher who intends to change the world, not merely to interpret it, does not stop at reaching reliable knowledge of what is. He also examines the realistic possibilities of what might be and engages in the practical realization of those possibilities which best correspond to certain objective human needs. A revolutionary social philosophy is, therefore, both knowledge and evaluation, discovery of general truths, and a radical social criticism" (p. 22).

Markovic's understanding of social philosophy is not only directed against the reified Marxism of the bureaucratized societies in Communist Europe. Being a Marxist at heart he has neither forsaken his belief in the possibility of a revolutionary future, nor has he become a fellow traveller of liberal democracy in the capitalist West. His critical analysis of Western societies becomes merciless when he discusses the ideological premises of technological economism, the economism that led to affluence: "The growth of technology for its own sake is meaningless and dangerous. If the technostructure has any philosophy, it is a philosophy of complete nihilism, and of complete social irresponsibility" (p. 98). The social sciences in the West do not challenge this abysmal situation since they are dominated by positivism," ... according to which the sole function of science is to describe and explain what there is, and, if at least some laws are known, to extrapolate what might probably be. All evaluation in terms of needs, feelings, ideals or ethical, aesthetic, and other standards—is considered basically irrational and, from the scientific point of view, pointless" (p. 48ff). At best, this neutrality amounts to the silent affirmation of the status quo, at its worst to the open collaboration of science with power.

Markovic's belief in the possibility of a more humane future has survived the experiences of inhumanity in the twentieth century. But he is the opposite of a true believer when articulating inhumane truths: "The experiences of the twentieth century . . . do not give us reason to believe that evil in man exists only in the sphere of 'facticity' and only in the time which precedes genuine human history.... The scope and character of bloodshed and mass madness ... can no longer be explained by the romantic, dualistic picture of a latent positive essence and a transient bad appearance. Evil as a human disposition must lie very deep" (p. 219). Existentialist-sounding passages like these, which one encounters in the writings of most members of the Praxis group, add persuasive honesty to Markovic's book, even though they don't alter his Marxist convictions.

These convictions are best exemplified when he uses the mixed Yugoslav system of central bureaucracy and organizational selfmanagement to illustrate the future possibilities and present deficiencies of a post-capitalist and post-bureaucratic society. Yugoslavia is not Paradise regained, but discussed as one concrete historical possibility among others. "All possibilities are open in logic, but not in history. Human nature is not an abstract, fixed, transcendental entity. But neither is it something which care be created by any arbitrary decision of a free individual. In each historical epoch there is a general structure of human being, as a crystallization of the whole past history of human praxis" (p. 209).

The moderate historical determinism of these lines becomes only occasionally threatened when Markovic is surfing on the wave of an ideologically predestined future. Then, when for example attempting to justify truly revolutionary Russia against the Stalinist aberration, he sounds somewhat shrill and out of step with the general tone of the book (p. 158). The overall impression, however, lets one forget these rather superfluous exercises in ideological rhetoric.

Less excusable in this book are the absence of a name index and the sloppy editing. To make Campanella (1568–1639) a contemporary of the eighteenth century (p. 211), to attribute to Hegel a nonexisting book, entitled *Philosophie des Geistes* (p. 123), and to misspell the Italian Marxist Antonio Gramsci persistently as Gramshi is bad enough, but nothing compared to the atrocious spelling of German titles and quotations in the footnotes.

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The Political Thought of William of Ockham: Personal and Institutional Principles. By Arthur Stephen McGrade. (New York: Cambridge University Press, 1974. Pp. xiii + 269. \$18.50.)

The present monograph constitutes a valuable addition to the growing body of literature on the political thought of William of Ockham. It is all the more welcome as the spectrum of scholarly opinion regarding the matter at hand is unusually broad and varied. Ockham has been alternately described as a moderate and an extremist, a conservative and a brash innovator, a papalist and a conciliarist, an individualist and a constitutionalist, an Erastian and a theological positivist. Some have even questioned whether

he had anything like a coherent political theory or, if he did, whether it bore any relationship to his nominalist principles. Setting his own independent course through these muddied waters, McGrade offers a synthesis of Ockham's thought that does justice to and reconciles all or most of its apparently contradictory strands. The book is divided into two uneven parts, the first of which deals with Ockham's expressly political views, and the second with the elements of his academic philosophy and theology that may be deemed relevant to those views.

The Ockham presented in these pages is not a skeptic or a mere critic of existing institutions but a constructive thinker to whom may be traced some of the most innovative ideas of the time. His overriding concern was with the dignity and freedom of the individual, whose rights he defended among other ways by insisting that no one could be corrected without first being shown his error (cf. pp. 48-63). Although he was not the only theologian of his generation to hold that the head of the Roman church could lapse into heresy, he was the first to devise a process of doctrinal correction whereby, once convicted, an erring and pertinacious pope could be brought to heel. On the institutional side his attacks were pitched for the most part at the hierocratic conception of society which claimed for the pope the fullness of spiritual as well as temporal power. Yet he never challenged the fundamental assumptions of medieval society. His sole intention was to explore new ways in which the harmonious cooperation of the spiritual and the lay powers could be secured. The position that he advocated in that regard is characterized as a balanced dualism. Others before him (e.g., Thomas Aquinas and John of Paris) had rejected the monism of the curialists, but Ockham went further than all of them in calling for a disengagement of the two powers, a disengagement that he sought to promote by emphasizing the spiritual nature of the church and simultaneously limiting the sphere of governmental activity to the punishment of wrong-

Although consistent with his nominalism, Ockham's politics cannot be said to be deduced from it. His own proposals for the reorganization of society, both ecclesiastical and lay, were defended on grounds of practicality rather than of theoretical necessity. This is not to be taken to mean that he was an unrestrained voluntarist or a proponent of reason of state politics. As McGrade rightly points out (pp. 189–96), he fully acknowledged the role of reason in human affairs and did not doubt for one moment that moral conduct should be governed by objective

principles. One wonders, however, to what extent the argument in this instance goes to the root of the problem. The basic issue is not whether Ockham admitted the existence of nonsubjective norms of morality but whether these norms are ultimately founded on anything other than an act of God's free will. In an effort to preserve the divine omnipotence against the encroachments of philosophical necessitarianism, Ockham reduced the natural law in the strictest sense to a single negative precept prohibiting the hatred of God. He was thereby led to interpret differently the classic question of the dispensability of the precepts of the Decalogue. While in many particulars his theory resembles the older view, the spirit that informs it can hardly be regarded as traditional. Similar reservations may be voiced in regard to the significance attached to Ockham's threefold division of natural right into absolute, nonabsolute ideal, and suppositional. By referring to this so-called suppositional right as "natural," Ockham has invested it with a higher dignity than it might otherwise be thought to possess. Though less conservative than that of the political Augustinians of his age, his position may thus be more conservative than that of Augustine himself.

For want of space McGrade has deliberately avoided lengthy comparisons with other writers. The occasional references to Marsilius of Padua and Dante, helpful as they may be, betray a lack of awareness of the element of accommodation in the works of both of these authors. In view of the role that he assigns to the "stronger or superior part of the whole citizen body," it is doubtful whether Marsilius can be considered a champion of popular sovereignty (cf. pp. 62, 83, 109, 123, 214, 226). Dante's views on the subject of Christianity and its relation to civil society may likewise be considerably more radical than his own generally respectful statements would seem to suggest (cf. pp. 84, 99, 217). A larger question arises concerning the "political" character of Ockham's writings. That, as a result of his involvement with Ludwig of Bavaria and of his own quarrels with three successive popes, Ockham developed a passionate interest in politics during the last 20 years of his life is undeniable. There is a sense, however, in which even the works of this period may be said to have been somewhat less than political. Ockham's success would undoubtedly have been greater had he been less outspoken and especially less bitterly polemical in the public utterance of certain legitimate but potentially disruptive ideas.

The foregoing strictures are not meant to

detract from the merits of a book that abounds in illuminating insights and pointed formulations. McGrade's essay not only provides a competent and remarkably lucid account of the politics of this "most formidably technical of the great scholastics" (p. 230), it can also serve as a fresh point of departure for a study of the impact of political Ockhamism during the century and a half that immediately preceded the break with premodern thought consummated by Machiavelli and his followers.

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How Family Members Perceive Each Other: Political and Social Attitudes in Two Generations. By Richard G. Niemi. (New Haven, Conn.: Yale University Press, 1974. Pp. 214. \$10.00.)

The main goal of this book was to assess the accuracy, in sample survey data, of various family members' reports on other family members' attitudes and demographic characteristics. The author's primary conclusions are somewhat narrowly methodological, recommending various "nuts and bolts" techniques to maximize validity of data collection. For example, he suggests specifying explicitly which target individuals the reporting family member is to consider in answering each question, and paying greater attention to such measurement techniques as scaling or factor analysis that will indicate whether or not disparate questions are in fact measuring the same concepts, and so on. Of more interest to political scientists, however, will be the excellent detailed descriptive data on variables relevant to political socialization. The data-base utilized is the now well-known 1965 Jennings and Niemi survey of a national probability sample of 1669 high school seniors, which also yielded interviews in one-third of the cases with both parents, in another third with just the father, and in the remaining third with just the mother. This study provides by far the most complete and valid data extant about parent and off-spring political characteristics.

The book essentially analyzes the accuracy of each family member's observations about each other's demographic, attitudinal, and behavioral characteristics. It proceeds by considering the student's observations of the parent; husband and wife of each other; and parent of student. In each case the author assesses, in turn, the aggregate level of agreement across the relevant family members, their pairwise agreement, the determinants of inaccuracy, and

finally systematic biases affecting accuracy.

There are two aggregate differences of consequence. All observers underestimate the frequency of political Independents among their targets, by as much as 13 percent in one case (parents' underestimating students' Independence). And, parents systematically perceived less conflict, rancor, and tension in the family relationships than did their children, though these differences tended not to be enormous (less than 10 percent on most items). Hence the author concludes, probably justly, that at an aggregate level, there is little to be lost by using one set of family members' judgments about another's characteristics.

Viewed pairwise, family members are much less accurate about each other. More important, accuracy varies enormously across different characteristics. At one extreme, family demographic characteristics seem to be perceived quite accurately by all (though it must be remembered that the children in this case are 17 years old, probably an optimum age for accurate perception of other family members). Among political attitudes, though, the picture is much more variable. The direction of presidential vote is generally accurately perceived, for example, the student's perception of the parent's presidential vote correlates .82 with actual vote. Somewhat less accurate are voter turnout (.68) and party identification (.59), and way at the bottom is level of political interest (.25). Finally, reports of family structure and relationships show, remarkably, almost no consensus at all, beyond some minimal levels of agreement on how well the parents get along together.

The author identifies three rather interesting sources of systematic bias. One is just "good guessing." Accuracy improves as the real attribute becomes more common. For example, the vast majority of children whose parents voted in 1964 perceived parental turnout more accurately than did the small minority of children whose parents did not vote. "Social-desirability" biases tended graciously to overestimate the frequency of others' socially desirable attributes, such as doing one's citizen duty by casting a vote or voting for a victorious candidate. And "self-directed" biases systematically underestimated political disagreement between the observer and any other family member, by distorting the other's position (i.e., an "assimilation" effect).

But there is a great deal of unsystematic inaccuracy as well. Its major correlates tend to be fairly obvious. For example, broken families make children less accurate about their parents, remarriages produce inaccuracy between husbands and wives, parental agreement makes children more accurate about either parent, strongly partisan and more politicized individuals and families induce more accurate perceptions of political dispositions, and so on.

Most provocative, however, are the dramatic differences in accuracy across attributes. Everyone perceives others' presidential votes quite accurately, but no one can predict how anyone else feels about the emotional tone of their family life. A fairly static and well-known object such as party identification is perceived fairly accurately, but estimates of others' thermometer ratings of social groups are wildly inaccurate.

Inaccuracy could potentially be caused by a number of variables, most prominently (1) ambiguous or unstable target dispositions, (2) inadequate infrafamilial communication, (3) motivated distortions, (4) sloppy perceptions, and (5) inadequate measuring instruments. For example, why does everyone underestimate each other's Independence, or show lack of consensus about family emotional relationships? These attributes may be harder to detect than simple, overt, concrete attributes such as a presidential preference or vote. Or they may be less stable, and so come across in confusing ways. Maybe they are quite stable and meaningful, but not regularly or honestly discussed. This research has not yet succeeded in picking these apart. However it goes a long way toward providing some excellent descriptive baseline data from which additional research can begin to do so.

Finally, there is one curious theme running through this description of adolescents and their parents. Parents are amazingly ignorant of their children's political preferences (37 percent completely unable to place them on the party identification scale) and when they do perceive a preference, it is highly subject to the "selfdirected bias." That is, parents either don't know what their kids think, or simply (and often wrongly) claim "they think just like me." And, again, parents seriously underestimate (from the child's point of view) the level of emotional conflict in the family. The impression one gets is of parents trying to present to the interviewer an erroneously harmonious, united, and intimate picture of the family, glossing over the real emotional and attitudinal conflicts that are there. But maybe the family is not such a Pandora's Box as the parents seem to think. Emotional conflicts do not carry over into political conflicts, and the children seem to want to attribute their own preferences and/or socially desirable behaviors to their parents, even when they have to distort a little. American families could perhaps afford to be a bit more open than they seem to be, without utter anarchy breaking out.

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The Politics of Organizational Decision-Making. By Andrew M. Pettigrew. (London: Tavistock, 1973. Pp. 302. \$13.50.)

This is a case study of innovative organizational decision making in the business firm of Brian Michaels (a pseudonym), a British clothing and furniture retailing concern. It examines the events leading to the selection of a third generation computer for the firm's central administrative offices. The author employs a longitudinal research design covering the period 1964–68 during part of which he was a participant-observer in the Management Services division of the firm.

The study is of interest to political scientists because Dr. Pettigrew conceives of the organization as a political system (following Norton Long and adapting Eastonian concepts) and he treats decision making as a political process. The work of March and Simon (Organizations. 1958) and Cyert and March (A Behavioral Theory of the Firm, 1963) serves as the theoretical point of departure. Pettigrew criticizes March and Simon for making a theory of individual decision making into a theory of organizational decision making thus neglecting the impact of the outside world on the decision maker's perspective. He argues that although Cyert and March recognized organizational decision making as a political process, they paid insufficient attention to the determinants of conflict, the role of power, and the generation of support in the decision-making process. Pettigrew's objective is to complement the political approach of Cyert and March by examining the generation of demands and the mobilization of power in the process that led to Brian Michaels' innovative decision to acquire a more advanced computer.

Dr. Pettigrew's findings concerning the generation of demands are not surprising: "Demands are generated and processes in the context of social structures in which individuals are differentially located and have, by implication, access to varying amounts of resources that are the bases of power.... [W] here a demand is voiced, who articulates it, and how it

is diffused are all crucial determinants of the way in which it is received" (p. 266). More important is the mobilization of political support without which a demand fails. Pettigrew concludes that the mobilization of support is a function of strategy and structure which are linked by perception. Those who understand the structure of an organization can use it to their advantage in formulating strategy. In addition to structural position the mobilization of power by a decision maker depends upon: control over information flow in the decision process; political access to superiors; the extent of his/her role set; his/her assessed stature (by superiors and subordinates); and the strategies of relevant extra-organizational actors.

Dr. Pettigrew's study produces three additional results worth noting. First, he demonstrates the dynamic character of organizational decision making as reflected in the elaboration of social structure and the fluctuation of key variables over time. Organizational power relations tend to be unstable and the power balance changes, especially during an innovative period. Second, he reveals the political character of innovative decision processes. Innovative decisions are a source of conflict as they affect the distribution of resources. Even if the resources are expanded (he does not see power in zero-sum terms) and the power of all decision makers increases, the uncertainty resulting from the allocation of incremental resources will provoke "political behavior" (p. 273).

Finally, Pettigrew provides a perspective on organizational innovation that differs from the predominant orientation of those who have studied it most intensively, social psychologists and most organizational development (O.D.) advocates (e.g., Lewin, Likert, Bennis, and Slater). They view change positively as a desirable event or a goal to be achieved and managed through more sensitive human relations on the part of the leadership. Pettigrew sees change as a neutral happening (in this study a response to an external stimulus) that produces conflict and results in internal politicking and cannot be neatly managed to the satisfaction of all whom it affects. Surprisingly, he does not appear to recognize the extent to which his analysis departs from the O.D. perspective.

This book is a valuable contribution to the theory of organizational decision making, especially for political scientists. This raises the question of why our discipline has largely ignored the political nature of organizational behavior. Pettigrew is a sociologist and most of his references are to persons outside political science. He makes us aware of the wide utility of the theories and concepts developed in the

analysis of conventional electoral and governmental politics.

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Fascism and Dictatorship. By Nicos Poulantzas. Translated from the French by Judith White. (London: NLB and Atlantic Highlands, N.J.: Humanities Press, 1974. Pp. 366. £5.75.)

This is at once both an interesting and curious book. On the one hand it documents the significant changes that the "standard version" of the "Marxist" interpretation of "Fascism" has suffered at the hands of Marxist theoreticians since the termination of the Second World War. On the other, it reveals how far such commentators have yet to travel to provide a convincing account.

First and foremost, it becomes immediately clear that Fascism (and this refers to an indeterminate number of political regimes including at least a Spanish and a Japanese, as well as a German and Italian, "Fascism" [cf. p. 13]) is not to be conceived as the simple creature and agent of "finance capital" as it was characterized by the theoreticians of the Third International (pp. 72f., 88, 96f., 98, 110). Not only was Fascism not their creature, we find that "the big monied capitalists" frequently found themselves "discomfited" and "fettered" by the "relatively autonomous" fascist. regimes in both Italy and Germany (pp. 83, 85, 88, 96, 110-113, 134f.). Moreover, in the author's judgment, Fascism-rather than being unremittingly "reactionary"-displayed some clearly "progressive" features: in Germany, for example, "industrial recovery" was "stronger than anywhere in the world." Between 1933 and 1939 industrial production in Germany "had more than doubled." "As for Italy, industrial recovery between 1922 and 1929 was the strongest in capitalist Europe." Fascist Italy's recovery after the crisis of 1929 "was quite spectacular" (p. 99). Nor were these achievements restricted to industrial production. Poulantzas is quite explicit: "... the massive introduction of capitalism into agriculture," we are informed, "produced some spectacular results in Italy-notably the yield of cereal production ... " (p. 119). There was also a significant reduction in unemployment (pp. 191, 220) and an expansion of social welfare legislation (p. 221). Moreover, the standard of living of industrial workers did not fall as a whole, and in some cases significantly improved under Fascism (pp. 192f.).

The "standard version" of the Third International had informed us that Fascism was the simple tool of "finance capital." Now we learn that Fascism was far from a simple device. Fascism, we are told, was a "means" for the "establishment" of the hegemony of "big capital" that somehow "confined," "fettered," and "antagonized" that same "big capital" whose hegemony it served!

Where the theoreticians of the Third International informed us that Fascism "wound down the forces of production" to the "stagnant levels" required by "monopoly capital," Poulantzas tells us that Fascism (still somehow in the service of that same "monopoly capital") accelerated industrial and agricultural production. Where the Third International told us that Fascism produced pandemic unemployment and a grievous decline in the conditions of the working class, Poulantzas informs us that Fascism reduced unemployment, maintained stable wage levels, and in some cases significantly improved them. None of which should really surprise us because, as Poulantzas tells us, the theoreticians of Stalin's International were sadly mistaken about the "essence" of Fascism (pp. 18f, 36-52, 58f.). But then, in Poulantzas' judgment, everyone was mistaken about the "essence" of fascism. The reason seems to be that one one had, at that time, the benefit of "Mao's thought"!

Poulantzas seems to think that the "thought of Chairman Mao" provides some precious insight into the entire complex phenomenon (pp. 140, 228-230, 271). To be sure it is not very clear what these insights are, but they seem to turn on what might be called the "metaphysics of class struggle." Thus we learn that the history of the Soviet Union itself is to be understood only by recognizing the role of the "soviet bourgeoisie" in the "class struggle" that continues in contemporary Soviet society (pp. 230-33). If contemporary Soviet history is to be explained in terms of "class struggle" then it is not surprising that fascist history must be similarly interpreted. Fascists, capitalists, and the present leaders of the Soviet Union all serve "bourgeois interests" and invoke a "bourgeois ideology." Anyone who is "elitist" is surely "bourgeois." Anyone who is "nationalistic," or "militaristic," or "anticlerical"-or who emphasizes the special ideological role of education, or who is "anti-intellectualistic"-is clearly given to "bourgeois ideology" and must, in some sense, be serving bourgeois interests (pp. 254-56).

Only convinced "Marxists" seem to be capable of making such neat distinctions with such assurance. That Maoism, a "true prole-

tarian ideology" serving proletarian interests, is neither elitist, nationalistic, militaristic, anticlerical, "thought-reformist," or "anti-intellectualistic" is accepted without the least reservation. Contemporary "Marxists" seem to be gifted with that special insight that permits them to distinguish between a "proletarian" nationalism, a "proletarian" militarism and a "proletarian" elitism, and those that are "bourgeois." Thus, Marxists, unlike almost everyone else, never have difficulty in sorting out the species traits of "fascist" from "Marxist" authoritarianism, "fascist" from "Marxist" totalitarianism.

In effect, the principal merits of Poulantzas' book arise out of its recognition of the developmental character of fascist ideology, Fascism's multiclass recruitment base, and Fascism's "relative autonomy" from its putative "class sponsors." Its shortcomings are those generic to books written from a "Marxist perspective": the assumption of conspiratorial politics; the convictions that "classes" have specific "class ideologies," and that political regimes are actually the by-products of identifiable "class interests." There is far too much documentary material available that details the relationship between fascist political regimes and the organized class agencies in their respective societies to permit such arguments to pass without serious reservations. Poulantzas' grudging admission of Fascism's autonomy, as well as his recognition of Fascism's mistreatment of its putative beneficiaries, goes a long way toward laying the ghost of the "Marxist interpretation of Fascism." That Poulantzas seems unaware of his achievement provides some insight into the nature of ideological thinking.

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In Defense of Political Philosophy: A Reply to Robert Paul Wolff's "In Defense of Anarchism." By Jeffrey H. Reiman. (New York: Harper Torchbooks, 1972. Pp. xxxii + 87. \$1.95, paper.)

Reiman's book is a polemical critique of Wolff's much-discussed *Defense of Anarchism* (New York: Harper Torchbooks, 1970). Written with verve, it is readable, even agreeable, in style. The author summarizes his argument at a number of points, particularly in the preface and long introduction (especially pp. xxvi—xxxi).

Reiman's main argument is that Wolff first

identifies the political with the moral realm, political authority with moral authority (pp. xxii, xxiv, 36-37, 44, 48), and then goes on to argue that the fact of a moral authority (i.e., one which could issue morally binding commands) would be incompatible with the moral autonomy of individual persons (pp. xxi, 35, 49, 79). Reiman asserts, however, that the very notion of a moral authority is incoherent (pp. xxii, 4, 9, 51, 62). Accordingly, it is simply pointless to attempt to reconcile such authority with moral autonomy. Hence, Wolff's mapping of political authority onto the unthinkable notion of moral authority serves simply to create a straw-man issue.

Once Wolff's problem is exposed as a pseudo-problem (pp. 6, 61), and thus dissolved, we are free to ask whether a political system could be morally legitimate (or justified). This is really the question whether a given set of laws (with a specified form of official coercion) tends to "yield more morally worthy results" than can be expected from (1) the absence of any laws or from (2) other practicable alternative political systems (p. 32). This question must be asked by reference to the "general foreseeable effects" (p. 32) of such systemsand of their total absence. Legitimacy, when it can be established, provides prima facie reasons for obeying particular laws; but these reasons can be overridden by other moral considerations in a particular instance (p. 35). On this view moral autonomy is not sacrificed; for the agent can always decide, on a given occasion and for moral reasons, not to obey a morally sanctioned civil law, e.g., a traffic law (see especially pp. 37-39). However, there is still a general presumption in favor of complying with laws, given the overlap, in general, of the effect of these laws (in the behavior mandated) with morally good results (pp. 35, 43, 46, 53-55).

Moral autonomy pertains to individual occasions, when we weigh our moral reasons and decide what we ought to do; political authority pertains to the overall tendency of the laws to be good-producing on *most* occasions. Hence the two notions can be reconciled (see pp. 41, 42–46, 80). Indeed, civil disobedience is the classical example of their reconciliation at the point where moral autonomy and legitimate political authority do conflict (pp. 57–59).

Now that I have summarized Reiman's argument briefly, I have three questions. How accurate is his interpretation of Wolff? How telling is his critique? What contribution has Reiman made to political philosophy in his constructive account of the nature of political authority and of the moral presumption in favor of obedience to law?

I think Reiman misinterprets Wolff on a fundamental point. He represents Wolff's position as based on denying any so-called executive moral authority (e.g., God), whose very commands themselves constitute moral duties (pp. xx, 1, 36, and also 45, 54). But Wolff is not concerned with whether governments can create moral duties ex nihilo. His point is the rather different one that we have no moral duty to obey laws as such (as distinct from duties to do any good thing, whether mandated by law or not).

Wolff is a rule man (in that all our moral duties are specified by *strict* rules). Since he believes we• have no *strict* obligation to do whatever the law says, simply in virtue of its being valid law, he concludes that laws qua laws have no special or peculiar moral standing at all (unlike promises, contracts, lies and so on). Obedience to law, insofar as the matter does come under moral scrutiny, is not categorically imperative and, hence, is not one of our moral duties at all.

Reiman, on the other hand, has no place for these strict rules, even about such things as promises (p. 55), in his account of duties. He is a rather standard sort of utilitarian (see p. 38). Keeping promises usually has "morally valuable results"; hence we have developed a moral maxim (that says they should be kept) which can, nonetheless, be overridden on a given occasion. It is no different with laws. In a legitimate state the laws have a tendency to "morally good results" (indeed it is this tendency that actually legitimates such a state in the first place [pp. 56, 65-66]). Here "Obey the Law!" is a moral maxim; but like all such maxims, though it points to "morally good results," it is not a strict or absolutely imperative rule (pp. 54-55, and also 9, 43).

This brings us to an interesting point of convergence in the two positions: we may have obligations to the law but these can never be strict ones (which is what Reiman affirms); none of the strict obligations we may have can be obligations to the laws per se (which is what Wolff affirms). We can only conclude that we have no strict political obligations at all. This is important. For if we make strict political authority turn on it (as did the great "obligationists" in the history of political philosophy, such as the Socrates of Plato's *Crito* or Hobbes or Locke or Kant), then to deny strict political obligation is to deny political authority.

It is the perception of this fact that has been seized on by Wolff in his *Defense of Anarchism*. And, since Reiman has mislocated Wolff's animus, as being against "executive moral authority" (the phrase is De George's), his basic

critique of Wolff is largely beside the point. Reiman accordingly fails to see that Wolff's strictures really are telling against traditional justifications of political authority which rely on grounding that authority in the citizen's strict obligation to obey the laws issued.

The implications of this "strict obligation" view (which Wolff's anarchist shares with those who affirm an obligation to obey the law) are equally striking: they serve to reveal an important pattern in the logic of justifying political authority. For where obligation is the prior issue, as it is in the perspective under discussion, we cannot ground that obligation by reference to the other political concepts (such as the capacity to issue rules). Rather, the task of justifying the basic political notion, whatever it is (and in the case at hand it is our strict obligation to obey laws), requires that we go outside the entire system of political concepts. Hence, the justification of political authority, whether it can be accomplished or not, must be attempted by reference to some external, nonpolitical standard.

The question, then, becomes whether Reiman offers a significant alternative to this externalist program for justifying political authority. I would suggest that he does not. His argument remains externally grounded in that he simply identifies justified political authority with morally justified political authority (see especially pp. 44–45, 53–55). Authority proper attaches not to the lawmaking role or to the political institutions in which it is embedded but to the external ground, to the "morally worthy results" (as determined by some superordinate ethical standard).

Reiman's book, then, is a conventional defense of political authority on moral grounds. It opens no new territory and stays well within the traditional argument pattern, as found in the externalist program for justifying political authority.

REX MARTIN

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The Study of Government: Political Science and Public Administration. By F. F. Ridley. (New York: Crane, Russak, and Co., 1975. Pp. 240. \$9.00, paper.)

The title of Professor Ridley's book is deceptive, for *The Study of Government* is neither an introductory text nor a systematic advanced treatise on government. Rather this book consists largely of a number of the author's essays that have previously been published within the last ten years on various

aspects of public administration and political science. As with any book made up of splicedtogether essays, the style is uneven and the theme wanders; hence, the book is likely to exasperate a careful reader who wants more details at certain points, tighter logic to reinforce speculation, and more polish given to arguments which at places are repetitious or half developed. Yet, as Professor Ridley admits at the outset, his work is "a polemic" that "advocates a certain approach to the study of politics and public administration." The thesis the author vigorously presses throughout is: "The focus of study [of government] ... should be institutional, the method comparative, the purpose practical."

The first half of the book is devoted to examining political science as an academic discipline and here the author argues for political scientists to return to a "more formalistic and utilitarian approach" and move away from the behavioral and value-free scholarship. This theme no doubt will sound old-fashioned to many contemporary American political scientists who now by-and-large recognize the 1950s behavioral revolution as having been won and accept the existence of the so-called "post-behavioral" or "post-post-behavioral" era where presumably the best of both "traditional" and "behavioral" are blended and utilized. The recent growth of the policy analysis schools and public administration programs that combine both the behavioral and the applied, best reflects this "post-post-behavioral" trend in America. Even though British Professor Ridley's message may have arrived too late to be meaningful for American political scientists, his vigorous "polemic" for the traditional at least can be used to provoke plenty of lively debate among undergraduates in introductory methods courses.

The second half of The Study of Government deals with public administration and here the author is on firmer ground for this field has been his specialty for some time. Professor Ridley has published several noteworthy books on European public administration, including Public Administration in France, and Specialists and Generalists. Students in public administration will especially find the second half of Ridley's book useful when placed alongside several recent American writings such as Vincent Ostrom, The Intellectual Crisis in American Public Administration; Frederick C. Mosher (editor), American Public Administration: Past, Present, Future; Frank Marini (editor), Toward New Public Administration; or Dwight Waldo (editor), Public Administration in the Time of Turbulence-all of which wrestle

with the central issues: what is public administration? and what should it become? Unlike, for example, the American theorist Vincent Ostrom, who believes that American public administration should cut its ties with its Wilsonian-Weberian past and undergo a "radical paradigm shift" by adopting contemporary public choice theories, British Professor Ridley argues for the discipline to focus on the pragmatic issues of institutional reform-an approach that stands very much in the mainstream of American public administration doctrines dating from Wilson, Whites, and Willoughby. Here again, this portion of Ridley's book which stridently advocates the "traditional" should stimulate provocative debate in introductory administration methods courses, even though the nagging question is never really confronted by the author-why, given all the interesting varieties of models, paradigms, and approaches offered in the field today, is the "traditional" the soundest model for public administration to adopt? Many readers will find Ridley's vision of public administration uncomfortably restricted given the diversity of administrative methodologies developed within the last 30 years.

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Sozialdemokratie und Marxismus: Zum Spannungsverhältnis von Godesberger Programm und marxistischer Theorie. By Alexander Schwan and Gesine Schwan. (Hamburg, Germany: Hoffmann und Campe, 1974. Pp. 397. Price not listed.)

The purpose of this book by Alexander and Gesine Schwan, professors of political science at the Free University of Berlin, is to examine the relevance of Marxism for current social democratic theory in Germany. The authors discuss the intellectual foundations of the SPD's 1959 Godesberg Program, criticize Marx, Lenin, and contemporary Marxist system theorists, and finally develop some of their own ideas about democratic socialism. Taking their own positions from the Aristotelian and Christian-humanist tradition, the authors focus their criticism and polemics on the young socialists (Jusos) in the Social Democratic Party, whom the Schwans regard as being dominated or at least highly influenced by various currents of Marxism. They conclude that Marxist theory has to be considered irrelevant for the positive formulation of a theory of democratic socialism, because even Marx himself in his conceptual formulations

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and practical intentions became a traitor to and rejected his original ideas of emancipation and realization of human potentials (p. 166).

For the reader who followed the authors' argumentation up to this point it is surprising that in the final chapter Yugoslav and Czechoslovak reform Communists are presented as potential contributors to a theory of democratic socialism. There can be no doubt that reform Communists in Yugoslavia with their municipal and industrial self-government or the initiators of the Dubcek spring of 1968 turned Marx against Lenin and Stalin by actualizing certain fundamental democratic and human principles implied in Marxism. But in pointing to these modifications of communist practice the authors contradict their own line of thought by advocating Marxist theorems in reform communism while condemning them for German young socialists.

Furthermore, the Schwans neglect to mention those Marxist-reformist traditions which influenced the Godesberg Program and which are-in the Hegelian sense of the wordpreserved in this still-valid declaration of social democratic principles. Among the best known examples are Rudolf Hilferding's concept of organized capitalism and Fritz Naphtali's notion of industrial democracy (Wirtschaftsdemokratie). To have responded to writers like Otto Bauer and Karl Renner (both Austro-Marxists), to Max Adler, Max Seydewitz, Arkadij Gurland, Fritz Sternberg, Richard Löwenthal-Sering would have meant a real challenge to the Schwans, since these oftenforgotten theorists influenced social democracy not only in the Weimar Republic but in the Federal Republic as well. Their arguments are more coherent and on a higher analytic level than are those of contemporary young socialists. As a result their contributions to a theory of democratic socialism should not be ignored, rather they should be regarded as a field for fruitful social, economic, and political analysis.

Despite such reservations, I think the Schwans have done a very creditable job of promoting a hot discussion on democratic socialism in Germany by defining their own position and turning it against that of the young socialists.

PETER LÖSCHE

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Social Policy: An Introduction. By Richard Titmuss. (New York: Pantheon Books, 1975. Pp. 160. \$8.95.)

It is of great importance to establish at once what this book is not. It is not really a book (if by book we mean a unified and coherent work), nor is it really written by the late Richard Titmuss, the widely known and respected social philosopher and both proponent and critic of the British welfare state. Instead it is a series of Titmuss' lectures prepared for his Introduction to Social Policy course at the London School of Economics and compiled and edited after his death by his wife, Kay Titmuss, and Brian Abel-Smith. Despite a fine effort by the editors, the work suffers from its form on two accounts: as a series of rather disconnected lectures it lacks a coherent theme, and as an introduction designed for first-year graduate students it lacks the analytical sophistication found in Titmuss' previous works. Nonetheless the work is worthwhile as a distillation and reiteration of Titmuss' thinking, reflecting the tremendous insight which has come to be expected of him and reflecting, as well, some less readily acknowledged limitations.

His concern is with the subject of social policy, an academic discipline much more common in England than in the United States. where it appears to exist, if at all, as a subfield in social work schools. "Social policy" is a difficult term to come to grips with; it appears to be one of those terms which everyone knows the meaning of even though no one can define it. Titmuss' best effort (p. 49), though lacking in precision, conveys the concept in a general sense: "Social policy is basically about choices between conflicting political objectives and goals and how they are formulated; what constitutes the good society or that part of a good society which culturally distinguishes between the needs and aspirations of social man in contradiction to the needs and aspirations of economic man."

Studying social policy does not, however, necessarily imply (though it does not rule out either) studying how decisions are made or the objective analysis of the social impact of decisions. Instead social policy as a discipline (profession?) is unabashedly concerned with normative questions of social change and redistribution as well as analytical questions of the impact of various policies and programs on these normative concerns.

Titmuss repeats the basic concerns of his previous works: the distinction of social man from economic man; the inhumane consequences of private market mechanisms and institutions; the importance of the "gift relationship" in social policy; and the backwardness, if not barbarism of the United States in matters of social policy and institutions.

As before, he does not concern himself with the problem of how the universalist public and social welfare services he advocates are to be financed. Nor does he deal adequately with the problem of accountability in the public sector. He argues (p. 55) that, since the discipline of the market is not present, other kinds of discipline must be fashioned for the public sector and its personnel: "more education, training and in-training; an incorruptible, secure civil service, developing its own codes of behavior, rule-making, and procedures, and continually pre-occupied with problems of equity and fairness as between one citizen and another; the use of 'quality controls' through Parliament, investigating committees, research, inspectorates, pressure groups and so forth. In short, if social services are to be delivered effectively, equitably and humanely, more and better educated social administrators are required." This is a singularly lame and unconvincing response to the problems of bureaucracy, begging, as it surely does, the question of how one trains administrators to be "continually pre-occupied with problems of equity and fairness," and also ignoring the institutional settings into which these well-trained civil servants will be placed and the resulting dynamics to which they must respond.

Titmuss' insensitivity to the accountability question is further illustrated in his discussion of the Gary, Indiana, education performance contracting experiment which he criticizes as an example of the American mania for private markets and the reprivatization of even previously accepted public services. He observes that "the American arguments in favor of introducing criteria of profit into the educational system are that (a) teachers work harder, (b) fewer teachers are needed, (c) parents are brought into participation, and (d) the teaching profession is brought under control and thus there is less professional bias in favor of the bright, higher I.Q. white child" (p. 41). Nowhere does Titmuss mention the primary-and often stated-rationale for education performance contracting, namely, that they are a means of improving educational quality by holding schools accountable for their results.

In many ways Titmuss is at his best in his broad-scale global analysis of U.S. social policy which he characterizes as "Model A, the residual welfare model" as opposed to "Model C, the institutional redistributive model" which characterizes—or he would like to see characterize—the United Kingdom. Unfortunately he is at his very worst in dealing with specific examples from the American context such as the one quoted above. Some of his examples, used as a

foil more than anything else, are little short of outrageous as when he observes (p. 43) that "few black people in the U.S.A. can afford to pay the price of private medical care in hospital (now probably approaching something like £500 per week)." It should not be construed as an apology for the present American health system to note that private health insurance, medicare, and medicaid undoubtedly enable the majority of blacks—and whites—to obtain hospital treatment for out-of-pocket sums a fraction of the total charges he cites. The portrayal of the United States in this, as in his other books is too stark, inflexible, one-sided, not to mention inaccurate.

This is all too bad. There is enough wrong with U.S. social policy and enough good sense in what Titmuss has to say to make these kinds of embellishments unnecessary.

HAROLD WOLMAN

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Critique of Earth. By Arend Theodoor van Leeuwen. (New York: Charles Scribner's Sons, 1974. Pp. 295. \$10.00.)

This is the second series of Gifford Lectures delivered by van Leeuwen in 1970 and 1972, respectively. The first series, entitled *Critique* of Heaven, was published earlier and is in fact already out of print. It is a pity that the two series do not appear in a single volume; they are much more a continuum than a contrast, and some of van Leeuwen's initial remarks in *Critique of Heaven* help explain his project with an explicitness that does not recur until the final chapter of the present book.

The subject of the lectures is the thought of Marx. Van Leeuwen is a theologian by trade. These facts might lead one to anticipate an exercise in theologizing Marx, and indeed an element of this exists; for one thing, certain of Marx's writings have never before been so closely scrutinized for their religious allusions and redolences as they are by van Leeuwen. But the author's more prominent aim is to "Marxize" theology by presenting Marx as a pivotal figure in the history of that subject and an indispensable guide for contemporary religion. Since the bulk of van Leeuwen's work consists of textual exegeses of Marx's writings and embodies some of the best qualities of that kind of approach, the book is of considerable value even for those political theorists who may share none of van Leeuwen's concerns as a theologian.

The fitles of both lecture series are derived from a famous passage in the Introduction to the Critique of Hegel's Philosophy of Right in which Marx stresses the theoretical necessity, as he sees it, for transmuting the critique of "heaven," i.e., of religious notions, into that of "earth," that is, of political economy and of the socio-economic structures for which it furnishes ideological support. Van Leeuwen's division of labor between the two books does not, therefore, conform to the most standard chronological division between the "earlier" (pre-1845) and the "later" (post-1845) Marx, according to which the Theses on Feuerbach constitute, to some degree or other, a turning point. Rather, the bulk of Critique of Earth, like all of its companion volume, concerns various earlier Marxian writings. Only in the final chapter does van Leeuwen finally succumb to the temptation, overwhelming for a theologian with an interest in Marx, to analyze the "Fetishism of Commodities" section of Capital, in which Marx himself drew lengthy analogies between the religious sphere and that of political economy-and even then he does so only very briefly.

The book contains chapters about Marx's newspaper articles on the new Prussian draft legislation against wood-theft and concerning divorce, about Hegel's political theory and Marx's critique of it, and about some themes in Marx's Critique of Political Economy and Capital. One of van Leeuwen's most useful contributions is his systematic analysis of the pejorative connotations assigned by Marx to the terms "materialism" (as in the biblically based expression, "verworfene [reprobate] Materialismus") and "material interests" (of the landlords), in his attack on the proposed wood law. This legislation was a dramatic example of the historical transition from feudal to capitalist relationships (the landholders had become aware of the commercial possibilities of the dead wood that it had formerly been the peasants' custom to glean), and van Leeuwen admirably shows the genesis of some of the mature Marx's major themes in Marx's groping attempts to understand the broader implications of what was occurring in 1842.

On the other hand, Chapter 8, entitled "Money as Mediator," illustrates some of the possibilities of excess inherent in van Leeuwen's exegetical approach. It is true that Marx, early in Volume I of Capital, speaks of the "transubstantiation" of commodities into gold and cites three lines from Dante's Paradiso by say of making a slightly humorous point about the need to have real money in one's purse in order to enter into an actual exchange transaction. It

is also true that Marx cites Dante on two other, more familiar occasions in his later work. But van Leeuwen's excursus on the "coinage" of faith, which was at issue in the original Dantean verse, leads to interesting, brilliant, but unnecessarily complex extensions of the original analogy that go far beyond what it is reasonable to impute to Marx's train of thought when he wrote it down. In van Leeuwen's defense, of course, it could be argued that he is simply drawing systematic interconnections among regions of Marx's thought about which Marx himself, who had obviously absorbed so much from so many cultural traditions, was not fully conscious.

In his final chapter, van Leeuwen returns to an attack, initiated at the beginning of Critique of Heaven, on Karl Barth-first for his failure to include Marx in his history of nineteenth-century theology (even while Barth assigns a very prominent place to Feuerbach), and then for the narrowness, compared to Marx, of Barth's own theological outlook. Van Leeuwen finds it unacceptable to undertake critical theological thinking today without engaging, as part of that thinking, in political and economic analyses. When all is said and done, says van Leeuwen, "Barth's is indeed a 'Swiss theology' par excellence" (p. 269); nowhere does one find him raising questions about the basic political and economic structures of the country of which he was a citizen. Consequently, Barth and his many admirers miss Marx's most basic conclusion about modern Christianity, namely, that it is the most adequate form of religion for the bourgeois state.

Unless theology is capable of passing through both aspects of radical Marxist criticism, the "heavenly" and the "earthly," according to van Leeuwen, it has no future. His conclusion is an easy one with which to agree, it seems to me, but there is certain to be disagreement as to whether or not one finds it troubling. Van Leeuwen clearly does, while Marx, it seems plain to me, did not. Although van Leeuwen does not make any systematic use of the expression, "political theology," as a label for what he is advocating, I found his book reviving in me the old questions that arose when I once had some involvement in a course in that putative subject-area: is there really such a thing? and can there be such a thing?

At any rate, persons with serious interests in religion or in political theory or in both are bound to derive great profit from reading van Leeuwen's careful, scholarly book—whatever the intellectual coinage in which they choose to draw their profit! Critique of Earth is also a direct contribution to Marxist-Christian dia-

logue, much richer in content than many of those metacontributions that confine themselves to talking about the fact either that such a dialogue is taking place or that it *could* take place.

WILLIAM LEON MC BRIDE

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Roscoe Pound: Philosopher of Law. By David Wigdor. (Westport, Conn.: Greenwood Press, 1974. Pp. 356. \$12.95.)

In some ways the career of Roscoe Pound is a bit of an embarrassment to the law. The road from St. Paul and "The Causes of Popular Dissatisfaction with the Administration of Justice," to Harvard and praise for Stolberg v. California is hardly an inspiring one. Conceivably, Mr. Wigdor agrees, for his story seems to run downhill when, after an exciting sojourn in Chicago, Pound leaves for Harvard. Suddenly the human Pound who could rouse the entire student body at Nebraska to torchlight protest parades at the word of his impending departure becomes a cardboard character.

I suspect that the story need not have been told that way. Leon Green reported to me that Dean Pound, for all his orotund pomposity, was generally beloved by the bar for what were known as the "sonofabitch" stories and for an ability to drink most any bar luminary under the table. And, arguably, the Harvard years brought his greatest achievements: a Supreme Court—indeed a world of legal scholarships—that used "interest analysis" as a daily tool of its work, and a corps of loyal graduate students whose views stretched the ideological spectrum from the Lawyer's Guild to the Liberty Lobby.

The Pound who could have seen triumph in his declining years is, however, Pound the Dean, not Pound the Philosopher of Law. The choice to emphasize the one and not the other, as Mr. Wigdor has done, is plausible. Pound was known as a philosopher, whether or not he was much of one. But I for one would have preferred to know more about Pound at Harvard. Possibly that preference reflects my narrow, law school bias, but it also reflects a deficiency in Mr. Wigdor's analysis and understanding of Pound's career and jurisprudential work.

Mr. Wigdor argues that there are two threads to Pound's thought—a sort of Hegelian, but not idealist, organicism and a Deweyan instrumentalism. The case for this continuing dualism is a good one, but Pound's Harvard years suggest that there were some regularities in his actions

that Mr. Wigdor's analysis simply does not explain.

Examples are easy to find. Consider that Pound was brought to Harvard with a national reputation as a progressive reformer, was made dean three years later, and yet his philosophy, indeed even his concern for the administration of justice, apparently never led him to make an alteration in the routine of educating law students. Or wonder how, between the Cleveland Crime Survey of 1927 and the Anti-Realist Manifesto of 1931, fact research changed from something good to something bad. Or puzzle over what happened between the red scare "Heresy Trial" at Harvard in 1921, in which Pound stood among the accused, and the Wigmore/Frankfurter controversy over the Sacco-Vanzetti case in 1927, in which he stood on the sidelines. To me the pattern is plain. Something made Pound sour on the remnants of progressivism that began to reshape old doctrine after World War I, for it is apparent that whenever Pound did not start out on the Right, he moved that way.

Mr. Wigdor does not note, much less explain, this pattern, but in avoiding explanations of the regularities in Pound's actions, he does no worse than his subject. For both men decisions are wrapped up in a mysterious balancing process in which results are self-evident, at least in retrospect. Thus, if Pound resolves a problem one way it is because one or the other of the dual elements dominated, just as, given the outcome of any case decided by today's interest-analysis. Supreme Court, a competent constitutional scholar can guess the interests and the weight given to each. But if that same scholar tries to intuit the interests, not knowing the result, that scholar will find him- or herself talking about Mr. Justice Burger's values, for it is those values that seem to control the choice and weight of interests that, once known, mechanically determine results. Thus in practice Pound's philosophy is literally a formal mechanism in which the decisive questions-what interests of what weight-are hidden from view. Mr. Wigdor does not understand this fact as is made obvious by his failure to reach behind the formal dualism of instrumentalism and organicism to try to understand the values that determine which side of Pound's own balance compel his solution to a problem, And, since it is in Pound's role as dean that both the nature of his philosophical system and his values become simultaneously apparent, it is understandable why Mr. Wigdor slights this period in Pound's life.

But to explain is not to justify. If the system virtually drops out in determining results, what

was its impact and thus the impact of Pound the Philosopher? Surely the answer to this question is to be found in the largely untold story of Pound the Dean for, if the system made little difference, what made Pound significant in the eyes of his colleagues and students, was his role as a Harvard Law School institution. That role we still do not know enough about.

JOHN HENRY SCHLEGEL

State University of New York, Buffalo

The Psychology of Conservatism. Edited by Glenn D. Wilson. (London and New York: Academic Press, 1973. Pp. 277. \$13.50.)

Here is an attempt to establish the concept of conservatism as a more or less one-dimensional, centrally organizing element of personality. It comes upon the scene at a time when the concept of political-economic conservatism, at least, is under attack: Converse has shown that it is not an organizing concept for the mass public: Rokeach has argued persuasively and with evidence that it comprises not one dimension but two (equality and liberty); Schoenberger has shown that for certain practicing conservatives it is indeed an organizing concept, but is in no way related to the personality characteristics that McClosky said informed and guided the conservatives he measured; the general attack on the methods of The Authoritarian Personality (ignoring its validity) has infected the belief in the relationship between authoritarianism and conservatism, and so forth. Thus, although Mr. Wilson and his associates are not joining issue with some of these critics of the concept (indeed they agree with the critics of The Authoritarian Personality), they must do some persuading to revive conservatism as a viable, single-voiced concept in socio-personality research.

Nor do they help their case by their implied and sometimes explicit metaphysics: there is something out there that is conservatism, if we could only devise the right instrument to capture it. Fortunately, this metaphysic does not impair their research (only their language about it), for the book is built around a simple 50 item "C" (for conservatism) Scale, from which all confusing prose is removed and the respondents simply circle "yes" or "no" or, "if absolutely uncertain," place a "?" next to the item itself. Some of these items are "death penalty," "evolutionary theory," "coeducation," "white supremacy," "computer music," "disarmament," and "Bible truth." It will be

seen that these items embrace much more than political conservatism; indeed, one of the aims of the authors is to show the generality of the dimension and thus to be able to place political preferences within this larger construct, and so to help explain them. In this they have some success, partly, I think, because their conceptualization of conservatism is both broader and better than the concepts used by analysts of belief systems in mass publics.

To do this, of course, they must show the interrelationship of the items to each other to reveal the way they hang together. Under the scrutiny of factor analysis they can show that there is one general factor loading, which they label, appropriately enough, "conservatism," and several less embracing dimensions: "realism (versus idealism)," "militarism-punitiveness," "antihedonism," "ethnocentrism and out-group hostility," "religion-puritanism." As might be expected from these labels, the C-Scale is positively related to F-Scale, dogmatism, and ethnocentrism scales, giving some further plausibility to the authors' contentions that there is a great deal of commonality in the measures and concepts, but many will doubt that these concepts "are all orderable along a single dimension, best called liberalism-conservatism" (p. 33).

The articles are highly pragmatic, often devoid of even those limited "mini-theories" beloved of American psychologists (dissonance, attribution, equity, transaction, etc.), and steer a wide circle around that kind of major informing theory that graces (or deforms) The Authoritarian Personality. But there is an attempt to explain conservatism itself in an all too brief, interesting, theoretical chapter by Glenn Wilson (the author or co-author of eight of the 17 chapters). "The conservative attitude syndrome," he says, "is a generalized susceptibility to experiencing threat or anxiety in the face of uncertainty" (p. 259). It has sources in such genetic factors as "anxiety proneness" and "stimulus aversion" (sic), as well as low intelligence, female sex, and old age, and in such environmental factors as "parental coldness, punitiveness, rigidity," and lower class membership. These combine to create feelings of insecurity and inferiority, which, in turn, produce a generalized fear of uncertainty, expressed either as avoidance of stimulus uncertainty (dislike of innovation, etc.) or avoidance of response uncertainty (lack of self-reliance and search for external authority). Both kinds of uncertainty avoidance are then expressed in the conservative attitude syndrome which guides responses to particular issues such as those expressed in the C-Scale. The theory has a

beautiful simplicity to it, seems to account for much of the evidence (indeed, is constructed from the findings in an inductive manner), and is close enough to observables to allow for further testing of the sources, as well as the consequences, of the conservatism measured.

With this apparatus, what do the authors discover? Children's conservatism is different from adults in that conventionalism (good behavior syndrome) is negatively related to ethnocentrism. (In line with the general "closeto-the-data" posture of the work, the author of the piece [Nias] does not examine how it is that these relationships become reversed in the process of growing up.) Among clergy, theological conservatism is part of the larger conservatism syndrome (Wilson and Bagley). In an interesting study of superstitious behavior (avoiding walking under a ladder), Boshier finds that it is the younger liberals and the older conservatives who behave in this inferred superstitious manner. He speculates that, after the counterculture, superstition is more associated with liberalism than formerly was the case. As was known before, conservatives have aesthetic preferences for less ambiguous art (Patterson). And in direct confirmation of the theory, Kish finds that liberals seek stimulusrich situations and choose less conventional vocations.

The broad conceptualization of conservatism distinguishes this work from others in the area; the simplicity of the measure and the theory may have a reductionist tendency, but they have the great advantage of clarity. Perhaps the concept of a general factor of conservatism is on the way back; it might even help to explain some political conservatism. But it is important to remember that people are guided by their economic stakes and interests, as well.

ROBERT E. LANE

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American Politics

The Uncertain Search for Environmental Quality: A Case Study in the Failure of Modern Policy-Making. By Bruce A. Ackerman et al. (New York: The Free Press, 1974. Pp. 386. \$13.95.)

The study of environmental policy can generate significant personal frustrations and uncertainties. It also can highlight uncertainties about the performance of our political system. The Uncertain Search is an effort to investigate

the issues related to the latter without adding to the former.

Ackerman and his co-authors seek to "prepare the way for fruitful theorizing by carefully exploring an actual decision-making process which reveals in a paradigmatic fashion the nature of both the challenge and the opportunity that technocratic intelligence offers to a contemporary democracy" (p. 2). The case study analyzes the interrelationship between technocratic intelligence, political decision making and legal enforcement as exhibited in a "modern" governmental institution-the Delaware River Basin Commission (DRBC). The dilemmas of an interstate agency involved in complex technical policy area (water pollution) provide the focal point for this effort. Technical research, public involvement, cooperative federalism, and administrative decision making are scrutinized.

Two major problems receive emphasis: (1) the capability to link the results of comprehensive technical research into administrative decision-making processes; and (2) the ability of institutions to design and implement effective policy. The findings support the uncertainty of accomplishing either objective. The necessity of depending on poorly conceptualized technical research on water pollution, the absence of an in-house capability to review technical information, and the weaknesses interjected by the former in the implementation of policy are the focal point for the first problem. A discussion of effluent standards and pollution control strategies provides the medium for identifying and evaluating the second problem.

The findings of the study are not startlingly new. The DRBC, despite its "modern" design, suffers from common, contemporary problems. Quantitative research findings and recommendations are at odds with qualitative judgments on river use. A major DRBC cost-benefit analysis of alternative cleanup policies, compiled on the basis of poor information, did not provide effective standards for decision making. The expertise of decision makers and strong commitment of participating governmental and industrial actors could not produce consensus on policy objectives. Intergovernmental actors, representing divergent constituencies, could not generate a meaningful, basinwide community of interest despite the apparent commonality of problems. In short, "politics" reintroduced fragmentation and prevented the full integration of technical considerations with decision making. In this light, DRBC pollution control policy for the basic lacked the qualitative element of enforceability which would have made it effective.

Incomplete integration of disparate interests forces policy makers to ignore the demands of technical considerations. This breakdown in the linkage between technocratic intelligence and decision making appears endemic to contemporary governmental structures. It exists in many other policy areas. The authors raise a legitimate issue, yet, like many others, they suggest a solution (redesigning institutions) which does not fully appreciate the nature of the problem, particularly in the arena of environmental policy.

Historically, environmental policy has not exhibited the linkages desired by Ackerman and his co-authors. Policy has lagged consistently behind technological innovation, leaving the consistent impression of too little, too late. Incredibly swift social and technological change has made this defect unavoidable, rendering environmental policy obsolete on promulgation. Each change in existing policy (technological demands aside) has reflected a concern both for the effectiveness of policy and institutions and for new parameters/issues which policy should address. This latter aspect, reflecting shifts in policy maker perceptions of the nature and scope of the problem, is based partly in the need to bridge the technological lag. More importantly, it also reflects major shifts in political choices, preferences, and priorities. In effect, the definition of pollution has changed.

In light of these definitional changes, analysts have focused on technological and legalistic mechanisms as solutions to political problems rather than dealing with the political foundations of technology, policy, and law. The more fundamental issue of politics in a democratic society, and the unique pressures it raises for policy decision making, seem to messy or inelegant to deal with. Yet, it is here that root causes of "uncertainty" lie. The authors realize this, yet shy away from the political dimension of policy problems, and take refuge in the more mechanistic arena of designing institutions.

An example of this shortcoming can be found in the book's final chapter. The authors observe that the 1972 Federal Water Pollution Control Act poorly comprehends the technologically possible means of combating pollution. To them, "no discharge by 1985" represents wishful thinking rather than realistic policy making. To an extent they are right. Yet, this sort of policy goal also represents an attempt to make technology a servant of decision making rather than its master. The reversal raises a new "uncertainty." Can a democracy effectively formulate definitions of

policy problems and issues which permit it to act upon rather than react to technology and thus formulate effective policy/law? The last few years of air and water pollution control efforts raise serious doubts.

The lesson of the DRBC is thus not solely its failure to make effective linkages (the authors' "uncertainty"). Its additional lesson is that it is necessary to look beyond purely technical and technocratic approaches in program and institutional design to the foundations of political policy making in a democracy: Can meaningful policy goals be set to guide decision making? The authors seem to brush by this fundamental problem.

Aside from the substantive (de)merits of the book, it is also weakened by its structure. It is a patchwork of chapters authored by different groupings of the collaborators (p. ix). The interdisciplinary efforts lack full integration. Also, the period of time between research and writing (1971–1974) leaves many new developments unevaluated—a regrettable necessity. This book can benefit those who have never experienced firsthand the dilemmas of environmental policy. It is, however, only a limited beginning toward suggesting where we go from here.

SHELDON M. EDNER

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Health Care Politics: Ideological and Interest Group Barriers to Reform. By Robert R. Alford. (Chicago and London: University of Chicago Press, 1975. Pp. 281. \$12.50.)

Health Care Politics is a near-polemic analysis of the health care delivery system in terms of interest group politics. While such an analysis might seem quite conventional to a political scientist, it is novel within the health research community. So much so, that Alford's earlier article outlining his thesis created a considerable stir.

Alford contrasts his thesis with two other theories of health care reform, those propounded by the "market" and "bureaucratic" reformers. In Alford's view, the market reformers see the problem as solvable by market mechanisms, if the health care market were freed from the many restrictions on it, including both the government's heavy-handed regulation and the AMA's restriction on the number of physicians. The bureaucratic reformers, on the other hand, contend that since health care must be delivered in part by enormous institutions, the fragmentation of the existing system can only be overcome by forcing bureaucratic coordination on all the actors. The bureaucratic

reformers' efforts to coordinate the pieces of the health care delivery system have failed to improve the quality of health care for the poor; their failure, according to Alford, can be understood from a "'structural interest' perspective. Powerful interests benefit from the health care system precisely as it is—with its ineffective layers of bureaucratic 'planning' and 'administration' and its uncoordinated separate organizational and professional components responding to demands by the sick for care" (p. 6).

Alford's data consist of the reports and minutes of the half-dozen committees assembled to deal with the New York City health crisis, as well as reports by management consultant-type projects and the coordination committees required under various federal grants. The book looks briefly at other cities and concludes that New York is typical. It closes with a long theoretical essay explaining why the structural interests remain dominant in the contest.

Any judgment of this book must, of course, be a value judgment; one man's reform is another man's poison. I am inclined to see the overall structural interests argument as sound, probably because my biases are compatible with Alford's. The lengthy discussion of the work of the various committees is not very informative, however. The author's reliance on documents rather than on interviews prevents one from obtaining a clear picture of the permanent cleavages in the politics of health care in New York City. There are presumably a number of actors there and in Washington whose views of the health delivery system are close to Alford's. What conflicts have they weathered, and with what results? How does one conflict relate to the one occurring a few years later?

My main problem with the book is its heavy normative style. Not because I disagree, but because I suspect it obscures part of the truth. Alford concludes pessimistically that even when reforms work, they are undercut by the pervasive tendency of the system to restore itself to an equilibrium containing the same problems as before. He writes: "If the institutional or class analysis is even partly correct, then a much more fundamental struggle to change American social institutions is called for, and this struggle will require the emergence of a social movement and political leadership which is not yet visible" (p. 266). Maybe. But I am left wondering. I tend to reject the idea that New York's history is one of all failures. Perhaps a more systematic "objective" effort to sort them out might yield a more complex picture of when and under what conditions the Medical Societies and the Big Hospitals win and lose. This in turn might give us a more sophisticated understanding of the nature of the system.

Nevertheless, I hope that this book is widely read and debated within the health research community, wince it is capable of stimulating further needed research.

ROBERT L. CRAIN

The Rand Corporation

Guide to U.S. Elections. By Congressional Quarterly. (Washington, D.C.: Congressional Quarterly, 1975. Pp. xvi + 1103. \$47.50.)

The value of any reference book rests largely with providing easy access to useful information not readily available elsewhere. By these standards Congressional Quarterly's Guide to U.S. Elections is an outstanding achievement. We now have available from a single source exhaustive election data on presidential, congressional, and gubernatorial elections. (Southern gubernatorial and senatorial primary results are provided in a separate chapter.) The elections are compiled in a manner that facilitates brief forays into the volume in search of specific information. The editors have thoughtfully included candidate name indexes for each office, which are indispensable for tracing an intermittent political career.

The Guide is more than an occasional reference, however. For compulsive browsers who have tired of looking for Galbraith's Swiss chalet in their world atlas, the Guide offers a welcome change. For the first time we can celebrate some of history's most hapless creatures-the chronic losers. Ebon Moody Boynton passed through the bicentennial unnoticed except by the Guide's browsers. In his own way Boynton was a remarkable man. Between 1878 and 1898 he lost five attempts for Congress under three different party banners. Moreover, of his three parties only the Democrats managed to survive Boynton's candidacy. The 500 or so party labels worn by congressional candidates are also among the more interesting artifacts of the giant archaeological dig that resulted in this book. Who, we can ponder, were Missouri's Benton and anti-Benton Democrats, and why did their internecine warfare persist for six years?

Students of American political development have in the *Guide* a valuable set of data made more attractive by the fact that its contents are available on computer tape from the Inter-University Consortium at Michigan, Perhaps the

best way to describe its research attractions and limitations is with a sample of its contents. Consider the career of Edward Joy Morris. In 1843 Morris, running as the Whig candidate in Pennsylvania's 1st Congressional District, handily defeated two Democratic contenders with 45 percent of the vote. According to the Guide Morris failed to run for re-election. Nine years later in 1854, a Whig candidate with the last name of Morris lost to the incumbent Democrat by a three percentage point margin. Whether this was the same man is unknown, but it is clear that two years later Edward Joy Morris ran as the Union party candidate in the 2nd Congressional District and won by less than 2,000 votes. After serving two terms as a Unionist, Morris then switched to the newly formed Republican party and doubled his margin of victory over his Democratic rival despite the fact that the Union party fielded a candidate. Here his congressional career ended, and the district reverted to Union control.

What do we know about this politician that we did not know before, and of what value is this information for understanding American political development? The Biographical Directory of the American Congress which most closely resembles the Guide in its content also identifies the terms that Morris served in Congress and notes too that he began his career as a Whig. Moreover, it reports that he lost re-election in 1844, a fact which escapes the Guide; and that he resigned from the House in 1861 to become Minister Resident to Turkey, The Directory fails, however, to provide the election results or record that Morris changed his party affiliation twice. On the whole our knowledge about Morris' career is substantially improved with the Guide's information, and when used in conjunction with the Directory, we obtain a thorough sketch of this nineteenth century politician.

With such information available for a large proportion of congressmen, a number of interesting issues about America's political development that have heretofore eluded systematic scrutiny can now be studied: (1) With more complete information about party changes we can investigate whether the leaders of new political parties came from existing parties or constituted a new set of elites who emerged from the "grass roots." In the case of Morris, the former path of leadership recruitment was followed; (2) What were the effects of partiesmore precisely, party labels-on election outcomes? The level and intensity of partisanship during the nineteenth century has in recent years attracted much attention and disagreement. Perusing these returns one finds that for

much of the century political parties were highly fluid and occasionally, as with Morris, less stable elements of the political environment than the politicians whom they endorsed; (3) Growing congressional careerism in the late nineteenth century has been credited with working important qualitative changes in Congress as an institution. With the names of losing candidates available here for the first time, we can begin to sort out the effects of competition and ambition on congressional turnover.

In addition to the 700 pages of election returns, the Guide presents useful historical articles on nominating conventions and congressional redistricting, and provides capsule descriptions of the prominent political parties. There are also numerous supplementary tables some of which, however, are little more than filler. Unfortunately, figures available in the Consortium's files on the partisan division of state legislatures have been omitted. Serving as a ready reference, as a browsing book, and as a data bank, C.Q.'s Guide to U.S. Elections is destined to become one of the most worn volumes on the reference shelf. Even at \$50, it is a bargain.

SAMUEL KERNELL

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The President Is Calling. By Milton S. Eisenhower. (Garden City, N.Y.: Doubleday, 1974. Pp. xxiii + 598. \$12.50.)

Milton Eisenhower is a rare and gifted public servant, to whom every president for over a half century, from Hoover to Nixon, entrusted major responsibilities. After beginning as a civil servant in the Department of Agriculture in the Coolidge era, Eisenhower was chosen by Franklin Roosevelt to design the Office of War Information in World War II and to establish the War Relocation Authority to effect the removal of thousands of Japanese-Americans from the West Coast following the attack on Pearl Harbor. His subsequent posts included membership on Truman's Commission on Higher Education and the chairmanship of the Presidential (Johnson) Commission on the Causes and Prevention of Violence. Simultaneously Dr. Eisenhower pursued a distinguished career in educational administration as president of Kansas State, Pennsylvania State, and Johns Hopkins Universities.

This is a rich memoir, abounding with the perceptive insights of an astute administrator, wise in its diagnosis of the ills of the modern presidency, and bold in its prescriptions. Dr.

Eisenhower provides candid assessments of the operating methods of each president with whom he served. Thus Kennedy is admired for his style, but less commendable was his failure to keep a promise to inform the public that he had asked Eisenhower and other distinguished citizens to serve on a committee to secure the release of prisoners captured by Cuba in the abortive Bay of Pigs invasion. After bitter abuse from leaders of the president's own party while Kennedy remained resolutely silent, Dr. Eisenhower dispatched "a brutally frank letter to the President, charging bad faith on his part." The letter was referred to the State Department; henceforth Dr. Eisenhower was persona non grata and "the White House was in no better standing with me" (pp. 371-72).

Eisenhower affirms the image of Lyndon Johnson as practitioner of the "two-shift" working day, volcanic in temper, and cruel to his associates. But Johnson was also one who, over three decades of association, never forgot that Eisenhower provided him his first exposure to a national audience, when, in 1942, as associate director of the Office of War Information, he invited Johnson to participate in a national radio program.

Dr. Eisenhower never concurred in Dwight Eisenhower's approving estimate of Richard Nixon's suitability for the presidency. Milton Eisenhower was always struck by the contrast between the private Nixon-"gracious, warmhearted, friendly, humorous, and devoid of pretense"-and the public figure-"austere, remote, often devoid of personality, given to self-praise, ... 'Tricky Dick'... (p. 335). Nixon's disastrous presidency is explicable to Eisenhower largely in terms of George Reedy's classic analysis, The Twilight of the Presidency. Nixon readily indulged in his preferences for isolation, for avoiding Congressional leaders and distinguished citizens, his abhorrence of the press, and sequestration in his Florida and California hideouts.

Especially rewarding is Milton Eisenhower's account of his relationship with his brother, President Dwight Eisenhower. Their close and affectionate ties were nurtured by the warm family life of their childhood. Milton and Dwight were also philosophically compatible. Dwight found it helpful to explore his innermost thoughts and plans with Milton who eschewed subservience, who perceived himself as representing no special interest and having no selfish purpose, and who sought to raise questions and provide information that might be helpful to the president. Milton was careful not to bruise the sensitivities of department heads and White House staff, as he once had watched,

with distaste, Harry Hopkins do as confident to Franklin Roosevelt.

Dr. Eisenhower compares his relationship with President Dwight Eisenhower with the relationship of John and Robert Kennedy. Unlike the Kennedys, the Eisenhowers agreed that the president's brother should not take on a cabinet post since it would smack of nepotism and that Milton's public pronouncements might be discounted simply as echoing the president.

Long involved in organizational issues of the presidency, Dr. Eisenhower offers definite prescriptions for the office's improvement. He advocates a single term of six years for the president and accords great weight to the argument that this arrangement will provide better leadership response to public problems than the trimming and adjusting of a president preoccupied with his own reelection. He opposes the nation-wide primary as unduly favoring the candidate who commands substantial financial backing. Dr. Eisenhower calls for stronger reorganization legislation for the president's use and for the line-item veto which, he contends, would avoid much of the Nixon-Congress conflict over impoundment, a confrontation in which he argues, with much effect, Congress must be allotted a substantial measure of blame.

LOUIS W. KOENIG

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The Community and the Police—Conflict and Cooperation? By Joseph Fink and Lloyd G. Sealy. (New York: John Wiley, 1974. Pp. 216. \$9.95.)

In this book Joseph Fink and Lloyd Sealy argue that conflicts between the police and the "community" (i.e., neighborhoods in which lower class blacks live) could be reduced considerably if police departments, rather than focusing upon traditional law enforcement activities, were to devote more of their efforts to mediating conflicts within the ghetto and providing services to its residents. They recommend, for example, that the police help welfare recipients to obtain delayed checks and tenants to get heat in their apartments, that policemen iron out disputes between merchants and local people and play basketball with neighborhood children (pp. 196 f.).

Since this book is less a work of political science than "police science," it would be unfair to criticize Fink and Sealy for failing to consider questions that their argument raises for political scientists. In particular, they fail to

analyze who is likely to support and who is likely to oppose police reforms such as they recommend. But its failure to consider such questions does not mean that this book is of no interest to students of urban politics. Fink and Sealy's book presents a brief for the sort of innovative programs which are advocated nowadays by police reformers and liberals (its preface was written by Senator Birch Bayh), and which are funded by the Law Enforcement Assistance Administration. As such, it is useful to consider this book as a primary source—as exemplifying a point of view that is important in American politics today—and to ask what this point of view signifies.

Although on its face a recommendation that the police shed their authoritarian demeanor and perform services for their clients appears benign, there is something disquieting-and puzzling-about it. Fink and Sealy's proposals would greatly enhance the power of the police. The ombudsmen function which they want the police to serve is performed currently-if not always adequately-by public officials, party organizations, community action agencies, and little city halls, and the policy areas into which they want the police to intervene fall currently within the jurisdiction of departments of welfare, housing, recreation, and the courts. Such a comprehensive understanding of the responsibilities of the police, of course, is not unprecedented. Etymologically, the term "police" is related to "policy," and in public law the "police powers" of the state encompass the state's general responsibilities for the health, welfare, and morals of the public. But traditionally liberals have rejected the notion that the jurisdiction of police agencies is coterminous with the police powers of the state, and they have castigated regimes which grant such comprehensive power over public policy to the police as "police states."

American practice, to be sure, has not always coincided precisely with liberal theory. James Q. Wilson, whom Fink and Sealy cite frequently, speaks of a "service" and a "watchman" style of policing-styles in which police behavior is not oriented primarily toward law enforcement. But Wilson notes that the service style is characteristic of upper income suburbs-communities which are quite orderly and where the services the police perform are of marginal importance in the lives of the community's residents. For these reasons the police acquire little power over their clients by involving themselves in matters other than law enforcement in such a setting. In cities where the watchman style characteristically prevails, expectations concerning the appropriate level of

public order are not especially high and informal social institutions-family, neighborhood, church, political machine-play a significant role in the maintenance of order. Thus, the police may be arbitrary in such cities but their power is not extensive. But in ghetto neighborhoods, the services which Fink and Sealy want the police to perform would be of great salience to local residents, and in these neighborhoods, informal institutions of social control are not able to establish a level of public order satisfactory to a large segment of the local and extralocal community. Fink and Sealy's proposals thus would give the police the resources to exercise considerable power over the poor in an environment where they would have an incentive to make use of that power. It must be recognized, then, that there is a mailed fist within the velvet glove of proposals for police reform such as Fink and Sealy advocate.

Why are liberals, who traditionally have been hostile to the police and sympathetic to the poor, prepared nowadays to support programs that will increase the power of the police over the poor? Clues to answering this question can be found in Fink and Sealy's book. First, many of the programs which Fink and Sealy advocate were, as they note, developed and funded by universities, foundations, and federal agencies. Second, as Fink and Sealy observe, the reforms which they wish to see adopted presuppose a new type of policeman: "the same basic manpower pool from which personnel are drawn for various social service agencies could just as easily provide recruits for police agencies" (p. 190). These two facts suggest that liberals might be prepared to ignore the implications which the reforms they support have for the power of the police because these reforms will enable the institutions which they control, and the classes for which they speak, to gain influence over police departments. And this explains why such reforms are resisted by old-line police administrators, by rank-and-file policemen and their unions, and by elected officials who represent working and lower-middle class constituencies. These groups recognize that such reforms threaten the control they currently exercise over the police and the monopoly they traditionally have enjoyed over jobs within police departments.

MARTIN SHEFTER

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Political Issues in U.S. Population Policy. Edited by Virginia Gray and Elihu Bergman. (Lexington, Mass.: D.C. Heath, 1974. Pp. 212. \$14.00.)

This reviewer has always had mixed feelings about edited compilations, particularly those derived from conferences, workshops, and the like. While, on the one hand, he has contributed chapters to several in the spirit (delusion?) that they represent productions of authentic scholarly importance, on the other he has found them to be of little value in his own research and teaching. Obviously not all edited works are the same, but most tend to present rather unexciting fare. Instead of pulling together carefully selected and edited pieces of original research or fresh new ideas unavailable elsewhere, they tend to contain either a disparate collection of contributions rehashing work already published or very preliminary notions in need of further thought. The crucial variable separating the former from the latter types would appear to be the editor. Unfortunately, the editors of *Political Issues in U.S.* Population Policy have not exercised their functions to the extent necessary to produce an effective, coherent volume. Growing out of a postdoctoral institute on population for political scientists sponsored in 1973 by the Carolina Population Center, the book is lacking both as an introduction to the politics of population in the U.S. for the uninitiated and as a significant contribution to the small but expanding body of literature in political demography.

Blending individually authored papers into a coherent, readable collection is, to be sure, a difficult task. It is virtually impossible without an introduction to delineate the common theme, establish an organizational framework, and relate individual chapters to the overall effort. Likewise a conclusion is helpful if not essential. This volume has neither. The only cues offered to assist the reader in assimilating the 11 contributions are the title and a brief statement in the acknowledgments to the effect that each of the contributors linked "his or her research interests to a specific problem in population policy" (p. xv). The reader must decide whether these are significant problems and how they relate to each other. Furthermore, the division of the book into three parts ("Population Policy and Political Analysis," "Empirical Studies of Population Impact on Public Policies," and "Political Implications of Population Policies") occasionally seems arbitrary if not downright confusing. Because of these and other editorial omissions, Political Issues in U.S. Population Policy as a whole suffers. There seem to be certain generalizations running through it—most notably the comprehensive, indirect nature of U.S. population policy and circumspect impact of demographic conditions on policy—but these are not effectively articulated.

In evaluating the individual contributions. one is struck, as is often the case with edited compilations of original pieces, by the diversity in approach and quality. This reviewer found the chapters in Part I especially confusing, both individually and collectively. In Chapter 1, Elihu Bergman begins by stressing the broad range of government activities affecting population but then narrows his focus considerably to the politics of federal family planning programs and abortion. Thomas Blau follows with an interesting, albeit somewhat disjointed, critique of Bachrach and Bergman's thesis on population policy making presented in their book Power and Choice. In the last chapter of Part I, Vijai Singh, a sociologist, argues for the value of cohort analysis in population policy research.

The promise of Part II is to shed light on the effects of demographic change-most importantly but not exclusively population growthon public policies. The five chapters do indeed provide us with at least preliminary evidence. but they also demonstrate some of the pitfalls of the cross-sectional ecological analysis characteristic of the American state policy literature. John Wanat and Virginia Gray, Richard Feld, Clifford Kaufman and Bryan D. Jones, and David Caputo sift through a variety of data to find apparent relationships between demographic characteristics and such things as state government efficiency, the spending patterns of local governments, and differences in the utilization of revenue sharing. In contrast to these somewhat intuitive, data-oriented contributions, Gregory Rathjen's chapter constitutes an exemplary piece of research on the impact of population size and growth on the federal judicial system. In it the author carefully establishes the significance of his topic, lays out the relevant theory, describes the data, presents the analysis, and discusses his findings. While not the last word on the subject, Rathien's work certainly provides important new insights.

Part III begins with a thoughtful essay on black attitudes toward population control. The author, Clinton Jones, treats the black community as a political subsystem and traces its suspicion of organized population programs to the historical and institutional racism of American society. He then uses content analysis to document variations in black attitudes. Although raising some interesting points, Virginia Gray's chapter on women and population poli-

cy lacks cohesion. In the concluding contribution, Stanley Hauerwas and L. John Roos take the dubious, at least debatable, position that the ethical issues at stake in population policy making ought to be made explicit.

Political Issues in U.S. Population Policy is not without its merits, particularly the chapters by Rathjen and Jones, as well as aspects of the other contributions. Taken as a whole, however, the volume has problems. With more decisive editorial direction, many of these problems might have been avoided.

TERRY L. MCCOY

University of Florida

Police-Association and Department Leaders: The Politics of Co-Optation. By Stephen C. Halpern. (Lexington, Mass.: D.C. Heath, Lexington Books, 1974. Pp. 127. \$11.00.)

If union politics in general has been left primarily to economists and historians, the study of unions within public organizations has been, until very recently, considered the bailiwick of public administrationists and personnel scholars. However, just as the rise of militancy among craft, trade, and nonskilled laborers forced economists, historians, and some political scientists to notice the impact of such political organizing in the private sector, so the last decade, with its mushrooming spate of strikes by police, fire, sanitation, and education personnel has finally jogged the empirical interests, if not the analytical consciousness, of some analysts of the public sector. Further, the literature on police, regardless of disciplinary source, is all but bereft of anything short of citing the existence of police unions or associations. Stephen Halpern's detailed case analysis of three police associations attempts to fill the limitation in the literature of political science posed by the dearth of studies of public unions. Secondly it studies police politics from the "group perspective" rather than the present trend in the literature which usually studies the police officer as an individual.

More precisely he selects the cities of Baltimore, Buffalo, and Philadelphia and isolates the relationship between the office of chief executive of the police agency and the police employee organization as the major objective of analysis. Halpern contends that this relationship encompasses the bargaining and power variables which are important to the "goals and tactics" (p. 2) of both the police commissioner and the public employee group leadership.

From the beginning the author makes the case that the formation of police organizations was not only important in mobilizing agendas to enhance employee working conditions but, as in the case of Buffalo, also came to exert significant influence over the executive direction of the department. Of particular concern to the author are two issues: police accountability through the internal review process and civilian review boards and the attempts of police employee associations to exert control over the managerial decisions of the department and thus reverse the unilateral direction of policy determination.

In all three cities, as in many cities, the issue of civilian review was an important one. However, only in Philadelphia was the creation and function of a working civilian review board the focus of debate. From his analysis of the issue the author draws an important point, which I believe has broad-reaching value for the study of police in any city, namely, "it is crucial to understand that the opposition of police associations to these boards reflected far more than objection to that one method of civilian participation in police affairs" (p. 87). Indeed the mere spectre of a civilian review board acts as a lightening rod for police association arguments "that police possess unique skills, training, and experience that make it impossible for civilians to evaluate knowledgeably their performance. Civilian review, they argued, denied police the professional prerogative to judge their colleagues who had been accused of misdeeds" (p. 87). Halpern supports this conclusion with evidence from police association leaders who see as one of their long-term goals heightened professionalism of police-in much the same manner that exists in the legal and medical professions. In the trenches, this means the street-level patrolmen are seeking control of their profession. They want a say in the process of reprimand and review of behavior and control over everything from the initial selection of candidates to the training, promotion, and day-to-day procedures of the police officers. The commissioner and the police officers are united over the preservation of departmental autonomy from outside review; but they are certainly in conflict over the professional demands of police organizations which culminate in a struggle for managerial control.

The second major issue in Halpern's study is that of internal review. He finds the efforts to gain increasing departmental influence and control on the part of the police organizations, their call for heightened professionalism aside, lead to threats to truly objective review by an internal inspection office. Here Halpern puts some empirically interesting analysis behind the arguments of departmental whitewash.

The third area of interest is that of "managerial incursion" by the leadership of police associations into areas which, even under the host of superior-subordinate conditions. had been composed of a paternalistic, rigidly paramilitary and unilateral relationship between the executive officers of the department and the line officers. This section represents Halpern's most substantial contribution. As he indicates earlier, his approach to police is contrary to most of the literature in as much as he concentrates on the police association-or the line of officers acting in consort rather than as individuals. Just as importantly, where much of the literature of policy deals with conflict between the police and external groups of citizens, this section of Halpern's study concentrates on the growing internal conflict within police departments as the politics of police associations makes an impact. The discussion of both militant and conventional bargaining tactics uses cases where the police associations are strong (Buffalo and Philadelphia) and relatively weak (Baltimore).

In summary, the book provides some good empirical information on a significant new political pattern in urban areas. However, while it is strong on empirical insight and provides some new areas of interest for scholars of urban bureaucracy and politics, it goes no further. Like most of the literature in this area (case study or otherwise) it does not spend enough time establishing some larger theoretical or conceptual and historical standards of explanation for these patterns in the urban condition. The Halpern study is an intelligent, yet theoretically weak, addition to our primarily empirically bound literature.

DAVID PERRY

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Women in Politics. Edited by Jane S. Jaquette. (New York: John Wiley, 1974. Pp. 367. \$14.95.)

Starting with the observation that it is the social science discipline which has responded with the least enthusiasm to the impact of the modern feminist movement, Jane Jaquette sets out to fill the apparent gap by presenting under one cover a series of recent papers and articles on women and politics. The result is a very valuable addition to the small but growing body of literature with a focus on the very challenging problem area of female political participa-

tion and consequent rewards. For those teaching courses on women in contemporary political life; this anthology will be an essential source book for years to come.

The parameters of the book are set wide indeed. Divided in two parts and four different sections, the first contributions examine the individual American woman as a voter and activist. Jane Jaquette's introductory essay offers a perceptive and sometimes bemused review of the earlier literature in the field. In the comment of the author: "The pattern of female participation in American politics seems to provide the occasion for a mild tut-tut or a studied ho-hum..." (p. xiv). To the degree that women do not take on male political characteristics, they are seen as weak, unable to overcome outworn stereotypes, and lacking participatory elan. By contrast, Jaquette suggests, to raise the question of female stake in politics will bring different issues to the fore. Quite simply, the kinds of reasons brought forth to explain lower levels of male political participation should be applied to women as well. Are women more alienated than men from the political system? Has pluralism removed from the political arena issues of first importance to women? Her concluding comments on the male bias in political science are pointedly ironic: "What starts out as an objective study of male-female differences all too often ends up as a diatribe against female weaknesses and a paean to male virtues" (p. xxxi).

The changing patterns of voting and political participation among American women come up for a thorough examination by Marjorie Lansing. Using survey data from the University of Michigan, she finds support for the hypothesis that changes in the structure of society have led to a redefinition of political roles for American women. The major shift in the political behavior of women has resulted from the general acceptance of the legitimacy of the female vote or, as she defines it, of a participant role as appropriate for women. The second part of her paper explores various dimensions of female activism and finds increased participation of women to correlate with college experience, youth, and labor force activity.

The effects of socialization and the conflict between motherhood and participation are the topics of papers by Lynn B. Iglitzen and by Cornelia B. Flora with Naomi B. Lynn. The conclusions of their separate studies—both based on interviews with a sample of respondents from their own communities—amply confirm the key role of sexist ideology suggested first in my own *The Silenced Majority*. Audrey Siess Wells and Eleanor Cutri Smeal proceed to

take up the question of women's attitudes toward women in politics, starting with a provocative analysis of bias and shallowness in earlier surveys. Their own research data point to three meaningful and distinct categories of potential supporters for women politicians.

In the section titled "Women as Political Elites," Emmy E. Werner and Louise M. Bachtold examine the personality characteristics of female state legislators and find these-not surprisingly-to be more intelligent, more assertive, more venturesome, more imaginative, and more liberal in their attitudes than women in general. More importantly, no evidence was unearthed of ego defensiveness or anxiety produced by role stress among the women politicians surveyed. Mary Cornelia Porter and Ann B. Matasar next present a review and analysis of the role and status of women in the Daley organization. Though the research design is questionable, the dismal picture painted of stereotyping and bland dismissal of women by this powerful political machine is convincing enough. Mary M. Lepper's study of women in the federal career structure finds, on the other hand, some movement of women into higher echelons. There is, she suggests, sufficient and clear administrative machinery to cope with sex discrimination in government. Yet stereotyped notions holding women unsuitable for the top positions get in the way; her factor analysis of the career structure indicates that the hiring and promotion system for women is frequently based on technical competency, while for a man it is based on peer relations and anticipated long-range development. Sexist stereotyping is also and consistently found in Supreme Court decisions in Susan Kaufman Purcell's incisive analysis, titled "Ideology and the Law." The past cavalier treatment of sex discrimination cases now appears at an end, yet she points out that it is still not clear whether a majority of the Supreme Court will declare sex a suspect classification in the future. *

Three contributions are included in Section 3, "The Movement: New Alternatives?" Both Nancy McWilliams and Warren Farrell focus on consciousness raising groups as distinctive of the new feminine style in politics and view these as a positive and promising political innovation. The attempts to avoid hierarchy in feminist undertakings are noted with but a few suggestions as to its attendant problems. Ironically, in Jo Freeman's provocative critique, "The Tyranny of Structurelessness," it is just this favored mode of operation within the new feminist movement that invites a new tyranny. She finds it necessary to restate the obvious: Power is not abolished by deliberately avoiding

selection of who shall exercise it; all this does is abdicate the right to demand that those who exercise power and influence be responsible for it. The diffuse power within the feminist movement insures only that the movement becomes totally ineffective in the political realm, not that any group or person is prevented from dominating it.

Part II, Comparative Perspectives, starts out with two highly interesting explorations of women's changing status and roles under communism, Barbara W. Jancar demonstrates that communist ruling groups had to break up the traditional family structures and mobilize women into the work world in order to achieve the objectives of economic development. Yet little attention was given to revolutionizing the female status; the Chinese Communist léaders appear to be the only ones who believe that a radical approach to the question is essential to the modernizing process. Women under Communism, as a consequence, have been unable to exercise any real power and are still being conditioned to identify themselves primarily in the traditional subordinate roles. Gail W. Lapidus reaches similar conclusions in her carefully designed study of the Soviet pattern of modernization and its impact on women. The Soviet experience, she suggests, calls into question the Marxist model of social change; it is the character of the development process rather than development per se that is decisive in shaping women's roles.

Among the remaining contributions we find a succinct summary of the Marxist analysis of women and capitalism, authored by Temma Kaplan, and a somewhat inconclusive case study of mobilization of women in Allende's Chile, authored by Elsa M. Chaney, suggesting the failure of the radical government in this area. Another case study from Costa Rica by Jo Ann Aviel demonstrates the important role that can be played by the school in the political socialization of women. "Memsahib, Militante, Femme Libre" is the intriguing title of an essay by Judith Van Allen which briefly describes the impact of colonialism and modernization on African women and analyzes three types of responses women are making to the dislocation produced by that impact.

Kay Boals' article, "The Politics of Cultural Liberation," in turn touches on the important and complex question of the liberation of the female self within the context of struggle against cultural domination by an outside group. The revolutionary's dilemma—to be pulled in two opposed directions toward reassertion of traditions on the one hand and toward the creation of a modern developed

society, on the other—proved to be particularly painful in the realm of male-female relations in Algeria. Boals finds that a growing number of Algerian women are eager to change the traditional patterns of female seclusion and male dominance, yet are inhibited from doing so by the internal psychic ambivalence created by the desire to affirm the Algerian heritage and culture.

Mary Cornelia Porter's bibliography is the final and valuable addition to this timely anthology. Though the contributions are not even in quality and a more rigorous editing job could be wished for, there is no doubt that Women in Politics contains sufficient rich and provocative material to recommend it to anyone who does not insist on remaining blind to the professional and political challenge suggested by the title.

KIRSTEN AMUNDSEN

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Mayors in Action: Five Approaches to Urban Governance. By John P. Kotter and Paul R. Lawrence. (New York: John Wiley, 1974. Pp. 287. \$14.50.)

As befits two Harvard professors of organizational behavior, the authors' primary focus is behavior. They want it understood that this work is interdisciplinary, "not a political science essay." Their report is the result of a two-year exploratory, interdisciplinary, comparative examination of the behavior of 20 mayors no longer in office and of their administrations during the 1960s. What do mayors do? Why do they do that? What impact does such behavior have on their cities?

Can a general purpose "model" of mayoral behavior be constructed that can describe the different ways in which mayors behave, explain why a particular behavioral pattern emerges in a specific situation, and predict the impact of a mayor's behavior on his city? Pointing out that such a general purpose model does not currently exist, the authors proceed to develop one. But to the uninitiated, the process is tortucus, and the "average" mayor (my model, not theirs) would find the reading difficult.

Social scientists, however, will discover this to be a solid work and should be particularly interested in the research methodology and design. It includes an impressive amount of field work.

After a complete examination of all literature on mayors, the authors re-examine the more serious writings to identify what are at least implicit as 10 different models of mayoral

behavior, although most are not stated or written as a model of mayors' behavior as such. Examples are the Power Broker model, derived from Edward C. Banfield's Political Influence (New York: Free Press, 1961), the "Muddling Through" model, from David Braybrooke and Charles E. Lindblom's A Strategy of Decision (New York: Free Press, 1963), the Multihat Role, from Leonard I. Ruchelman, ed., Big City Mayors (Bloomington: Indiana University Press, 1969), etc.

A personal judgment evaluation of each of the 10 models in each of the 20 mayoral situations studied in the inquiry led the authors to conclude that while each mode has a certain limited usefulness, often complementing and supplementing one another, none is entirely satisfactory. By defining six of the models as focusing on the behavior of the mayor, the other four as treating the context in which the behavior occurs (contextual variables), a new framework was developed which, in a sense, shows how the ten models "fit together." In this framework a mayor's behavior can be seen as occurring in three different but somehow interrelated processes: in one the mayor decides what to do-sets the agenda; in another he builds and maintains a network of relationships needed to pursue the agenda; in a third he accomplishes tasks that are on his agenda.

These three processes of the mayor's behavior—agenda setting, network building, and task accomplishment—occur in a context of at least four variables: the mayor's own personality, the formal structure of the city government and the mayor's role in it, the distribution of power in the community, and the city environment itself. This context affects the behavior and in turn is affected by it.

The first three chapters of the book are devoted to the authors' rationale for this new framework. The next three describe a variety of actions they found the mayors to be using for the three key processes of agenda setting, network building, and task accomplishment. A chapter then identifies five mayoral types, each with a different behavioral pattern as defined by those three key processes. The five types of mayor are called: Ceremonial, Caretaker, Personality/Individualist, Executive, and Program Entrepreneur. Of the 20 mayors studied, "approximately" four representatives were found of each type.

The model that has evolved up to this point describes how mayors behave and how short-run dynamics and constraints both affect and are affected by such behavior, concentrating on relationships between the processes and the contextual variables. It is completed with a

chapter on "Pattern Dynamics," with emphasis on system dynamics in the long-run. Relationships among the four contextual variables themselves are examined and found to be such that if any two are not "aligned," the consequences eventually create problems for the mayor. If he cannot somehow eliminate the nonalignment, people generally look to the electoral process to correct the situation by replacing the mayor.

If all four contextual variables of a mayor's system are aligned simultaneously, we have a state of "coalignment," a condition which none of the 20 mayors in this study attained, although some came close. Relationship between election results and coalignment results, as depicted in an interesting chart (p. 140), appears to be fairly clear. This issue of coalignment is important and is explored further in some of the remaining chapters, using mayoral behavior is complete. In a later chapter on "Implications for Social Scientists" the authors suggest some implications of their coalignment model for further research on administration.

If mayors would take the time and effort to assimilate this book, they would discover a number of findings from the research that have practical relevance to their efforts to be effective. In a chapter, "Implications for Mayors," the authors describe seven that seem to be particularly important, although they admit that none is currently an accepted part of "conventional wisdom." This leads to the final chapter, "The Future: Some Disturbing Trends," which indicate a bleak prospect in view of current trends affecting cities and their populations. The conclusion of Kotter and Lawrence: "Unless subsequent events change or alter these trends, our model predicts an urban crisis of unparalleled size and scope."

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Transformations of the American Party System: Political Coalitions from the New Deal to the 1970s. By Everett Carll Ladd, Jr., with Charles D. Hadley. (New York: W.W. Norton, 1975. Pp. xxv + 347. \$12.50, cloth; \$4.95, paper.)

Ladd and Hadley begin by reminding the reader that "when the priestess of Apollo at Delphi made ready to deliver an oracle, she positioned herself upon a special seat supported by three legs, the tripod." This book reports their panoramic "view from the tripod."

The first half of the book uses the concept of the "agenda" (which Ladd developed in his American Political Parties) to explain the soci-

ety's shift during the 1930s away from a business-directed society and economy (business nationalism) to an emphasis on the government's responsibilities for the direction of economic life and individual welfare (governmental nationalism). The agenda of government nationalism is used to explain the movement of blacks and Jews into the Democratic party; it is used to explain the intensification during the 1930s of a nascent class cleavage in partisanship; and it is used to explain the apparent continuity of both the sectional and religious dimensions of party support.

The social group character of the party coalitions and the agenda of governmental nationalism also explain why the expansion of the middle class and the growth of suburbia did not presage a decline in the Democratic party. The expected Republican majority did not emerge, according to Ladd and Hadley, because the partisanship of the social groups (religious, racial, sectional) was only weakly predicated upon social status. Then, too, the postwar middle class was not the entrepreneurial middle class which had supported the Republicans since the Civil War, but an increasingly professional middle class with a symbiotic relationship to the liberal state and government nationalism.

Their analysis of the Southern Democracy is one of the most interesting to be published in recent years. Using older Gallup data, Ladd and Hadley document substantial southern support for the welfare programs of the New Deal during the 1930s and 1940s. The data provide a benchmark against which they chart the political movement of the South, as the Democratic party extended the application of governmental nationalism to include race relations and as the South experienced economic changes which undermined its sympathy for the policies of the national Democratic party.

The second half of Transformations of the American Party System uses the agenda concept to explain the withering of the New Deal system. As Ladd and Hadley describe it, the current Democratic-Republican division is the party system of an industrial society in which the major questions revolve around the distribution of material benefits. They believe that the dealignment/realignment of the parties is a result of the fact that America, sometime over the past two decades, began to evolve into a postindustrial society. The agenda of this postindustrial society, which has substantially resolved economic needs for all but a fraction of the population, is social and cultural questions; and the "leading class" which defines the agenda is the new, enlarged middle class. The principal importance of the enlarged middle class is that it has magnified the power of the intellectual stratum and made it possible for them to wield greater influence over the content of political debate than they have ever previously enjoyed.

Ladd and Hadley conclude that the future of the party system is being molded by a declining concern with industrial society welfare issues and an expansion of a self-conscious intellectual stratum which uses the communications system to perform many of the educative and political tasks previously reserved for political parties. The symbolic politics of 1972 are, in their view, a harbinger of a future in which organized activists will frequently capture presidential campaigns in order to proselytize their vision.

On the whole the first part of the book is one of the best descriptions in print of the social group basis of the New Deal and the issue agenda that provided its raison d'etre. It has only two problems. The abandonment of the Republican party by the upper status segment of the population seems much more marked in their Gallup data than in the SRC data for the 1952 to 1960 period. Whether the data are really in conflict or only reflect different definitions of what constitutes the upper class (Ladd and Hadley are not very clear about such things) is impossible to determine. Whatever the explanation for the difference, it seems certain that the declining Republicanism of the middle and upper class is not as large as Ladd and Hadley indicate in their analysis. A second problem is their attempt to show that the emergence of the Democratic majority did not depend upon the mobilization of a new group of voters in the 1920s and 1930s. The problem with the analysis is that it begins in 1940, although the process of mobilization of new Democratic voters began a couple of decades before 1940 and largely ended by that year. Also, the mobilization which created the New Deal is based on a complex interaction of party identification and turnout rates, and simply examining party identification and voting choices after 1940 will not pick up the effects. To sustain their thesis, they would have to refute the data in The American Voter (pp. 153-56) and The Changing American Voter (Chapter 5) which show that Democratic ascendence in the 1930s depended upon large numbers of latent Democrats, both young and old. voting for the first time.

Finally, the application of the postindustrial society model to explain changes in the New Deal party system seems to be a procrustean exercise. It is beyond question that there are a new set of social issues that are frequently

orthogonal to the prevailing party cleavage, and that they are playing a significant role in the ongoing realignment of the parties. Unfortunately, one of the least postindustrial issues, race, is principally responsible for the altered partisanship of the South, the region which contributed about 50 percent of all of the observable change in the social basis of the parties since 1960. Also, there is the fact that at this writing (October 1976) New-Deal-style welfare issues are at the core of the current presidential election. Which is a model for the future, 1972 or 1976?

However, the value of the book does not depend upon the accuracy of the authors' predictions about the future of the party system or about the importance of post-industrial politics to the declining hold of the parties on the electorate. The book should be read because its interpretation of the social and economic infrastructure of the New Deal system is insightful, and because it provides a suggestive explanation for the social changes that are bringing that system down.

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Urban Outcomes: Schools, Streets, and Libraries. By Frank S. Levy, Arnold Meltsner, and Aaron Wildavsky. (Berkeley: University of California Press, 1974. Pp. x + 278. \$12.50.)

This is the third book from the Oakland Project, following Arnold Meltzner's Politics of City Revenue and Jeffrey Pressman and Aaron Wildavsky's Implementation. Many more are on the way. Urban Outcomes adds to our knowledge of Oakland using a reasonably traditional case study approach, here baptized "close causation" (p. 17). It is a useful if limited study. Its focus is "urban outcomes." In the three service areas studied, these outcomes include (1) for schools: teachers' salaries, student/teacher ratios, and costs per student; (2) for streets: resurfacing rates and street construction projects; and (3) for libraries: book circulation rates. The outcomes are all analyzed by neighborhood area within Oakland.

The authors essentially present a "social audit" of the sort James Coleman has called for: tracing budgetary commitments through the service production process to the provision of teachers, streets, and books. Given the sparse literature on these processes, the book justifiably includes substantial descriptive detail for each service area. But the empirical work stops short of considering actual impacts on citizens.

The book deserves careful reading for such matters as capital expenditure decisions by the street engineers, the emphasis of the librarians on hiring highly qualified librarians (in contrast to more books), and the way that extra-local funding helped move school resources more toward the poor. The reportage is excellent. The writing is sharp and clear. We have come to expect no less from Wildavsky and company.

Mixed with the empirical work is a strong normative brew. The authors, reflecting the concern of the late 1960s with equality in service delivery, are intent on documenting distributional differences according to income and (occasionally) race. They find some, but remarkably few. Coming from Berkeley, the authors are ultra sensitive to the political (primarily distributional) aspects of each decision. The Oakland people are not. The school administrators, and especially the street engineers and librarians, generally adhere to their professional norms in resource-allocation decisions. Operationally, this means (re)building streets where traffic flows are greatest, and providing books to book borrowers in general response to their requests. School resources are allocated more directly toward equalizing resources per student. The strongest sections of the book are those where the decision rules leading to these patterns are made explicit. The professional bureaucrats are presented, reasonably convincingly, as the key decision makers. This stable picture of repetitive decision making is, not surprisingly, fully incrementalist. One imagines that, if the authors had instead selected certain more controversial decisions, more input from nonbureaucratic actors would have been apparent. But what we are shown is surely useful.

The authors are also concerned with answering "who benefits?" Two general types of criteria have been used in the literature: those originating with researchers and those originating with the persons they study. The authors are sensitive to abandoning criteria which might derive from the citizens of Oakland. They try, a little. Meltsner's earlier book included an outstanding chapter on the remarkable absence of opinions on tax policy even by politically active Oakland citizens. Here too some fragmentary material suggests that general citizens have very few clear ideas concerning specific policies in these three service areas. Current advocates of "policy maximization," "issue politics," and the like should find these sections of the Oakland reports chastening.

A second plausible answer to who benefits might come from analyzing interest groups and their inputs to the political process. A widespread view is that more affluent citizens participate more in the political process and correspondingly receive more policy outputs than the poor. This book does not provide much direct detail (although other Oakland Project reports do) to show that this is not the case in Oakland. The reader may first react with doubt, but gradually accept the thorough-going "professional" character of Oakland politics. With perhaps Chicago or New Haven in mind, the authors write, "public works elsewhere may be a pork barrel, but in honest Oakland street decisions are straight" (p. 163).

At this point, other researchers might have sought to explain the distinctive political culture of Oakland leaders and citizens which maintain the legitimate this style of decision making. But "reform government," "public regardingness," the "Irish ethic," or "non-ideological particularism" are not used here. They might have entered if the authors had sought to place Oakland on the national urban map. A systematic comparative study is unnecessary for this; comparisons with other case studies and comparative work could be used, particularly that of Robert Lineberry, Elinor Ostrom, and Harry Hatry.

Instead the authors leap to their own normative typology. It then is used to interpret their findings and suggest policy implications. Their typology is a threefold classification of policy outcomes. First is what they call simply "the more, the more"; their two defining examples indicate its vagueness: "Roads go only where the cars already are. Experienced teachers transfer to the well-to-do schools" (p. 224). Thus lumped together are policies which positively favor the more affluent as well as policies where a user charges principle is applied. This vagueness is not accidental; it derives from ignoring the inputs (here primarily fiscal) to Oakland's government from the neighborhoods whose outcomes receive such detailed attention. The authors are appropriately apologetic on this point (p. 21); Henry Aaron, John Weicher, and others have developed straightforward procedures for estimating intraurban fiscal burdens which could readily have been applied in Oakland. Perhaps they will be in the future.

Their second policy type is "compensation" whereby poor neighborhoods receive larger resource allocations per user (e.g., extra teachers) than more affluent neighborhoods.

The third type is "resultants." Here the policy is allocated with sufficient compensation to make the impacts identical across neighborhoods, e.g., identical test scores by children in all neighborhoods. The authors affirm in a

tortuous concluding chapter that they themselves prefer more redistributive policies in Oakland (less No. 1 and more No. 3). But they then go on to point out how probably such policies would fail for many reasons, the most immediate being that the affluent could simply move out of Oakland.

A logical weakness of the whole argument, however, becomes more apparent at this point. Why impose these three ideal types on Oakland and then take a strong personal stand? Why not attempt to see how Oakland citizens (and leaders) view these issues? The argument (in Chapter 1) was that citizen preferences could be ignored because citizens had few opinions on the specific service issues studied. This may be true. But it does not follow that the same applies to views on these three very broad types of policies.

The authors seem inordinately critical of other research approaches, such as comparative analyses of cities. These, they suggest, "do not identify particular levers which might be used to alter policy outcomes." For example, after one finds that cities with numerous Irish Catholic citizens have large city government budgets, they ask, "if a community wants to increase its budget, should one suggest that it import Catholics from Ireland?" (p. 11)

By contrast, these authors are sufficiently convinced of their own ethical views to essentially ask citizens of Oakland to adopt their general principle of dramatically increasing redistribution, and accordingly to reorganize the entire city government. Is this policy analysis or political philosophy? Surely one alternative would be to document the ethical views of different ethnic and income groups within Oakland (blacks if not Irish) and then to point out how the city government might be reorganized to become more responsive to these different groups within Oakland. They might thus suggest better means to an accepted end; instead they suggest new ends.

Still, it is likely that *Urban Outcomes* will be read more outside than inside Oakland; political scientists with an interest in service delivery cannot afford to ignore it.

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Against the Law: The Nixon Court and Criminal Justice. By Leonard W. Levy. (New

York: Harper & Row, 1974. Pp. xvi + 506. \$12.95.)

If anyone reading this knows someone who religiously clings to the belief in the immaculate conception of constitutional policy making, steer him/her to Leonard Levy's book on the Nixon Court and criminal justice. For what Levy does in his meticulously wrought analysis of the 1969 through 1972 Terms of the Supreme Court is to demonstrate beyond a reasonable doubt that profound changes in criminal law have occurred since the end of the Warren Court and the advent of the Nixon Court. Levy provides abundant and persuasive qualitative evidence that the Nixon Four plus White (and sometimes Stewart) are pro-prosecution conservative activists. He shows that true to the activist tradition they have brought about a constitutional revolution to further their policy preferences.

Levy's book is aimed at a general audience and in the first chapter he goes over ground familiar to specialists in the field, e.g., a summary of the accomplishments of the Warren Court, selection politics, and a sketch of the justices with particular attention to the Nixon appointees, and a brief overview contained within a discussion of "strict construction" of the wide latitude judges have in fashioning constitutional policies. Once over the preliminaries, specialists will have a field day with the subsequent six chapters as an uninhibited Levy lets few sleeping dogmas lie (a Levyathan pun as is the title of his book) and subjects to most searching scrutiny the decisions of the Nixon Court concerning the criminal justice guarantees of the Bill of Rights. Each opinion of the Court along with concurrences and dissents is dissected in terms of its reasoning, handling of precedents and history, the persuasiveness of the arguments, and the way opposing viewpoints of colleagues are handled. In the concluding chapter Levy even brings in a primitive form of bloc analysis to provide a quantitative complement to the qualitative analysis.

No justice is spared from Levy's critical eye although the Nixon Four plus White do (deservedly) win more poor marks than the somewhat more moderate (in terms of opinion behavior) Stewart and liberal activists Douglas, Brennan, and Marshall. For example, Burger is char-broiled for his Harris vs. New York opinion as "surely one of the most scandalous, extraordinary, and inexplicable in the history of the Court" (p. 149). Burger, among his other sins in Harris, misrepresented a vital fact in the case and no one, not even the outraged dissenters, caught it. Powell gets a professorial

spanking for his Kastigar v. U.S. opinion for "having contradicted history and garbled the precedents" (p. 183). White doesn't make right in the case which legitimized six-person juries, Williams vs. Florida, and Levy's indictment charges White with a "hodgepodge of bad reasoning and guesswork" (p. 267). And so on.

But Levy is not always anti-prosecution. For example, with the 1972 death penalty decisions Levy finds after a lengthy and detailed examination that the decisions are "wrong as a matter of law and history" and "wrong in reversing irrevocably the sentence of death for many who were justly convicted without caprice or unfairness" (p. 418). (But Levy fails to reconcile this latter point with his more realistic earlier evaluation of the inherent institutionalized unfairness of the criminal justice system particularly for the poor.)

Levy's overall assessment of the Nixon Court's work in the criminal justice sphere is damning. He concludes that the Nixon Court

reinterprets precedents, distinguishes them away, blunts them, obliterates them, ignores them, and makes new law without the need of overruling or being bound by the past.... It cultivates the illusion, suitable to the image of a conservative court, of having some respect for precedents; at the same time, it narrows them until they become meaningless and moribund (p. 423).

What the justices are engaged in is "result-oriented adjudication which is a corruption of the judicial process" (p. 438) and constitutional law today is essentially the embodiment of the prosecution-oriented attitudes of the Nixon Court. This leads Levy to his ultimate denunciation: "The lawyers who today constitute the majority of the Court in most criminal-justice cases are no damn good as judges" (p. 439).

Levy's indignation, however, seems out of joint with the realistic appraisal of constitutional decision making offered in his first chapter where he recognizes that:

inevitably, then, our constitutional law is subjective in character and to a great degree result-oriented. We may not want judges who start with the answer rather than with the problem ... but as long as mere mortals sit on the Court and must construe that majestic but muddy Constitution, we will rarely get any other kind" (p. 35).

It seems then that Levy's real gripe is that the Nixon Court majority gives the Bill of Rights a less generous reading than he does. I am skeptical that Levy would accept the results of decisions he most bitterly criticizes if only they were brilliantly argued. Neither do I buy the argument that poorly crafted opinions sap

public confidence in the Court (note the increase in positive ratings in the polls during the 1970s). It would have been more consistent with much of his earlier argument had Levy in his conclusion questioned the Nixon Court's criminal justice policies qua public policy or had he stressed that he would like to see these philosopher-kings be more philosopher and less king rather than condemn result-orientation.

In brief, Levy's qualitative analysis is first rate and, by the way, supports the quantitative work that has been done on the Nixon Court. The methodology, although traditional, provides in the expert hands of this leading constitutional historian a public policy perspective that is as compelling as it is timely—as the Nixon Court revolution continues unabated.

SHELDON GOLDMAN

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Federalism and Clean Waters: The 1972 Water Pollution Control Act. By Harvey Lieber in collaboration with Bruce Rosinoff. (Lexington, Mass.: Lexington Books, 1975. Pp. xiv + 288. \$18.50.)

Without exaggeration, Professor Lieber describes the Federal Water Pollution Control Act Amendments (1972) as "the most ambitious and expensive pollution control requirements to be promulgated into law." But was the act also a massive, and unnecessary, federal displacement of state pollution control authority and, thereby, a repudiation of the Nixon New Federalism supposed to govern administration policy? In a welcome effort to evaluate environmental policy in terms of broad disciplinary concerns, the author places the law squarely in the context of American federalism and asks: (1) "Was the Act really necessarywere the states, the key implementers of water pollution controls, unable to unwilling to clean up their waters?" and (2) if federal action were necessary, "... are the provisions of the law likely to fill these needs and improve specific program inadequacies of the states?" Five states are selected for "intensive" impact study: New York, Mississippi, Texas, Washington, and Wyoming.

The author suggests that the justification for federal action is "mixed." Among the states studied, they found Wyoming was "needlessly forced to install sophisticated monitoring devices" and Texas had to "unnecessarily reshuffle" a well-developed program. About the law's general effect, the author concludes:

"... the Act will help to spur the more retarded states, have a mixed effect on those with developed programs, and have a negative effect on a few like Wyoming." The impact analysis, however, appears to be the least substantial portion of the work. The significant aspects of the work lie in a different direction. By treating the Act as "a case study in the New Federalism," the author offers a carefully detailed and insightful explanation of why the New Federalism failed to appeal to those ostensively committed to it. "Despite the rhetoric," writes the author, "the commitment by governors, Congress, and the administrative agency to a federal partnership in practice was not deep" (p. 196). The author describes how Congress, governors, the White House, and the EPA engaged in a somewhat unwitting, but highly successful, effort to frustrate the New Federalism and why the states, in particular, were persuaded to embrace an Act which largely rejects in practice the New Federalism. Another secondary, but important, theme throughout the book is how, and why, the U.S. Environmental Protection Agency came to exercise enormous autonomy in implementing the program—an excellent study in the evolution of administrative discretion. "Congress' role [in implementing the Act] seems to be reduced to that of a frustrated, sometimes angry bystander," writes Professor Lieber; the EPA, he concludes, exercises "almost total control over the [implementation] process" and enjoys the ability "to further tighten requirements over state and local governments" (pp. 123-24).

The policy impact assessments are unsatisfactory for several reasons. Although published in 1975, the empirical materials on state water quality and implementation of the Act appear to have been gathered no later than 1973-far too early for confident judgments about policy consequences. A more useful source on impact now is the multivolume Report of the National Commission on Water Quality, published in 1975. Moreover, many judgments about policy impacts contained in the book's Appendix seem purely speculative, as, for example, those relating to "1976-76 Needs" and "Effect of 1972 Amendments" in each state; evaluative criteria and data, in any case, are not readily identified. Data supporting many conclusions seem inappropriate. Thus, the author's material on the "Extent of Water Pollution Problems" in each state-a critical baseline data for assessing the "need" for federal intervention-comes from a 1960 water quality survey and takes no account of potentially important water quality alterations in the many years intervening before the Act was passed. Often, data seems irrelevant to conclusions. In a detailed study of the Texas water quality program, for instance, we are told that "the most pressing Texas water pollution problems are caused by municipalities" (p. 162); but the figure and table supporting this assertion reveal no such information. One wonders, then, upon what basis the conclusion is justified that "Texas was doing quite well without the necessity in most cases for additional federal requirements..." (p. 192). Such characteristic flaws gravely diminish the force of the author's argument that federal intervention in state pollution control programs was often unjustified.

In short, one should read this book for its informed and enlightening discussion of how the New Federalism miscarried and for its insights into the development of administrative discretion in water pollution management. Although the author has asked very significant questions about the impact of the Act upon the states, the questions still await more persuasive answers.

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Congress Against the President. Edited by Harvey C. Mansfield, Sr. (New York: Praeger, 1975. Pp. viii + 200. \$15.00, cloth; \$5.95, paper.)

Of all the manifestations of the past decade's frenetic political events, the struggles between president and Congress have been among the most intriguing and productive of profound changes. The twin lightning rods of Vietnam and Watergate have invested new meaning to Edward S. Corwin's characterization of the Constitution as "an invitation to struggle," altering (at least for the time being) the character of legislative-executive relations. Political scientists, no less than others, have had to relearn generalizations and reassess assumptions about these institutions.

Now we are busy consolidating our information and insights concerning these new developments. After all, nearly everything written before, say, 1970 about seniority, the Rules Committee, the filibuster, the budgetary process, appointments and confirmations, and the war-making and treaty powers is acutely in need of revision. Changes have been so swift that journalists rather than scholars have led in announcing them. Thus Mansfield's collection of papers is timely and useful. However, considering the APSR backlog, by the time this

review appears the collection will itself need updating.

Inevitably, the collection is something of a mixed package, but it is sufficiently rich that it can be recommended for the classroom or for general enlightenment. And, happily, the contributions are highly readable.

Picking favorites among the essays is bound to be a personal matter; in my case I tended to be drawn to pieces on subjects about which I was least familiar. One of the finest articles is Allen Schick's "The Battle of the Budget," a primer on budgetary politics, the causes of intensified budgetary conflict, and the altered rules of the budgetary game. Schick has done this sort of thing before, but his explanations are so authoritative, his observations so insightful, and his writing so lucid, that even close students will profit under his tutelage. One hopes he will soon furnish us with a book on the budgetary process.

Another intriguing essay on a complex topic, Richard Pious' "Sources of Domestic Policy Initiatives," reminds us that presidents have no monopoly over policy initiation. Indeed, although many factors propel presidents toward the Initiator-in-Chief role, there are often equally compelling reasons for avoiding such a role. Pious details the sources and channels of policy initiation in both the executive branch and Congress.

Pious draws several useful distinctions in a field where distinctions ought to be made more carefully than they have been in the past. He distinguishes between first- and second-stage innovation and appears to differentiate between innovation and initiation—although this latter is not clear. As with Schick's article, Pious' contribution seems an abbreviation of a longer argument, which I look forward to reading in detail.

Other informative chapters cover such diverse topics as senatorial confirmation of appointments (Ronald C. Moe), oversight of intelligence agencies (Harry Howe Ransom), Congress and foreign policy (Edward A. Kolodziej), and congressional staffing (Harrison B. Fox and Susan W. Hammond). Mansfield's own essay sets the tone of the volume by describing the dispersion of power within Congress—a development whose implications are addressed by some of his colleagues but not by others.

As usual in such collections, the most stimulating portions are those that invite controversy. For me these included John G. Stewart's discussion of congressional leadership and Robert G. Dixon's views about separation of powers, including executive privilege.

Stewart's informative and thoughtful essay

details the growth of central party organs in Congress from the vantage point of one who has observed them first-hand. He tends to identify the changes—especially caucus controls over committee appointments—with the notion of party government. "By 1975," he writes, "the visible trappings of a responsible congressional party system were in place" (p. 31).

Yet these party instruments, while admittedly stronger than a decade before, have yet to be used extensively for policy initiation or development. (Stewart's favorable view of the congressional Democrats' foray into integrated energy policy making, for example, will not be shared by everyone.) Moreover, as Stewart notes, the caucuses have ironically been resuscitated at the very time when parties appear to be withering at the grassroots, leaving most candidates and incumbents to fend for themselves. The parties' policy-making apparatus lies largely dormant.

In fact, caucus resurgence is probably transitory, linked less to "party responsibility" than to changing factional politics. Outnumbered in the House and Senate chambers, where conservative Democrats could align with Republicans to checkmate liberal initiatives, the Democrats' growing liberal wing turned to the caucus as the instrument to pry open the seniority system. Caucus activism could conceivably persist, but I tend to be less optimistic than Stewart.

Dixon's essay demonstrates that the executive force theory has not been demolished by the past decade's events. His discussion of congressional vetoes is informative. He properly distinguishes between "committee vetoes" of doubtful constitutionality and the congressional veto provisions of reorganization acts, the function of which is clearly legislative rather than implementation. Yet these are matters of degree; after all, Congress has been delegating matters to its committees at least since the early nineteenth century. A more telling objection to committee vetoes, in my judgment, is that they can enmesh committees and bureaucrats in paperwork, even precluding discretionary probes into issues of genuine

More questionable is Dixon's expansive reading of the so-called "separation of powers" concept, and hence his eagerness to defend executive assertions of privilege. He is wide of the mark in asserting (p. 139) that "Congress has an instinctive thrust toward control of the Executive." And I believe his view (p. 148) that "the courts, it seems, can save the nation from an imperial President" misreads Watergate's meaning. It is wise to recall Corwin's admonition that courts can only create vacuums, while

others must fill them. Otherwise, what can save us from an imperial judiciary?

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Yeas and Nays: Normal Decision-Making in the U.S. House of Representatives. By Donald R. Matthews and James A. Stimson. (New York: John Wiley, 1975. Pp. xiv + 190. \$14.95.)

Yeas and Nays is an important addition to the growing body of literature on congressional decision making. The fundamental problem addressed by Matthews and Stimson is how can congressmen vote on bills in a reasonably rational fashion given the number, scope, and complexity of the issues confronting them and the insufficient time and resources available. After reviewing and rejecting a number of models of legislative decision making including voting, the constituency, precedent, and ideology, Matthews and Stimson posit cue taking as the conscious strategy that congressmen adopt to cope with their decision-making burdens. A cue is defined as "any communication-verbal or nonverbal-intended or unintended-that is employed by the cue-taker as a prescription for his vote" (p. 51) and the essence of the cue-taking model is given in the following statement:

When a member is confronted with the necessity of casting a roll-call vote on a complex issue about which he knows very little, he searches for cues provided by trusted colleagues whobecause of their formal position in the legislature or policy specialization—have more information than he does and with whom he would probably agree if he had the time and information to make an independent decision. Cuegivers need not be individuals (p. 45).

The cue-taking model is deemed by the authors to be most relevant to the normal voting situation in Congress—that is, on low information, low salience votes. The authors talk of initial cue-givers—members with expertise on the issue in question—and intermediate cue-givers—members who have evaluated the cues of the initial sources and have themselves come to a position on the issue. The initial cue-givers tend to be on the committee where a bill originated, while the intermediate sources are more partisan in character, including party leaders, state party delegations, and sociolegislative groups such as the Democratic Stucy Group.

The authors then test the cue-taking notion with two distinct bodies of data—a set of

structured interviews conducted with a random sample of members of Congress between February and June 1969 and a roll-call based simulation for nine sessions of Congress. The interview data are used to show that members of Congress explicitly recognize the need for and use of cue-taking process. The accuracy of the simulation results gives one greater confidence that cue transmission really does occur. The fact that the results of two separate analyses on two independent data sets converge makes one extremely confident that the pervasiveness of cue taking in Congress has been convincingly demonstrated by Matthews and Stimson.

The analysis of the interview data is straightforward. There are some minor criticisms to be made of the presentation of results: some of the tables could be labeled more clearly; some information about uncodable responses might be given, especially when such responses are numerous; and some of the simpler tables might be eliminated with their information presented more economically in a sentence or two of text. There is a tendency, especially in chapter 4, to overinterpret percentage differences.

More serious objections can be raised about the operationalization of the cue-taking process via the computer simulation and the evaluation of the accuracy of the simulation, although the following criticisms do not upset the validity of Matthews and Stimson's findings. Concerned with maintaining parsimony, the authors incorporate nine cue-givers in the simulation: the president, the state party delegation, party leadership, committee chairman, ranking minority member, conservative coalition, Democratic Study Group, party majority, and House majority. The choice of these nine cue-givers reflects the extant literature on legislative decision making and the authors' own intuitions about the process. Yet other theoretically relevant cue-givers might be included. In particular, a cue-giver that combines state party delegation and committee membership might better represent the decision-making processes of members from larger delegations with members placed across a large number of committees.

The authors claim that the computer program that comprises the simulation is "itself a description of the process of congressional decision-making" (p. 114). This claim seems a bit exaggerated. To predict how a representative will vote on a particular roll call, the simulation first determines how often the member has voted with each of the nine possible cue-givers on the previous 50 roll calls taking into account the number of times the cue was available and the number of times the legislator

actually voted. A rank ordering of the importance of the cue-givers for each representative is then established with predictions about how a member will vote on a bill being generated by examining in order of importance how that member's cue-givers voted. It seems more reasonable to describe the simulation as prognostic rather than process, that is, as more of a data manipulation routine than a serious effort to model actual processes. There are some process aspects built into the simulation; the learning curve is intriguing and does allow for changes over time in cue-taking behavior.

The authors are not as sensitive as they might be to the problems involved in evaluating the predictive accuracy of the simulation. To be sure, Matthews and Stimson are keenly aware that their impressive 88 percent overall prediction rate is not to be compared to zero percent success or even to a chance model with 50 percent success. Yet the authors do not admit that there are other models to which their results might be compared and that these other models enjoy predictive success comparable to their own simulation. For example, more parsimonious party-only models can achieve predictive accuracy above 80 percent and perform even better when party is trichotomized as Southern Democrat, non-Southern Democrat, and Republican. On grounds of explanatory (as opposed to predictive) power, the Matthews and Stimson simulation is to be preferred over a party-only model. The authors recognize that predictive accuracy may be inflated by such factors as the virtual unanimity of many votes. Hence, they examine accuracy by the substance of the issue, the division of the vote, the type of vote, and the timing of the vote. They also analyze accuracy for the 100 representatives that were interviewed, thereby deftly combining the interview and roll call data.

In summary, Matthews and Stimson have documented very skillfully the importance of cue-taking in the U.S. House of Representatives. Their book is well worth reading both for its substantive findings and its intelligent use of multiple data sources. The problem now is to integrate the Matthews and Stimson work with other models of legislative decision making. The authors themselves might have done more to establish linkages between their enterprise and the work of, for example, Kingdon and Clausen. Yet this is certainly not their burden alone; an assessment of what we know about legislative voting and of how our various models mesh or conflict is now in order.

HERBERT B. ASHER

Ohio State University

Corporate Response to Urban Crisis. By Kenneth J. Neubeck. (Lexington, Mass.: Lexington Books, D.C. Heath, 1974. Pp. 166. Price not listed.)

Sociologist Neubeck explores four questions. First, why did major corporations become involved in solving the urban crisis? Second, how were these social program expenditures reconciled with the profit and growth objectives of business firms? Third, what benefits and costs are involved in this type of corporate activity? Fourth, what are the implications of this intervention for ghettoized blacks? The analysis is based on three case studies of corporations in St. Louis which Neubeck hopes are "similar to other corporate social projects undertaken elsewhere in the nation's central cities" (p. 132). The methodology is the case study approach which is based on a "flexible, exploratory research design of an inductive character" (p. 131). The author explains that the book "embraces a highly critical perspective ... because this is what all sociology should be about and is about" (p. xi).

Neubeck conducts three case studies of corporations in St. Louis which attempted to alleviate urban problems from 1967 and 1970. In the first case, Southwestern Bell Telephone Company, a wholly owned operating company of AT&T, focused on working with the St. Louis school system to train individuals for low level positions. The second case study focuses on Brown Shoe Company which joined with a grass roots community group to establish a plant in the inner city. The third case study describes the efforts of the Ralston Purina Company to allocate charitable trust funds to enable a black inner-city grass roots group to carry out its own plans for housing development.

Each of the three corporate activities is described in terms of its limitations. One of these seems to be that each of the corporations' responses to the urban crisis was one of "enlightened self-interest" (p. 110). Specificallv. Southwestern Bell served its own needs by training students for low level positions and thereby continued the "limitations on upward mobility for those with disadvantaged backgrounds" (p. 45). Brown Shoe Company employed workers at very low wages and "the profits from the factory were drawn out of the black community and not reinvested in it" (p. 85). The Ralston project had the greatest success because the company took a "hands off approach [which] placed resources and decisions in the hands of ghettoized blacks" (p. 112). Based on this, Neubeck points out that this is "one of the less reprehensible approaches to helping that might be undertaken by a corporation" (p. 112, emphasis added).

This last statement seems to reflect Neubeck's view that corporations are engaged in urban problem solving because of a "defensive managerial strategy which was designed to intervene in and manage a socially hostile environment." Corporate "foreign aid" tended to contribute to racial polarization and continued the colonial relationship between the residents of the ghetto and traditional power wielders

In reading this volume one must raise a number of questions. First, to what degree did the author's critical perspective influence his conclusions? Second, are these three cases of corporate activity representative of the corporate response to the urban crisis? Third, is it not useful for institutions based on the profit motive to intervene in urban problem solving, i.e., do they do more harm than good? Fourth. what would have been a "better" way of corporate intervention? Fifth, did the corporate activity have a different impact on the individuals involved than on the community as a whole? Finally, what are "structural requirements of capitalist enterprise" which must be changed to make the corporate response more viable? These questions deserve further exploration.

WOLFGANG PINDUR

Old Dominion University

Financing the New Federalism: Revenue Sharing, Conditional Grants, and Taxation. Edited by Wallace E. Oates. (Baltimore: Johns Hopkins Press, 1975. Pp. xiv + 160. \$12.00, cloth; \$3.95, paper.)

This edited volume is the fifth in the Resources for the Future Governance of Metropolitan Regions series and contains five stimulating essays prepared for an October 1974 conference. The contributors concentrate on developing the major political and economic themes of the New Federalism. Each essay is well prepared, stimulating, and quite readable, but the collection is loosely tied together and the reader soon becomes aware of five individually prepared contributions and not a neatly organized group effort. This observation is meant to be descriptive and not negative because each of the contributions is extremely useful to the researcher interested in the theoretical and empirical aspects of New Federalism. Before returning to this point, a brief synopsis of the contributions is in order.

As Professor Oates states in his introductory comments, "It is ... of great importance that we seek to understand what we have done, and are doing, in the course of modifying the structure of intergovernmental fiscal relations in the United States. This is the objective of the studies in this collection" (p. 6). Perhaps the most interesting approach to this question is the one utilized by Oates in his concluding essay dealing with the relationship between income elasticity of the tax structure and the growth of public spending. After developing a series of regression equations, Oates concludes that "for state and city governments, we found that over the decade 1960-1970 the income elasticity of the tax structure did indeed display a positive association with the growth in public expenditure" (p. 156). Oates, as he had hinted throughout this analysis, then goes on to point out the tentative nature of the findings because of time constraints with the data, endogenous variables, political considerations which could be influencing the analysis, and the fact that he did not cover the normative questions involved. Thus Oates is well aware of the factors limiting his analysis and is sensitive to the need to be cautious about them.

This is also the case with the contributions of Robert P. Inman and Martin McGuire. Inman examines the question of the "likely effects of matching, base-equalizing, or lump sum aid on local expenditures and taxes" (p. 89). Inman then develops policy models for the various types of federal aid and applies the equilibrium grants-in-aid policy model to the reform of school financing and concludes the chapter with a less than optimistic evaluation of the utility of the policy model approach. McGuire's chapter deals with the development of "...empirically testable models for describing the impact of the grants-in-aid system in price and income terms" (p. 116). Developing a series of econometric models, McGuire concludes by discussing the preliminary, but potentially useful nature of his research. As Oates and Inman do, McGuire points out the model's inability to accommodate a variety of empirical realitiesmost of these are directly tied to the political arena and public choice questions.

The chapters by Robert D. Reischauer and Jeffrey L. Pressman are much more sensitive to, the political realities dominating intergovernmental fiscal relationships. Reischauer, borrowing heavily on his knowledge of the Brookings study of general revenue sharing, concludes that "whether state and local governments' behavior will be significantly affected by the revenue sharing program will become apparent only after several years" (p. 87). Using a variety of

analytical techniques, Reischauer speculates about the probable impact of general revenue sharing on a variety of fiscal phenomena. Pressman, after analyzing a variety of the political implications involved with New Federalism programs, concludes that "... the struggle between proponents of centralization and decentralization, national power and local power, will continue to be a central one in the public arena" (p. 39). Pressman's analysis includes a wide array of the programs associated with New Federalism and his evaluation of possible political effects and impacts is quite well done. Thus we are back to the point raised at the beginning of the review.

These essays are provocative, but speculative. Each requires empirical results not yet available when they were prepared. In addition, the models and equations often ignore major political factors (such as local budgetary processes and their politics, the political power of the "haves" and "have-nots," etc.) with the result that their general applicability and explanatory value may be quite limited. Despite these reservations, the contributors carefully develop their arguments and a consideration of their underlying assumptions is most revealing. Certainly the policy maker interested in the impact of New Federalism or a specific program will not find answers here, however the researcher interested in a source of important hypotheses' to be investigated will find the veritable "pot-of-gold." The problem is to develop the data needed to investigate the hypotheses—a task which is always difficult. As a researcher long involved in evaluation research dealing with the New Federalism, one can only hope that the ideas and theoretical points raised in this collection have been thoroughly investigated by these authors or others in the past few years. In fact, Oates' basic assumption about the modification of the structure of intergovernmental fiscal relationships in the United States might even be subject to serious questioning and at the very least ought to be empirically verified. If the volume stimulates this type of research then it will be a major success. The contributions to it already are.

DAVID A. CAPUTO

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Voter's Choice: Varieties of American Electoral Behavior. By Gerald Pomper. (New York: Dodd, Mead, & Co., 1975. Pp. ix + 249. \$10.00.)

A search for the responsive voter—the individual whose electoral decision reflects policy

content—describes the main theme around which Gerald Pomper has built his secondary analysis of the "voter's choice." This book extends many of the basic concepts and findings reported earlier in "From Confusion to Clarity" (American Political Science Review, 66 [June 1972], 415–28), which, as Chapter 8, forms the heart of this volume. The work also displays the impact that V. O. Key, through his ideas about the responsible voter, has had on contemporary thinking about electoral behavior. Indeed, a secondary theme of the book is the role of parties and candidates in presenting vital issues to voters for their judgment.

The "responsive" voter is the individual citizen who can and does utilize, as a basis for decision making, policy differences that exist between the candidates. Pomper employs the now familiar V. O. Key argument that if candidates were to address policy questions during the campaign, the voters would be more likely to apply issue-laden criteria in their electoral selection. The study provides no direct evidence on how candidates and the parties have presented issues to the public over time; it. rather, utilizes national survey data from the University of Michigan Survey Research Center and Center for Political Studies national election surveys between 1952 and 1972 to determine if voters were more responsive to issues and party differences in some elections than in others. The analysis therefore sheds indirect light upon those political circumstances that either augment or attenuate the impact of issues on the electoral choice.

The antithesis of the responsive citizen, the "dependent" voter, is described as the individual who selects a particular political candidate because of more enduring determinants of the vote, such as social characteristics or traditional party loyalty. Presumably, dependent voters pay little attention to political events or issues of public policy, and instead base their vote decision on cues from their social group, economic class, race, or preferred party. According to Pomper, what has emerged from the electoral studies of the fifties is a portrait of the voter as dependent, while the contemporary voter is more accurately described as responsive.

The first half of the book is aimed at rejecting the portrait of the dependent voter. The importance of party identification, social class, sex, age, race, and education as determinants of the vote decision in recent elections is questioned. Ignoring congressional voting, Pomper argues that party loyalty and traditional demographic distinctions no longer explain the electoral choice. Party identification

has not only declined with the recent growth in the proportion of Independents, but it is also less strongly related to vote choice than in earlier years. The newly emerging Independents, unlike those of an earlier historical period, are found to be no less factually informed about electoral politics than partisan identifiers. Yet, in a fashion incongruous with the portrait of the responsive voter, Independents still participate in electoral politics at a rate well below that of partisans.

A noteworthy omission from the analysis aimed at determining if the vote choice is dependent on social background characteristics is concern with union membership, religion, ethnicity, or the urban-to-rural composition of the individual's immediate environment. The criteria used in selecting demographic variables for the analysis is not clarified, and one can only assume that age and sex are included because they represent recent interest in the youth and women's movements. It is interesting to learn that women now participate in voting at a rate equivalent to that found for men-except in the South where regional culture still suppresses female participation—and that young people are more likely than older people to be ideological liberals, regardless of their party identification. But the important point for this book is that these demographic characteristics are not related substantially to the vote choice. However, age and sex have rarely been seen as major explanations of the vote and, while interesting, this part of the analysis is somewhat of a departure from the main theme.

The most relevant discussion of the dependent voter model comes in an analysis of social class, a demographic factor traditionally assumed to affect the vote. To authenticate the dependent voter portrait, the empirical evidence would have to reveal a direct and permanent influence of social class on the vote choice. Indications of enduring and consistent class differences in the vote are, however, not evident in Voter's Choice. Pomper finds that the vote has been episodically related to class with the correlation strongest for those elections in which questions of social class were salient, thereby facilitating class voting. He thus concludes that vote choice is not so much a reflection of fixed background characteristics as it is a given reaction to the circumstances, candidates, and issues of the times.

Having rejected the theory that voters simply cast a ballot on the basis of party tradition or as a conditioned reflex of social differences, the second half of the book examines the portrait of the responsive voter—that citizen whose political actions are explicable but not

permanently predictable. The search for evidence of the responsive voter begins with an assessment of material that should be quite familiar to students of electoral behavior—party and candidate images.

The responsive voter can effectively express policy preferences only if some candidate or party will represent these concerns and promise to translate them into policy decisions if elected. To be "responsive," therefore, the voter must be aware of policy differences between the parties, must discern the direction of party differences, and must respond on the basis of these perceptions when making electoral choices.

The empirical evidence shows a consistent pattern of growth in the perception of policyrelated party differences during the period from 1956 to 1972. This change occurred almost equally across various age cohorts and at all levels of education, thereby suggesting political events as a likely cause. This increased awareness of partisan differences resulted in the clear perception of the Democratic party as the liberal party-a perception relatively independent of the voter's own partisanship or personal ideology. Yet, despite the increased awareness of partisan differences. Pomper argues that the effect of party on the vote choice has eroded and been replaced in importance by the voter's own issue preferences and those of the candi-

Numerous other writers have examined the changing role of issues and candidate personalities as explanations of the electoral decision, but Voter's Choice provides, through its use of causal modeling, a more concise method of illustrating the changes that have occurred. While the issue measures used in the causal analysis are not identical for the three time points compared (1956, 1968, 1972), the results clearly support the argument that issue and candidate contributions to the vote choice increased while partisanship decreased. The portrait is thus complete, the contemporary citizen in reaching a vote choice is more responsive to current issues and candidates than dependent on long standing party allegiance or socioeconomic characteristics.

In his conclusion Pomper calls for strong and responsible parties that take clearly distinct positions on the issues. Responsible parties would presumably provide policy content to the meaning of party loyalty and again strengthen the relationship between party identification and the vote—ironically a trait of the dependent voter.

Despite many admonitions about hos the nature of the times and historical events may

affect the reality of any portrait of the American voter, much of the discussion seems time specific. It is clear that shifts in political behavior have occurred during the past decade, and this book capably spells out some of these changes. Yet the fact that there was not more change in the face of such dramatic events as the civil rights movement, urban riots, the Vietnam war, and Watergate attests to the substantial inertia in certain attitudes and behavior patterns. Only the passage of time will allow us to test the endurance of the changes brought by the events of the past decade. Voter's Choice, while perhaps too complicated for the lay person or undergraduate because of the sophisticated analysis and intricate writing style, should be essential reading for political scientists who will want to compare the electoral impact of future political events with those of the past.

ARTHUR H. MILLER

The Liberalization of American Protestantism: A Case Study in Complex Organizations. By Henry J. Pratt. (Detroit: Wayne State University Press, 1972. Pp. 303. \$15.95.)

This sophisticated analysis of the National Council of Churches focuses on the forces that helped produce changes in policies and actions from its inception as the Federal Council in 1908 to 1970. Professor Pratt teaches political science at Wayne State. He received his doctorate in 1962 from Columbia University. This is pertinent for he served for a short time as research assistant to Reinhold Niebuhr while he was at Columbia. Therefore, this is not Pratt's first work in the politics and institutionalism of religion; also he served under one of religion's most sophisticated mentors.

Initially, his analysis is historical, a procedure which enables him to isolate three primary stages of development. From 1908 to 1933, the primary concerns in the council are social issues and problems, as one might expect during the climactic closing decades of the Social Gospel. At this time, "member churches were prepared to go along with the council's ... outspoken stance on controversial issues of the day, though not all of them were equally supportive and some actually balked" (p. 262).

From 1933 to 1963 there prevailed, according to Pratt, a general state of confusion in the council, with no clear policy guidelines, an uncertain organizational definition, and a floundering institutional identity. The New Deal took away from the council many of the "radical" social declarations, and the rise of

aggressive power in Germany diluted the message of the pacifists in the churches and the council. This complex period of trembling, if not fear, was capped by the hysteria of McCarthy and Velde and the attack from the rightwing churches whose people honestly supposed that the National Council was riddled with communists.

From 1963 to the recent past, the secular situation was again more favorable for the council; again they were needed and useful at the local and national levels. This was most notable in the arena of civil rights, and many staff specialists were added in this and other areas

Pratt does not confine his analysis to the good and bad fortunes of the council as it responds or reacts to external secular and religious forces. Internal study indicates how the organized bureaucracy changes in response to needs. Speaking generally, the staff of the council was recruited almost exclusively from the ranks of the pastoral clergy. Beginning in the 1950s, however, experts were appointed in such areas as civil rights, migrant labor, poverty, housing, voter education, black economic development, and community organization.

This is a useful book from the historical standpoint alone, covering many issues that cannot even be mentioned here, among them for example, analysis of the minority of liberal churches and their impressive power which belies their numbers, liberalism versus conservatism as reflected at the level of national organization, local councils of churches, the council and the black power movement, and the continuing search for Protestant identity. But Pratt seeks to achieve more than this by trying to lay a foundation for a "theory of interest group activism."

Having analyzed the National Council, the author does a sociohistorical analysis of the action frames of the AF of L, the NAACP, and the ACLU. He finds a striking similarity in institutional development and behavior in all four organizations. In each of the institutions, he finds a similar evolutionary pattern: an "early stage of social concern" which lasts about 20 to 30 years; this is followed by a second "stage of crisis and doubt about strategy"; and the change in strategy comes "about not in response to the reasoned objections of the critics, but to the 'logic of events' and a dramatic adverse alteration of external and internal conditions" (p. 268). Pratt is referring here primarily to power and ideological battles both within the organizations and from rival organizations. The third stage is characterized

as one of "reconsolidation and reform group alliances."

Concluding his study in 1970, Pratt was benignly optimistic about the active social concern, the "liberalism," of the National Council. "One would not anticipate any drastic changes in predisposition as long as NCC liberalism is supported by its more actively involved member bodies. True, there are numerous conservative churchmen in these same denominations; but their opposition has not succeeded in reordering priorities. Their impotency is partially attributable to the skill of council leaders in countering criticism and isolating opponents . . ." (p. 266). True, "drastic changes," whatever they might be, may not occur, but profound fluctuations in policy and procedure must be anticipated. The council is now experiencing one of those fluctuations. again from both internal and external forces. For example, between 1968 and 1975, the number of women and the number of minorities has sharply risen, from 16 to 30 percent and 8 to 25 percent, respectively. But this has not radicalized the council. On the contrary, there has been a serious drop in funding from the member denominations, due primarily to internal troubles among these groups and a drift from social concern. Within the council itself, as noted by Pratt in the later years of his study, there is an increasing awareness of the complexity of contemporary social problems. Simple declarations of religious conscience are recognized as ineffective and unnecessarily inflammatory. In any case, the council is once again in a period of reassessment and retrenchment, seeking for "deeper biblical and theological rootage." But the council continues to be maintained and staffed by predominantly liberal social forces, so the uncertain trumpet may be heard again when the time is fulfilled.

PAUL HARRISON

Pennsylvania State University

The New Federalism. By Michael D. Reagan. (New York: Oxford University Press, 1972. Pp. 175. \$1.95.)

Reviewing a book on intergovernmental relations five years after it is published can be an exercise in futility. After all, several events since 1972 have had significant effects on the federal system: the total amount of federal aid to state and local governments increased by over 75 percent between FY 1972 and FY 1977; general revenue sharing was implemented; Congress approved block grants for

manpower training, community development, and social services, further enlarging the noncategorical sector of the federal assistance system; state and federal courts tackled the issue of equity in the financing of public schools and the delivery of public services; and Watergate, the recession, and the presidential election campaign generated anti-Washington sentiment and raised doubts about the federal government's capacity to solve domestic problems. These recent developments, among others, underscore the fact that intergovernmental relations are in a constant state of flux. The dynamic nature of the subject matter can quickly transform studies of the present contours and future directions of the federal system into works of history. Fortunately, Professor Reagan's book is an exception.

The New Federalism has for the most part met the test of time. Much of the book is as relevant now as when it was published. The author's perspective is pragmatic, political, and philosophical. At the outset he notes that "old style" federalism is dead. The "new federalism" is intergovernmental relations, which have blurred the constitutional division of authority and functions between the federal government and the states. "Sharing," "independence," and "partnership" characterize the new federalism. While these themes are not new and have been developed more extensively in the writings of Morton Grodzins and Danie! Elazar, Professor Reagan takes a different position on their significance—the federal government is, and should be, the senior partner. Its authority derives from the constitution and judicial interpretations of key clauses; its power from domination of the income tax field and the growing dependency of states and localities on federal funds; and its influence from the skilled and sympathetic professionals who run the executive and legislative branches.

Federal grants-in-aid are considered the "cutting edge" of intergovernmental relations. The chapter on this subject is one of the most insightful treatments of the politics of the grant system in print. It contains a thoughtful analysis of the types and purposes of grants and a skillful probing of the rhetoric and realities of such issues as national decentralization, states' rights, federal domination, and "no strings" grants. However, relatively little attention is given to the problems resulting from the categorical grant explosion during the 1960s and the administrative responses to them.

The chapter on "Revenue Sharing—Panacea or Cop-out?" challenges much of what has become conventional wisdom about the merits of this instrument. Readers will find particular-

ly interesting the author's observation that instead of restoring vitality to state and local governments it would "in the long run, put another nail in the coffin of federalism" (p. 104). The arguments supporting and opposing general revenue sharing are well developed and, in view of the issues raised during the Congress' debate over renewal of the State and Local Fiscal Assistance Act, are still quite pertinent.

A disappointing feature of the book is the author's acceptance without serious questioning of several of the traditional indictments of state governments. While it is noted that wide variations exist in state performance across the country, for the most part states as a whole are viewed with distrust if not contempt: "State governments are structurally inadequate and politically weak even when not actually corrupt" (p. 111). This position is certainly not new to political scientists. What is surprising is that the discussion of the "proven incapacities" of the states is based more on political philosophy than on empirical evidence. Since during the last decade several states made significant strides to strengthen the governor's powers, reorganize the executive branch, revamp the legislature, restructure the fiscal system, and expand services, to ignore these developments detracts from an otherwise realistic picture of the workings of the intergovernmental system.

Although in a general sense the title of this book refers to intergovernmental relations, the author also considers the term new federalism in the specific context of the Nixon administration's domestic policy. Not surprisingly, this is viewed as being simply romantic rhetoric, a facade behind which the national government is to abrogate its domestic role, to reduce its presence merely to that of an onlooker..." (p. 163). The author suggests alternatives to general revenue sharing, including tax credits and expanded federal financing of public assistance and education.

The final chapter charts some future directions for the federal system. The general thrust of these prescriptions—labeled (regrettably) "permissive federalism" would be to make explicit and further enhance the federal government's dominant role in intergovernmental relations. The basic fact of life underscored by this phrase is that "there is a sharing of power and authority between the national and state governments, but that the state's share rests upon the permission and permissiveness of the national government" (p. 163).

Professor Reagan's approach is provocative, his argumentation is persuasive. Regardless of whether one accepts the author's point of view and the supporting evidence, The New Federalism highlights several areas where additional research and policy analysis could help dispell some of the myths surrounding intergovernmental relations.

CARL W. STENBERG

Advisory Commission on Intergovernmental Relations

Policy-Making in the Federal Executive Branch. By Randall B. Ripley and Grace A. Franklin. (New York: Free Press, 1975. Pp. 209. \$11.95.)

This volume is part of a larger project on policy making in progress at the Mershon Center of the Ohio State University. Ripley and Franklin describe the book as an interim report on the status of that research project. They suggest that the eight chapters in the volume, a number of which were written by project associates, can stand on their own but that the common framework, language, and methodology justify a single book. Whether this is true or not is less important than that the work is an heuristic undertaking designed to aid in theory building.

The framework of the study is a synthesis of three approaches to the study of public policy making that have played a significant role in political science scholarship in recent years. The three include: the comparative study of American states characterized by the work of Dawson and Robinson, Hofferbert, Dye, Sharkansky, and others; analytical efforts growing out of the typology developed by Lowi and refined later by Salisbury; and the study of decision making using a systems approach.

In organizing their work, Ripley and Franklin and their associates have divided the policymaking process into four parts: the external environment, the internal environment, policy actions, and policy results. These terms are roughly comparable to the concept "inputs, conversion process, outputs, and outcomes' used by systems theorists. The authors have operationalized these phenomena by employing variables that are relatively easily quantified, such as party strength, economic and social conditions, and public opinion as external environmental factors and agency structure, agency personnel characteristics as defined by rank and status, and similar measures as internal environmental factors. Unfortunately many other environmental variables that might be equally useful for analytical purposes, but for which quantitative data are not available, are

excluded. Although this exclusion is a source of some disappointment, it is difficult to quarrel with the authors in their effort to obtain measures suitable for statistical testing. In dealing with policy actions and results, Ripley, Franklin, et al. have used quantitative measures such as appropriations and expenditures. Again, they were aware of the limits of such variables as descriptions of outputs and outcomes. As a result, Chapter 5, prepared by Steven Shull, is devoted to examining what the authors refer to as functional actions using nonexpenditure measures. The results were inconclusive, holding out only limited promise for future research.

The most valuable contribution of this work is that it provides further empirical testing of the incremental theory of policy making. As stated by Ripley in his introduction:

The primary hunch underlying the research effort to be reported in the subsequent chapters is that policy actions, especially when measured by dollars, are not determined simply by previous policy actions. External and internal environmental factors are believed to have an important role in determining the level and kind of change in agency policy actions (p. 17).

In short the authors believe that the incremental explanation is too simplistic a way of looking at the policy-making process.

The authors have identified nine major hypotheses regarding the policy-making system. These relate to matters of agency structure, partisan division, interest group and opinion coalitions, and economic and social conditions, both general and specific to agency decision making. The study emphasizes appropriations and expenditures as measures of policy actions. Five separate studies were made using as few as eight agency sites and as many as 40. The studies were longitudinal, ranging in time from 1960 to 1971 in two instances, from 1952 to 1971 in two others, and from 1946 to 1971 in another. The authors sacrificed the benefits of in-depth analysis for the ability to do systematic and sophisticated statistical analysis. Statistical techniques employed in testing included historical trend analysis, block variable path analysis, and subsample tests for interactive effects. Bivariate as well as multivariate analyses were used throughout the work.

The findings of the individual studies, although frequently inconclusive, are intriguing and support Ripley's hunch on incrementalism. Chapter 2 reports William Holmes' finding that change in social conditions of a general nature and change in conditions on issues related specifically to agencies studied affect both

policy statements by agencies and their policy actions.

William Moreland's chapter explores the relationship between dimensions of agency maturity and budgetary policy actions in order to construct an alternative to the incrementalist explanation of budget outcomes. His findings demonstrate that, in addition to previous allocations, agency managerial capacity, agency size, and administrative experience of agency executives are all positively linked to outcomes.

The chapter that follows, authored by Lance Le Loup, identifies systematic sources of change in agency appropriations. It criticizes the incrementalist perspective on budgeting for emphasizing stability of appropriations and for defining change as random because this precludes examination of regular determinance of change. The findings show that percentage change in appropriations is determined primarily by percentage change at the agency request stage of the process rather than in the final appropriation outcome—thus indicating the relatively greater importance of the executive branch in the budget process than of Congress.

In the last two substantive chapters, Ripley and his associates link a number of independent variables such as agency maturity, partisan strength, economic conditions, and previous budgetary policy actions. Their effort results in what they call a dynamic change model in which previous budget policy actions represent only one element.

Although the authors are modest in their claims, this volume has made a significant contribution to the critique of the incrementalist model. They acknowledge that incrementalism should not be eliminated in creating a more workable model:

We are not disputing the claims that the rationality of persons dealing with complex phoenomena is limited, and they therefore deal with "successive limited comparisons" in thinking about and acting on reality. Nor, despite Gist's caveat, are we disputing the claims that budget-makers in particular think in terms of a base situation and the increments of change in it. Our data do not permit us to comment empirically on these claims and, at the level of hunch, we think they are probably in good measure true. They are certainly sensible claims, which explains in part the pervasiveness of those parts of the incrementalist perspective (p. 175).

The authors have rendered further damage to the utility of what many scholars regard to be an oversimplified model of decision making. More important, they have taken the first steps in developing their own conceptual scheme for future theory building. It remains to be seen whether the new model resulting from this enterprise will provide us with a workable theory of policy making.

ROBERT S. FRIEDMAN

Pennsylvania State University

The Dying of the Light: A Searching Look at America Today. By Arnold A. Rogow. (New York: •G.P. Putnam's Sons, 1975. Pp. 384. \$10.00.)

With some felicity, much pathos, and an irretrievable sense of loss, Arnold Rogow seeks to mobilize our errant consciences against the decline of American civilization. We are, collectively, poor candidates for this mission, for our superegos—defined as "the conscience and sensibility of the Western mind" (p. 14)—have, according to Rogow, declined almost to the point of no return. We Americans have lost the supreme value we once placed on human life and dignity, faith in reason, capacity for self-denial, respect for others. But in the twilight before the dark, with Dylan Thomas, Rogow bids us "rage, rage against the dying of the light." And he shows us how.

There is a theological quality to the work which makes the opening chapter on the Puritans, whom Rogow greatly resembles, peculiarly appropriate. They, like their successors, Fromm, Roszak, Slater, Mumford, Arendt, Jules Henry, and now Rogow, had a special knowledge of an internal state of grace which mankind imperiled with his sinful ways. Today, it is the refusal to accept a "domestic revolution of falling expectations" (p. 14) that is the source of the trouble. And the central problems, in addition to the decline of the superego mentioned above, are: "continuing loss of affect (or the capacity to feel and to respond to others), a further expansion of permissive sexual culture [about which Rogow is most ambivalent]..., decline of the family, diminishing ethnic and cultural diversity (and the eventual dominance in the world of a universal or 'pop' culture of which the leading characteristics are American), increasing conflict between hedonistic consumption and resource scarcity, and a movement away from traditional and personal freedoms" (p. 13).

While these are trouble enough, they represent only those occurring to the author as he wrote his preface. As he writes the book, the focus turns to the graceless state of the economy, with its continuing inequalities and injustices, the desecration of the city, rising crime and violence, the cruelties of racial oppression

and the degradation of the blacks by a repressive society, corruption of the media, and the lonely, bored life of the suburban housewife, the fear of communism (because it abandons the father principle), the corruption and unresponsiveness of government, and, of course, the trashy sex life of the times. The indictments are partially true; Rogow has the evidence which he lays before us without mercy—or much interpretation.

Why do we not do something about this? We do not move our indifferent selves to rekindle "the light that once blazed so brightly" in America because of the decline of conscience and sensibility, but more importantly because it is in the nature of reformers to be ambivalent about their own reforms. We feel guilty about our own violation of the conventional corrupting norms; thus, we are only half-hearted about reform, while the "forces of the Right" act from conviction (pp. 308-09).

Perhaps it was the purpose of this eloquent narrative account of pain and worthlessness to restore to us the conviction eroded by the corrosions of ambivalence. For this purpose, rage is useful, moral appeal relevant. But for the purposes of understanding, not as moralists, but as social scientists, or as social humanists, the work leaves us to create our own comprehension out of the materials of indignation. We have seen enough neo-Marxists straining to pour the materials of history in a leaky vessel not to ask for more, but there is an economic structure that affects the culture of which Rogow now almost despairs. The demand for Freudian causal interpretations of our decline, after Marcuse, Rieff, and Norman O. Brown, may be temporarily satiated; but even here, where Rogow has considerable expertise, the systematic working out of a theory of instinct and its vicissitudes is missing. Indeed, there is no system at all, not even the kind of partial system which Daniel Bell, or Jacques Ellul, or Hannah Arendt offers. The work is not grounded in anything but observation, much of it through careful clipping of the New York Times.

Modernity is a Rorschach. As Rogow says, "The supreme difficulty facing any interpreter of life in America is that almost any statement he wishes to make about the United States he can show to be at least half true" (p. 11). We know why the Right is so passionate in its views: it defends its privileges, expresses its authoritarian personality nurtured in patriarchies, "experiences anxiety in the face of uncertainty" (G. D. Wilson, The Psychology of Conservatism, 1973, p. 259), responds defen-

sively to ambiguities, and so forth. But Rogow's impulse to rage against the dying of the light, under this condition of half truth, needs explication. Herewith a modest list: (1) It represents a reaction formation against the death wish, or perhaps a projection: "Society is dying, I am not." (2) It represents the revenge of the frustrated reformer: "I cannot change things; therefore it must be that people are irremediable." (3) It represents a search for grace or absolution or avoidance of complicity: "I warned them to mend their ways." (4) It represents the struggle against Rogovian ambivalence, a reaffirmation in the face of doubt. (5) "This high I.Q. whimpering on a cosmic scale occurs whenever an external source of values fails to work" (A. Maslow, Toward a Psychology of Being, 1968, p. 16). When a child of the New Deal finds his faith in reformist liberalism shattered, he rages against the dying of the light. (6) Without moral passion, the tepid liberal loses his will. And (7) because the light is dying.

ROBERT E. LANE

Yale University

Achieving Effective Desegregation. By Al Smith, Anthony Downs, and M. Leanne Lachman. (Lexington, Mass.: Lexington Books, D. C. Heath, 1973. Pp. 128. \$8.50.)

Achieving Effective Desegregation is a practical guide for the school administrator who faces the task of desegregating a school district, wishes to avoid disruptions in the life of the school and community, and aims to achieve a quality education for all students. The authors' primary thesis is that committed leadership on the part of the superintendent and school board, who emphasize the "inevitability" of desegregation, take the initiative in developing their own desegregation plan, and move steadily toward developing an effective plan, will decrease the probability of disruption. They then proceed to present a detailed outline of the process of achieving effective desegregation starting with the moment the decision is made to desegregate the schools through the opening day and the entire first year of desegregation. The process is divided into three phases: planning, initial implementation, and positive development.

The authors use information from the literature on desegregation, from a field survey of ten districts, from essays by experts in the field, and from interviews with knowledgeable per-

sons. An immense amount of useful information has been distilled into a carefully crossindexed manual. Techniques cover a full range of concerns. Administrative measures include a discussion of improving representation in decision making, suggestions for promoting community support, a program to reduce staff uncertainty, and an approach to revising the role of the principal. The authors cover new classroom management techniques, new curriculum materials, approaches to improving teacher sensitivity to diversity, and they offer ideas on new supervisory arrangements to improve teacher performance. A section on community relations contains useful proposals for reducing community uncertainty about desegregation issues through a "rumor center," through an "open door" policy in the school, and through developing a School Board Communication Policy. They present advice on methods for gaining community participation in implementing the desegregation policy and obtaining the cooperation of other community agencies.

The authors emphasize the importance of including students in the planning, and they counsel the reader on the importance of forming democratic school structures which includes students of all ethnic groups. They suggest ways in which the curriculum and the guidance services can be used to improve student behavior, adjustment, and achievement while making a strong case for the maintenance of discipline within the school. The section on techniques for achieving effective desegregation also concludes with recommendations for diversifying the contents of the curriculum and changing classroom organization and scheduling to promote small-group work.

The reader needs to observe two cautions in reading the book. While many of the proposals made by the authors have been tried successfully in desegregating districts, other proposals are untried suggestions made by the authors on the basis of their observations and experience. The reader needs to be alert to separate the two types of counsel. The very timely nature of the material in the book creates the problem of obsolescence. For example, lists of consultants and resource agencies already need updating, and the suggestions for reading and for curricular materials will become less useful as time passes since they will not be current with the rapidly changing educational scene.

The administrator faced with the task of desegregating a school district will find Achieving Effective Desegregation a helpful guide in anticipating problems and preparing for critical junctures after assessing alternative courses of

action. It is the best treatise currently available for persons seeking practical information on a wide variety of educational problems faced by educators who are seeking to achieve the goal of effective desegregation.

JANE R. MERCER

University of California, Riverside

The Sources of American Student Activism. By James L. Wood. (Lexington, Mass.: Lexington Books, D. C. Heath, 1974. Pp. xviii + 187. \$15.00.)

As the American student movement fades into the background, further assessments about its nature and impact continue to appear. Why did it happen, who comprised its rank and file, what was the place of ideology, how did it reflect lineage and generational tensions, and why did it wane, are among the leading questions. Scholars have tended to fall into two camps when addressing the student movement. Some view it primarily from the standpoint of a social movement, with emphasis on ideology, organization, and social dynamics. Others are more inclined to take a socialization perspective, with emphasis on the precursors of involvement, motivational structures, and personal consequences.

In this transformed Ph.D. thesis Wood emphasizes the socialization perspective but he is not unmindful of the social movement aspect. Appropriately enough, the research setting is the birthplace of the student movement, the University of California at Berkeley. The data base consists of interviews in 1968 with a random sample of undergraduate and graduate students (N = 482, response rate = 89 percent) gathered by Robert H. Sommers and one set of parental questionnaire data gathered by Sommers and another gathered simultaneously by Jeanne Block (combined response rate about 60 percent).

The most successful parts of the book are theoretical in nature. After briefly surveying the nature of the student movement and the various interpretations of it, Wood sets out to develop a middle range theory. This theory combines the radical socialization approach with the family conflict approach. Rather than seeing these as mutually contradictory, as is the usual stance, he argues that they are complementary. The essence of his position is that

many activists act out radical political values that are derived from their parents' radical political or culturally unconventional values; yet the activists come into conflict with their

parents because the activists feel the parents have not lived up to their own political or cultural values (p. xvi).

Although the accompanying analysis lends at best moderate support for the position, it is at least as defensible (and in my view more so) than the monistic theories typically advanced. Wood sticks conscientiously to exploring various explanations throughout the book; this steadfastness is perhaps its greatest strength.

As with many dissertations turned first book this one pays excessive tribute to authority figures and belabors methodological niceties already within the grasp of most readers. On the last point, for example, much attention is paid to the "elaboration schema" designed to test and explicate two-variable relationships as though it were something new to the social sciences. This tediousness reaches its zenith with the adaptation and inclusion of two tables from a secondary source to make a point about contingency relationships (pp. 88–89).

Given the author's methodological self-consciousness it is all the more surprising to note questionable decisions and interpretations. Case numbers are often perilously low and might sometimes account for seemingly anomalous findings. Using parental reports to characterize power and affective relationships between parent and growing child is a weak technique at best. To use student self-reports on their relations with parents as the criterion for determining whether activism had increased or decreased lineage harmony is equally suspect, as is the assertion (based on synchronic data) that student radicalism leads to parental radicalism.

An example of a different sort of temporal problem occurs when the author tries to analyze the social characteristics of activists over time. Working only with the 1968 student survey, Wood attempts to show the changing composition of the 1964 Free Speech Movement, the 1966 Student Strike on campus, and the 1967 Oakland Induction Center demonstration. Whether the various "findings" actually correspond to reality is beside the point, because the data base will simply not allow one to address the question. The turnover in the student population was enormous across these four years. Illustratively, of the 492 respondents in 1968, 29 percent were on campus in 1964-65, 59 percent in 1966, and 91 percent in 1967. Trend interpretations could be rendered only if the relevant time-specific populations had been sampled.

The error is compounded by statements about the absolute amount of activism over the four years. The conclusion is reached that

activism decreased among virtually all social groupings. But that clearly cannot be deduced from these data. Is it any wonder that the author is drawn to the contradiction between his Berkeley findings versus those from replicated cross-national surveys. Even if he were right about the broadening social base and overall decline at Berkeley, he would be right for the wrong reasons.

The book is also curiously deficient in one area where it should be especially strong. Since the early 1960s there has been a veritable flood of literature regarding political socialization. Because much of what the author discusses draws upon pre-adult experiences and sources of political orientations one would have expected at least some utilization of the theoretical and substantive outpouring in the area. Yet that is not the case, and the book is the weaker for it.

On the other hand, and much to his credit, Wood does not take the extant literature on student activism as the received wisdom. He very correctly points to several deficiencies of some of the landmark writings and attempts, within the limits of the data, to overcome these deficiencies. If for no other reason, such efforts would stamp the book as a useful contribution to the literature on the student movement. Whether one wants to pay the extravagant price which the publisher has attached to this slender volume is another question.

M. KENT JENNINGS

University of Michigan

Governing American Schools: Political Interactions in Local School Districts. By L. Harmon Zeigler and M. Kent Jennings with G. Wayne Peak. (North Scituate, Mass.: Duxbury Press, 1974. Pp. 269. \$12.95.)

In some respects this book by L. Harmon Zeigler and M. Kent Jennings with the assistance of G. Wayne Peak is neither important nor interesting. The format and theoretical underpinnings are taken from elsewhere-a point freely suggested by the authors-while the methodology has been used frequently before. Indeed, the authors even resorted to using a sample designed for different purposes and used before by one of them in a previous study. Once the apparatus and techniques for largescale national surveys has been perfected, as certainly it has at Michigan's Survey Research Center, the cynic might dismiss a study such as this as just another "easy" method of expanding one's list of publications. In addition, books

reporting the findings of such surveys are tedious reading at best. Rare indeed is the author—one is reminded here of the late V. O. Key, Jr.—who can bring such data to life.

These limitations notwithstanding, there are other respects in which this book is groundbreaking. The study of the politics of schools is not new by any means, but most attention is recent and could benefit from the data base now generated by Zeigler and Jennings. It may not be glamorous, but Theodore Lowi's contention that taxonomy should precede ontogeny or phylogeny-a notion long established in the so-called hard sciences-makes eminent good sense in the study of governing our nation's schools. After all, there are about 16,000 units of government, with approximately 90,000 elected officials, controlling the expenditure of some 50 billion dollars, or more than 20 percent of state/local expenses.

The reluctance of political scientists to study school politics-except as a part of larger social issues-stems from many factors. Zeigler and Jennings suggest that it results from political scientists having been "so persuaded by reformers' arguments to keep schools separate from politics that they have virtually ignored educational government as an appropriate object of study" (p. 3). Perhaps most important in the present study's attempt to open this vast area to further investigation is their framing of the dichotomy between the reformers' dream of nonpolitical schools and democratic theory. If one accepts democratic notions of elected representatives translating majority preferences into public policies then schools don't come off very well. Indeed, a recent Gallup Poll (1975); commissioned by the National School Boards Association found 63 percent of those polled unable to name a single thing their school board had done in the past year. With such little interest, Zeigler and Jennings' findings that incumbents are virtually assured of reelection if they so desire, that boards tend to name their successors, and that boards bear only slight resemblance in social characteristics to the population they represent, should come as no surprise. Moreover, few members are interested in furthering their political careers, thus removing the constraints-of-ambition theory.

But schools are different from most other public bodies in that they are also service organizations. Thus one can take the view, which reformers certainly did, that with politics out of the school business schools could be run by those who know what education is all about: that is, by the experts, the educational managers, or more specifically, the superintendent. Governing American Schools documents

the shift in direction of policy making that has occurred under this operative assumption. Superintendents make policy and boards serve as legitimizing agencies, justifying and selling these policies to the community.

Zeigler and Jennings found the most competition for school posts and greater citizen input in those districts that were unreformed—that is, where board members were elected from districts or wards or where the choice remained partisan. Recent attempts in Los Angeles to revert to election from districts, suggests that those who favor more citizen input into school decisions had intuitively grasped what Zeigler and Jennings are documenting. Although maintaining objectivity in reporting these findings, I think the authors would prefer more politics and less expertise in the running of our nation's schools.

Other findings in this study should prove of heuristic as well as practical value. For example, practitioners might wish to peruse the findings of board-superintendent interaction; scholars might enjoy exploring the fact that right-wing groups do not appear to be as involved in school politics as left-wing groups. Concerning the latter, one might pick up a hint given by the authors, which is developed more fully by critics of schools in general—that one need not organize to influence something that one agrees with. The point being, Zeigler and Jennings have provided a wealth of data and suggestions from which the study of school governance might emerge as an important component of political scientists' research.

ROBERT S. ROSS

California State University, Chico

Comparative Politics

Economic Growth and the Public Sector in Malaya and Singapore, 1948–1960. By Lee Soo Ann. (New York: Oxford University Press, 1974. Pp. 192. \$20.00.)

Rising interest in the comparative study of public policy has focused attention on the paucity of data and case studies on public policy for Third World countries. A small segment of that void is filled by this case study of Malaya and Singapore during the period from 1948 to 1960. In this book, the author explores the relationship between public fiscal policies and economic growth. He begins without stating any theoretical propositions or enunciating any specific developmental strategy, but he does state his preference for an open

economy in which foreign investment and enterprise are encouraged, but with government playing a positive and guiding role to convert trade and private investment into a "catalyst for sustained growth" (p. 4). The book proceeds on the assumption that government expenditures and investments were critically significant factors in sustaining the fairly impressive economic growth of Malaya and Singapore during the period under examination. By his analysis of public expenditures, revenues, trade policies, and shifting budget priorities, the author seeks to identify those public policies that were effective in creating conditions for balanced economic growth.

While the book presents a wealth of information on government revenues, investments, expenditures, and fiscal policies, the author is unable to come to grips with the central problem of isolating the effects of public policies from "environmental factors" beyond government control, such as world market conditions, the availability of private investment capital, and the accessibility of foreign assistance. It is of some significance to note that the years covered by this study almost exactly encompass the period of the Communist guerrilla insurgency in Malaya. This was also a period when the Korean War and the stockpiling policies of the United States government drove up the prices of rubber and tin, the two largest trading commodities produced in Malaya. Because of export duties on these products, government revenues rose dramatically during this period. In numerous tables and in the text, the fiscal configurations of all these factors are described, but there is little analysis of the impact of specific policies on overall economic performance. Instead, the author presents piecemeal observations on factors that he identifies as probably contributing toward stimulating various aspects of the economy. He suggests that government should have diversified its tax base and engaged in more domestic borrowing. He also concludes that a large share of the unexpected government surpluses accumulated during this period came from consistent underestimation of revenue and overestimation of expenditures. These budget surpluses helped to pay for the heavy costs of counter insurgency operations, but they were also able to be diverted to infrastructure investments, for social services and for the costly rubber replanting scheme, all of which, in the long run, stimulated productivity and economic growth. While a number of other economic policies of government are mentioned with approval, it seems rather paradoxical that one

of the most important aspects of successful economic policy was derived from conservative errors in estimating government revenues and expenditures.

While political scientists may not find the economic analysis particularly relevant to their interests, the large amount of data collected on revenues and expenditures for Malaya and Singapore reveal the overall patterns of government priorities. Consequently, this book could be utilized as an important secondary source for public policy analysis of Malaya and Singapore. The book gives only slight attention to the processes of economic planning and public policy formation, and completely ignores the operations of the Economic Planning Unit which was established within the Malayan prime minister's department to assist the cabinet in the preparation of economic development plans. Some of the reports of this agency could have been put to good use in this study, but may not have been available to the author at the time of his research.

In the years since 1960, many important changes have occurred in the economies and public policies of Malaya and Singapore. The formation of the wider Federation of Malaysia in 1963 brought problems of integrating the economies of the member states. Singapore's exit from the federation two years later prompted that state to engage in a crash program of expanded industrial production and services geared for customers and trade on the world market. International oil companies began a massive development of off-shore oil resources. In response to the traumatic racial riots of 1967, Malaysia embarked on an ambitious and controversial New Economic Policy designed to effect a dramatic racial redistribution of wealth, ownership of industry, and occupational patterns in all sectors of the economy. For both Malaysia and Singapore, political and social considerations received far higher priority in economic policies and development planning. This volume would have been more valuable had it been extended to cover these more recent developments. In its present form, limited to the years from 1948 to 1960, the book should have been published at least a decade earlier, and even then it would have attracted the attention of a small and specialized reading public.

GORDON P. MEANS

McMaster University

An African Dilemma: University Students, Development, and Politics in Ghana, Tanzania, and Uganda. By Joel D. Barkan. (Nairobi: Oxford University Press, 1975. Pp. vii + 259. \$15.75, cloth; \$9.00, paper.)

An African Dilemma presents an overview of university students in Ghana, Tanzania, and Uganda, emphasizing how they conceive of their present and future roles in society. The study is based on three surveys in 1966 and 1967 involving self-administered questionnaires filled out by 1,900 students at Makere University College in Uganda, the University College, Dar es Salaam, and the University of Ghana, Legon. After a review of basic student values and career intentions, the book examines perspectives on national integration and economic development, political participation, political socialization, and perceptions of incumbent elites. In a provocative final chapter, Barkan considers the implications of his findings for development and political change. Appendices include a section on methodology and the text of the questionnaire.

Barkan's data indicate that social background is not a major determinant of student values and role orientations. Moreover, he argues that the educational system tends to "homogenize" social distinctions such as ethnic origins. Students expect to become part of an upper-middle-class technocratic elite working in state-supported activities, primarily education, the professions, and the civil service. Success is equated with personal and/or communal security, making a contribution to the development of the country, and doing one's job well. Elite status, per se, is not something students strive for. Nor do the students appear to have grandiose ideas about their career possibilities: desired and expected occupations coincide.

Students consider their countries' most pressing problems to be developing the economy and promoting social integration ("...defined as the progressive elimination of class and ethnic cleavages through an increase of the proportion of a society's population which interacts within its national institutions" p. 85). However, few students choose careers in agriculture, the primary economic sector in need of development. Likewise they are somewhat indifferent to the existence of elite/mass gaps, rationalizing that a technocratic elite makes a significant contribution to the developmental process. Barkan suggests, as did Coleman some years ago, that such indifference is a potentially dysfunctional result of the educational system and may be the cause of more serious class conflict.

Although students intend to "work hard at their jobs" to further development, they are unsure about what economic or political model their nations should follow. Most opt for a mixed economy and favor a technocratic political order that Barkan considers has authoritarian overtones reminiscent of pre-independence administrations. Few students seem to question the existing political system except as it affects their material interests. Compared with students in other Third and Fourth World countries, their political participation is only marginal. They seem unlikely to challenge existing elites even though they describe some of them in pejorative terms. For upwardly mobile students, quietude is both rational and a survival technique.

Comparative Politics

Although this concise and cogent book gives us a fuller view of students than is found in the many case studies based on single African countries, Barkan seems ambivalent in his assessment of what his findings mean for the future. He sees the students contributing reliability but not creativity to development, change, and stability, and he rightly questions whether this is sufficient. In one passage Barkan states: "...it is clear that existing procedures for granting status and material security in Africa, and the values communicated by the educational system must be changed if the prospects of development are to be raised" (p. 197). But how is this to be done and by whom? Political order and stability may be correct virtues leading the way as Barkan suggests, but every tyrant as well as democrat argues that. The dilemma is not where students fit in but what they fit into and toward what ends.

Anyone who has attempted survey research in Africa will appreciate the effort behind this study. Although I have questions about Barkan's decision to present a participation scale based only on respondents whose answers met Guttman criteria, it is still an informative and well-written book. I believe it belongs on the shelf of anyone concerned with students, development, and the role of education in society.

DAVID J. FINLAY

University of Oregon

Patterns of Political Development: Japan, India, Israel. By Roger W. Benjamin, with Alan Arian, Richard N. Blue, and Stephen Coleman. (New York: David McKay, 1972. Pp. xi + 192. \$9.95, cloth; \$4.95, paper.)

A variety of possible relationships between the three main variables is discussed briefly in the introduction and then data from Japan, India, and Israel are examined with these hypotheses in mind. Generally, literacy and levels of urbanization and industrialization are related to associational activity, which in turn is related to participation operationalized as voter turnout, party competition, support for minor party candidates, and so on. Since each country is unique in some respect and since different kinds of data are available from each, so the variables are constructed in slightly different ways for each country. For example, districts are compared in India and prefectures in Japan, but five different settlement types spread over the country are compared in Israel. Not forcing data into a preconceived pattern is a commonsense solution to widely discussed problems of data collection which does not seem to detract from the results, since similar patterns are still detected in the three countries. By extensive use of graphs, one of the book's strongest points, Benjamin has managed to restore the visual component to the word "patterns" so that even the statistically untutored can understand the relationships evidenced by the data.

One way in which the graphs are particularly effective is in showing that most relationships between the variables are not linear. For example, a finding that supports the suppositions of other theoreticians is that the proportion of people voting tends to decline after some critical amount of urbanization is reached, although the two variables are positively associated at lower values of urbanization. As the authors note, the nonlinearity of relationships would be still more obvious if data were available over a period of time.

There are a few difficulties in this as in any book. The introduction contains the by-now obligatory discussion of problems of conceptualizing and constructing indicators for modernization, along with a brief discussion of the "natural science trend" of hypothesis building and testing in the social sciences. I am sure it increases the scholarliness of the book to refer to previous literature on the subject, but the section offers no new interpretations or solutions to the difficult problems of defining modernization or building hypotheses about it. The conceptual fuzziness of this section contrasts poorly with the clarity of the data manipulations that follow and detracts from Benjamin's implicit argument that pragmatic solutions to the problems are most helpful at this point in our understanding of modernization. In addition, since it is patterns we are looking for, it would be more useful to have the summaries at the end of the case studies more directly parallel. The chapter on Israel, for example, has a wonderful page of summary graphs but the other two studies have helpful verbal recapitulation; both would be helpful in all three chapters. Also each case requires a second reading in order to follow exactly what form the hypothesis is taking and how it will be tested for that country.

Despite these few drawbacks, I would recommend this book to anyone trying to teach realistic yet creative research design, to anyone interested in modernization, or to anyone concerned with a common sense approach to the problems of scientifically interesting patterns in data. In short, almost all political scientists will profit from the graphs and the sensitive approach to aggregate data employed here.

SUSAN G. HADDEN

Clark College

Legislative Systems in Developing Countries. Edited by G. R. Boynton and Chong Lim Kim. (Durham, N.C.: Duke University Press, 1975, Pp. 286. \$11.85.)

Despite the growth of a substantial body of literature in the past decade, much remains to be learned about the development and functioning of legislatures in various political systems. This is perhaps especially true for "developing" systems where empirical research is oftentimes difficult and fraught with a host of theoretical and methodological pitfalls. The first book-length publication of the Consortium for Legislative Studies (general editor: Malcolm E. Jewell), Legislative Systems in Developing Countires, represents the collective effort of several scholars to further knowledge of legislatures and legislative behavior in a number of Third World polities.

Structurally, the book is divided into three sections entitled "Legislatures and Goal Setting Function," "Legislatures and Conflict Management," and "Legislatures and Integrative Function," respectively. Under these headings are placed a series of seven substantive papers and two general theoretical essays. Legislatures studied in detail include those of Japan, Thailand, Colombia, South Vietnam, Kenya, the Philippines, and Korea. Additionally, one of the "theory" papers discusses at some length the historical development of legislatures in Iran, Turkey, and Afghanistan. As is typical for a book of this type, the entire set of papers is preceded by an introductory chapter by the editors.

The basic fact that legislatures in most political systems—including all of those studied

in the several papers in the Boynton-Kim volume-are "weak" in the sense of playing at best a "minimal" role (the term is Mezev's) in what the editors call the "goal setting" function is recognized by the editors as well as each of the contributors. Recognition of the sharply truncated role that legislatures have in policymaking processes leads the researchers to explore alternative functions performed by these bodies. Hence, the previously mentioned focus on "conflict management" and "integration." The shift in attention to these functions leads many of the authors, when studying the behavior of individual legislators, to explore such phenomena as representational role-orientations and constituency service behavior. The assumption underlying examinations of the attitudinal and behavioral linkages between legislators and their constituents would seem to be that interactions between legislators and constituents may constitute an important device by which political support is generated. Intuitively, such an assumption seems quite plausible.

Two caveats, however, are in order. First, neither the authors nor the editors provide the empirical evidence necessary to sustain the assumption that legislators' attitudes and behavior influence support levels significantly. Indeed, only one paper (a study of the Kenyan legislature by R. F. Hopkins) presents any data whatsoever on citizen perceptions of the legislature. In brief, as Wahlke and others have recently noted, the extent to which legislatures as institutions and legislators as individual political actors influence political support levels remains an important task for future research, with only a handful of inquiries on this topic, being currently available.

Second, and relatedly, none of the several contributors nor the editors seem sufficiently cognizant of the distinction between support for the legislature and support for the larger political system. If, for example, legislators, through their representational and constituency service behavior contribute primarily to the development of a sense of diffuse support for the political system as a whole, then, paradoxically, the behavior of legislators in systems where legislatures are currently "minimal" may be contributing (albeit perhaps unknown to the legislators) to the maintenance of the status quo. Alternatively, it may be that legislators' performance of representational and constituency service activities contribute to the generation of support for both the legislature, as a perceptually distinct political structure, and the larger political system. Theoretical expectations regarding the relevance of the behavior of individual legislators for understanding the

structure and functioning of legislatures as institutions will be greatly strengthened when information on these matters is available.

The comments above should not be taken to imply that the papers in the Boynton-Kim volume have serious conceptual deficiencies. On the contrary, in most instances, the authors demonstrate considerable conceptual sophistication. Particularly noteworthy is a commendable sensitivity to historical and cultural factors influencing legislatures and legislative behavior in non-Western settings. Most of the authors strike a judicious balance between studying the legislature as an institution rooted in the historical and cultural circumstances of a particular polity and investigating the attitudes and behaviors of individual legislators. Additionally, attempts are frequently made to assess the broader relevance of micro-level findings.

Regarding the behavioral research, all substantive papers are based on substantial bodies of survey data. For the most part, analyses of these data are performed in a straightforward and competent fashion. There are minor exceptions. Styskal, for example, in his paper on the Philippines, incorrectly assumes that "beta weights" can be squared to yield the percentage of variance explained (p. 243). Although distressing when encountered, such methodological errors are happily rare and do not invalidate the general thrust of the arguments being made.

In sum, the several papers in this volume make a definite addition to our knowledge of legislatures and legislative behavior in non-Western countries. One could perhaps summarize the quality of work done by saying that the contributions are "solid" rather than "outstanding." As the first in a projected series of books on comparative legislative research, the generally high standard of the papers in this initial publication suggests that more valuable work can be expected from the Consortium for Legislative Studies in the future.

HAROLD D. CLARK

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Lunda under Belgian Rule: The Politics of Ethnicity. By Edouard Bustin. (Cambridge, Mass.: Harvard University Press, 1975. Pp. xx + 303. \$11.00.)

The Lunda state, in southwestern Shaba (alias Katanga), Zaire, is one of the most prestigious political configurations of Central Africa. Historically, this derives from the central role played by the Lunda state from 1600

on in the dynamics of state formation in the savanna regions of what are today Zambia, Zaire, and Angola. This impact was felt partly through its own expansion, partly through the diffusion of its forms, and partly through the periodic secession of rebel fragments who then replicated many of its structures. With the independence era, occasional speculation about a reconstituted "Lunda Empire," and the incandescent, ultimately tragic career of the sometime Prime Minister and erstwhile secessionist leader Moise Tshombe, of Lunda royal connections, combined to renew the aura of the kingdom. Even today, the Lunda paramount rulers are among a handful of chiefs whom the centralized Mobutist state has been unable to totally eclipse or dominate.

Although in the universe of Zairian precolonial societies it is by no means a typical example, Lunda is nonetheless an arresting case. which merits the meticulous attention it has received from Professor Bustin in this admirable book. The central focus of his inquiry, as suggested by the first part of the title, is the interaction between the Lunda institutions and Belgian colonial administration (though Lunda domains spilled over into Zambia and Angola, the seat of the kingdom was in Zaire). However, he completes the study with intriguing chapters analyzing the formation of the Lunda state and its nineteenth century crisis on the eve of colonial conquest, and the fate of Lunda during the turbulent first decade of independence. This perspective is more distinctive than it might at first appear; political scientists who have examined African historical states have tended to do so in interaction with nationalist movements and political parties (Lemarchand, Apter, Whitaker), while anthropologists have focused upon the African society somewhat abstracted from the colonial overlayer. Now that centralized, authoritarian regimes seem in the ascendant, the insights derived from the Bustin approach become quite pertinent. Parties no longer mediate between administration and those historical states that survive; the bureaucracy seeks devices to bind these local structures to its purposes.

Any system as distinctive as Lunda is bound to have attracted the analytical notice of administrators and anthropologists (Biebuyck, Crine, Duysters, Hoover), and Bustin has their work to draw upon. However, the major underpinning for his inquiry was in the administrative archives. These have been quite thoroughly exploited, in field work undertaken in 1967, 1968, and 1972. He also did some interviewing of Lunda notables, and Methodist missionaries and Belgian administrators who have labored in

the area. Some may regret that more recourse was not made to interviewing, exploiting in party the analysis of the arcane craft of oral tradition that historians have developed. Yet the archives themselves are a quite rich source, and the solidity of the documentation is beyond dispute. Bustin's impressive immersion in the extensive historical and anthropological research which has appeared in recent years provides the equipment for creative utilization of the archival material. In its multidisciplinary perspective and breadth of vision, the present work seems light years away from Bustin's first, heavily institutional book, La decentralisation administrative et l'évolution des structures politiques en Afrique orientale britannique.

Bustin skillfully relates the enduring principles of Lunda political organization, positional succession and perpetual kinship, to the organizing concepts of Belgian administration, preservation of the colonial order, labor recruitment, and cash agriculture, lightly overlaid by the influences of "indirect rule." Above all he demonstrates how much scope there was for the impact of a particular administrator, for the dependent manipulation of the colonial system by power contenders within the Lunda orbit, for major fluctuations of policy over time. Global characterizations of Belgian colonial policy tend to obscure the range of variation on these important dimensions.

The subtitle for the book is unfortunate, as Bustin is not centrally concerned with ethnicity per se. He does offer a brief and interesting analysis of the basis of Lunda identity, which is above all political and linked to the kingdom and not linguistic. However, this is not related to the ethnicity literature generally, nor do we receive an adequate treatment of the bases of solidarity among the most important "relevant others" for Lunda: the Cokwe, an historically related cultural system, whose hostile symbiosis with the Lunda state created many of the most difficult issues for central administration, colonial and postcolonial.

Overall, Bustin merits our praise and gratitude for this carefully documented study, which is a valuable contribution to our understanding of political change in Central Africa.

CRAWFORD YOUNG

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Lectures in China, 1919–1920, by John Dewey. Edited by Robert W. Clopton and Tsiun-Chen Ou. (Honolulu: The University Press of Hawaii, 1973. Pp. 337. \$12.00.)

Improbable though it seems today, there was a time, some half a century ago, when the ideas of John Dewey appeared more likely than the theories of Marx or Lenin-not to mention the then unknown Mao Tse-tung-enduringly to influence the destiny of China. In the late 1910s and early 1920s, Dewey had a considerable following among "progressive" Chinese intellectuals, including several of the most esteemed leaders of the iconoclastic "New Culture Movement" who had themselves been his students at Columbia. More to the point. Dewey and his wife arrived in China in May 1919-coincidentally at the beginning of a period of feverish academic and political upheaval-for a lecture tour which lasted, finally, more than two years. During this time, Dewey travelled to almost every important intellectual center in the country, usually accompanied by one or another of his eminent Chinese disciples. to expound in person his philosophy of education and his social and political views. It was, as many observed hopefully at the time, an unprecedented intellectual occasion and opportunity.

Lectures in China, 1919-1920, is a memento of that encounter, so auspicious in its time, now so anachronistic. The book comprises only two of the several series of lectures which Dewey delivered in the course of his stay (some 30 out of a total of perhaps 150 formal presentations), one on "Social and Political Philosophy," the other entitled "A Philosophy of Education." Both were delivered in the winter of 1919-1920 at Peking National University, China's premier institution of higher education and the indisputed center of intellectual radicalism. As with all Dewey's China lectures, these were given in English, translated paragraph-by-paragraph from the platform by one of Dewey's Chinese hosts, transcribed in Chinese, and in this form published in the periodical press and in collections, some of which went through many printings.

Unlike Dewey's lectures at Tokyo Imperial University some months earlier, which were later published as Reconstruction in Philosophy, the English texts of his lectures in China, or the notes from which he spoke, did not survive. What he had to say to his Chinese audiences can be gleaned only from the now scattered Chinese versions or, laboriously, from retranslations such as these. Dewey scholars are not likely to find here anything new or startling, though Dewey's awareness that he was breaking new ground for most of his listeners, and the fact that several of his interpreters were themselves men of wit and eloquence, give these lectures a cogency and directness rarely

found in his more formal writings. The real usefulness of Lectures in China, however, is to the historian interested in the transformation of Chinese thought in the early decades of the twentieth century. Dewey was an eminently commonsensical advocate of a certain strain of American liberal values: a sustained tradition of intellectual self-criticism; political civility and moderation; free access to, and unhampered dissemination of, information; socially responsible individualism. We have in these lectures a fair approximation (what translation can be more?) of opinions that circulated widely among Chinese intellectuals and had a significant, albeit transient, impact on the manner in which they assessed their situation and their options. The fact that Dewey was so often frankly critical of the traditions he sought to explain may well be a factor in their ultimate failure to act upon the values he expounded.

Professors Clopton and Ou preface the translations with a minimally helpful introductory essay that sketches the context of events within which Dewey's China visit was conducted and traces the rise and decline of his influence. Or perhaps one should say, of his popularity, since the question of influence is intentionally evaded. The introduction was apparently written several years before Lectures in China was finally published, and therefore does not benefit from much recent scholarship which has substantially enhanced our understanding of the period and of several of its leading personalities. More useful is an appendix which lists a number of additional lectures (some with brief abstracts of their content) which, though not printed in this volume, have been translated and are available in reprographic copy.

JEROME B. GRIEDER

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In the Spirit of the Red Banteng: Indonesian Communists Between Moscow and Peking, 1959–1965. By Antonie C. A. Dake. (The Hague: Mouton, 1973. Pp. vii-xvi + 479. Guilders 48.)

Indonesian Communism Under Sukarno. Ideology and Politics, 1959-1965. By Rex Mortimer. (Ithaca, N.Y.: Cornell University Press, 1974. Pp. 464. \$15.00.)

Among the most remarkable developments in post-World War II Asian history, was the almost meteoric rise, out of total disarray and discredit, of the Indonesian Communist Party (PKI) in the period from the early 1950s until September 30, 1965. On that date a new

debacle overtook the party, as a result of its involvement along with dissident Indonesian military in an abortive coup attempt. The era of the PKI's present ignominy dating from this 1965 coup, has by now lasted almost as long as the time span of its rapid and spectacular rise to power in the closing years of Sukarno's presidency, and this points up the fact that in all of its more than 55-year-old history (the PKI is the oldest Communist party in Asia) the PKI's legitimacy, let alone its expansion and influence, have been the exception.

Both studies here under review address themselves to what could without exaggeration be called the PKI's first "golden age" thus far, seeking to analyze the party's role both in terms of Indonesia's domestic and foreign policies and in relation to the Sino-Soviet dispute. Dake's book, by far the superior of the two, has, however, a more predominantly international focus, and-while by no means neglecting the party's critical domestic relationships toward the Indonesian Army, the other political parties, Muslim regional dissidents, and so on-measures the PKI's influence primarily by the major turning points in Indonesian foreign policy, e.g., the effects of Khruschev's Asian policies, the West Irian question, the Malaysian confrontation, Moscow's resultant coolness, and the partnership with the People's Republic of China. Dake's research has been exhaustive on each of these issues and is altogether persuasive that the party's position, e.g., with respect to the Malaysian question, became influential if not normative for the government

Both Dake and Mortimer, although the latter to a greater degree, accentuate what might perhaps be termed the PKI's nativistic or "neotraditional" tactics and appeal, i.e., the particularly close correlation between these tactics and indigenous, non-Western political styles and social values, re-enforced by the changing character of Indonesian nationalism after the struggle for independence had been won in 1949. The essence of this PKI "nativist" policy had already been described years ago, but it is useful to have it detailed again. There is little doubt, as Mortimer demonstrates, that the PKI, seeking to become integrated in the complex balancing process of political institutions, and while never discarding its function as "modernizer" of the socio-economy, made considerable effort to adjust itself to the religious syncretism of the so-called abangan life style and political culture in Java, just as, in its foreign outlook, it seemed to drift to the activist Asian nativism projected by Peking's self-conceived role in the Third World.

Perhaps the most useful section of Dake's book is just this Indonesian Communist (and eventually officially Indonesian) orientation on People's China, and Dake's factual accounting of the Chinese readiness to assist in the arming of the PKI and its allies presumably in the context of preparation for seizure of power, is to be especially recommended to those commentators who, despite all evidence to the contrary, persist in ignoring or minimizing Peking's involvement in the 1965 coup. Dake's analysis admirably integrates the government's and the party's relationship with Peking-the latter in preparation for the coup-with the dynamics of the PKI's campaign of domestic radicalization.

One aspect of this mobilization for an eventual seizure of power, as described by Dake, however, has made his book particularly controversial. This is the role of Sukarno in the preparation of the 1965 coup. It is Dake's contention that it was Sukarno-weary of the opposition of key army commanders who were opposing his policies of close alliance with Peking, and who were aligning themselves against the general radicalizing impetus in domestic affairs (what Sukarno once called his nation's "revolutionary gymnastics"), in which the PKI played such an important role-who suggested and encouraged Lt. Col. Untung, a key officer of his own presidential guard, to "take action" against the "disloyal" army generals. Some PKI leaders, among them party chairman D. N. Aidit, who, Dake seems to suggest, had been making their own coup preparations, became aware of Sukarno's wishes and associated themselves with the schemes of Untung and other military dissidents, proceeding to the denouement of the abortive coup and their own eventual catastrophes. While some authoritative analysts of the 1965 coup are agreed that Sukarno probably had some and perhaps a good deal of information about the plans of Untung, Aidit, and their associates, convincing proof that Sukarno, even indirectly, was the coup's instigator has simply been lacking. The evidence of Sukarno's aide de camp, Bambang Widjanarko, on which Dake's argument of Sukarno's initiating role appears to rest so heavily, and which, with an introduction by Dake, was published separately a year after Dake's own book, raises the most serious questions as to its legitimacy and credibility (as I indicated in Pacific Affairs, Summer, 1975, pp. 285-87).

This lapse (surprising in a study by one, like Dake, trained as a jurist) is, however, a rarity in these pages. Where Dake's account—as for

example in the sections on the PKI's and the Sukarno regime's policies toward China—can be compared with related studies by others, In the Spirit of the Red Banteng shows itself to be an invaluable source.

Though giving a comprehensive picture of such matters as the interplay of the PKI's and Sukarno's ideologies, the party's relationship to strata changes in Indonesian rural society, the effect of domestic economic deterioration in the Sukarno years, and the catalytic effect of the Irian and Malaysian confrontation campaigns, Mortimer's study, compared to Dake's. is generally more interpretive, attempting to distill the essences of the PKI's strategy and its programs as the power struggle unfolded within the Sukarno regime. As such, Mortimer only rarely breaks new factual ground. Hence to those already familiar with the course of PKI history and tactics, his book's value will probably depend on the quality assigned to a number of Mortimer's basic value judgments about the party's operations and presumed intentions.

Unfortunately these judgments seem to depend heavily on knocking down straw men. For example, even in the preface, Mortimer seeks to break down a number of what he calls "stereotypes of Indonesian Communism," which, he asserts, arose from "a tendency on the part of many Western academics writing about Indonesia" to "denigrate" the PKI's role in Indonesian national life. One such stereotype, according to Mortimer, is to hold the view that the millions of Indonesians who at one time supported the PKI did so "only" in order to articulate their reaction to a painful process of "social change." This particular stereotype, says Mortimer, does not do justice to "any rationally conceived interests" on the part of the supporters of the PKI, nor "to the character" of the PKI itself. The present reviewer is not aware of "many Western academics" concerned with Indonesia who, in fact, would deny that those who supported the PKI did so without considering "any" of their "rationally conceived" interests." Indeed, this reviewer doubts the premise, and he would question whether in Indonesia, or anywhere else in the world, to be aware of one's subjection to difficult "social change" (not further defined) can be confidently separated from a "rational" understanding of one's interests.

Another stereotype alleged by Mortimer, is or was the fear "in the West" that a Communist Indonesia would have marked the beginning of an era of Chinese "domination" of Southeast Asia. The PKI's "nationalism" and policy independence, according to Mortimer, would

have prevented this. It should first of all be noted that the ever closer relationship between certain Indonesian and Chinese policies in the 1963 to 1965 period, and the role played by the essentially Mao-oriented PKI in that relationship, was a matter of concern not just "in the West," but, e.g., in Malaysia, Singapore, and the Philippines as well. Secondly, it should be observed in respect of the PKI's "nationalism" and its "independence" acting as a barrier to alleged Chinese "domination," that it was precisely during a period when party pronouncements laid maximum stress on the PKI's "national" and independent role that the Sino-Indonesian • partnership, and the pro-Maoist orientation of Indonesian communism was most apparent. After reading Mortimer on this point, Dake's account of the Sino-Indonesian rapprochement is a particularly welcome corrective.

Mortimer's own conceptual "stereotype" of the PKI is perhaps revealed in such observations as that it is "at least arguable" that the PKI "represented the most hopeful" prospect for economic growth and social betterment in Indonesia, and that the present Suharto era of "ruthlessness of men in power" and the "misery and powerlessness" of the masses today, should be compared with the hypothetical future of an Indonesia dominated by the PKI as revealed in the party's "ideology and actions." Given this kind of perspective, it is perhaps not surprising that Mortimer's study ends with his "preferred interpretation" of the 1965 coup in which PKI leaders appear as having been "manipulated" rather than having been among the manipulators. Of a piece, in this connection, is Mortimer's categorical assertion (in the context of a two-sentence treatment of the Chinese role) that a Chinese connection with the coup "was not established," despite reports to the contrary. This statement incidentally appears on the same page (p. 423) as Mortimer's one and only reference to Dake's book, which would have seemed to merit considerable attention considering its extensively detailed analysis of the problem of the coup's possible Chinese connection. However, instead of dealing with the question of this Chinese connection, which might perhaps have been expected of one concerned to demolish the alleged stereotype of China's dominant role in Southeast Asia, Mortimer's sole specific reference to Dake deals with the unreliability of the Widjanarko testimony, though Mortimer vaguely refers also to other "conclusions" of Dake which he is "unable to accept." Touches like these should continually prompt a reader of Mortimer's

pages to considerable caution.

JUSTUS M. VAN DER KROEF

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Den Herskende Klasse (The Ruling Class). By Jörgen S. Dich. (Borgen, 1973. Pp. 244. D.Kr. 45.)

The Scandinavian welfare state is frequently hailed by foreign admirers as a model for emulation by other Western nations. Here, it appears, is found that most elusive blend of freedom, security, and economic well-being associated with a highly developed economy. Scandinavians themselves do, by and large, share this positive appraisal of their situation; they are a patriotic lot, and generally given to the view that theirs is, indeed, a good life.

Nonetheless, recent years have seen mounting criticism from both the right and left side of the political spectrum, and charges of either too much or too little socialization. Maverick parties, such as Anders Lange's party in Norway, have cropped up on the Right, while on the Left, dissident voices demand more wholehearted commitment to socialist transformation of society.

Not the least interesting aspect of this polemical and provocative book by the Danish Professor Jörgen S. Dich, are the hardhitting punches delivered both to the Right and to the Left as he mounts a scathing attack on the form the Danish welfare state has assumed. As a Marxist, Dich proposes to undertake a "Marxist analysis of class contradictions in our day" (p. 133), and submits that a new ruling class has developed, consisting of the functionaires providing the services of the welfare state, particularly the personnel in the health, education, and welfare sector of the economy. The "servant class" has become the ruling class, and the social policy it designs reflects, as the social policy of any era will, its own class interests.

According to Professor Dich, the almost cancerous growth of the public service sector since 1960 amounts to social exploitation, for the expansion of social services has reached a point where their utility is extremely diminished, and often negative. No cost-benefit analysis of the ruling class's services is ever undertaken. On the contrary, new jobs are constantly created in response to new "needs" discovered by experts. The cost is borne by the common worker through taxation, and the beneficiaries of this social exploitation are the middle class functionaries and their offspring. The smooth functioning of the system is aided by the fact that the ideology of the ruling class

has become the social ideology. It is an ideology which conforms to strong popular instincts, rooted as it is in humanist compassion, the desire for health, and fear of death, as well as in a middle class contempt for manual work. Thus, "the working class has let itself be fooled to pay for the middle class ambitions, based in its contempt for manual labor" (p. 108).

The butt of Professor Dich's greatest wrath is the educational system, from preschool to university level. Pointing to the low proportion of working class youth among the university students, he sees the institutions of higher learning as the playgrounds of middle and upper class children.

The status symbol of the middle class is their children's career. Those whose children become workers, lose face. In no other area is it more clearly demonstrated that we live in a class society. Since there are increasingly more areas where one can get a higher education free, and since the middle class has grown simultaneously, we are confronting an educational explosion where no one thinks of what the common man has to pay for the social ambitions of the middle class (p. 101).

One of the first priorities for system reform must therefore be in the educational sector, and Dich proposes reintroduction of self-financing of higher education as a means to reduce recruitment to the ranks of academicians. Limited enrollment, with high school grades determining university acceptance, an increased student-teacher ratio ("Whether there are lectures for 30 or 300 is pedagogically indifferent. In both cases it is just one way traffic" p. 149), and reduced salaries for academicians ("those for whom the work is important will thus be attracted to the field" p. 156), are some of his proposals for pruning this sector.

While Professor Dich in his anger may have overstated his case, his book has nonetheless sharpened the attention of Scandinavians to the pressing problem of a proliferating and inefficient bureaucracy, and the possibility of the reduction of the citizen-body to the status of minors, as their lives are governed in more and more detail by experts. The value of the book lies in its marshalling of evidence that the Scandinavian welfare state may have reached the saturation point both economically and psychologically. It deserves to be read by all who profess interest in Scandinavian politics, and as it poses problems that extend beyond Scandinavia's boundaries, it may be read with benefit by those whose interest is in the welfare state as such. It deserves translation.

INGUNN NORDERVAL MEANS

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The Political Economy of Africa. Edited by Richard Harris. (New York: Halsted Press, 1975. Pp. ix + 270. \$15.95, cloth; \$6.95, paper.)

With a broad-ranging introductory chapter and five others focusing on specific African states (Ghana, Nigeria, Zambia, South Africa, and a chapter comparing Kenya and Tanzania), this volume examines relationships between economic and political factors-a key nexus being that between the African state and alien economic power. Most of the contributors are concerned with the state's role in promoting, tolerating, or attempting to overcome (1) the economy's typically heavy dependence on imports for both consumer and producer goods and on sales in foreign markets of only a few primary-product exports to finance its import bill; and (2) the usually dominant role played by foreign private enterprise in mining, manufacturing, and other sectors. Zambia, for example, has relied on one industry, copper mining, for over 90 percent of its export earnings and about 60 percent of government revenues.

The chapters on Zambia, Kenya, and South Africa give a particularly useful analysis of the penetration of alien enterprise during the colonial era (in South Africa, the continuing quasi-colonial period) and demonstrate the white-controlled state's promotional activities vis-à-vis European settlers and investors and its regressive impact on African farming and commerce. Legassick's economic interpretation of South Africa's apartheid depicts a system derived from the demands of white farmers for protection from African competition for land and markets and of mining companies for abnormally cheap labor. The state's response had two basic facets: to restrict African agriculture severely (to 13 percent of the total land) and to limit permanent African settlement in cities. This forced blacks to become migrant laborers in the white sector and, along with rigid residential segregation, prevented them from organizing effectively against white-controlled economic and police power.

In assessing the state/alien-enterprise nexus during the postindependence period, the book's major theme is that foreign economic interests continue to exercise strong influence; hence, the relationship is "neo-colonial." This plausible and important (but scarcely new) hy-

pothesis deserves the difficult effort of empirical testing through comparative decisionmaking studies. The volume under review does not take that needed approach, but in its largely speculative analyses it does provide some promising subhypotheses. One (developed in the chapters on Nigeria and Kenya) is that alien influence is exercised through local political elites who find their pocketbooks and/or career-survival interests served by a status quo of smoothly functioning established foreign enterprises and by continued new direct investment which yields them "commissions," employment opportunities for their supporters, tax revenues to fund social services, and other benefits. Another (from the Ghana chapter) is that foreign-owned extractive enterprises use their current and potential export-earning capacity to bargain successfully for favored treatment-especially during balance of payments difficulties for the host country.

A third, perhaps unintended subhypothesis is that foreign private capital's influence has not been decisive, if even present, in postindependence conflicts over regime or government change. The four chapters on independent black-ruled states offer no evidence of significant investor involvement-direct or through their home governments or alleged local alliesin the two coups and intervening election in Ghana, the prolonged regime instability in Nigeria, or the threats to the survival of one-party governments in Kenya, Tanzania, and Zambia. As these same chapters indicate, most of the conflicts have been due to ethnic and/or regional rivalries, with little economic policy differentiation to tempt foreign investors to risk intervention on one side or the other.

A fourth interesting subhypothesis is that foreign enterprise has importantly influenced postindependence politics through its unwilling role as a "pork barrel." Under pressure to provide tangible benefits from independence, political elites in Kenya and Zambia, among other countries, could satisfy many followers by simply redistributing land, shops, salaried jobs, and other existing economic opportunities from aliens to nationals. Where large foreignmerchant and landowning groups did not exist, elites had the much more demanding task of engineering new opportunities.

The book's first four chapters argue that African states would benefit from ending or severely restricting foreign private investment and from reorienting their economies from producing for export to meeting internal needs. We are told that Africa must "disengage... from international capitalism" (p. 40) and that Tanzania should work toward "developing its

own productive capacity and limiting its involvement in the international economy" (p. 185). However, given its limited market and raw materials endowment can the typical African economy realistically be expected to produce much of its manufactured producer and consumer needs? And can it afford to neglect its export sector while groping toward a feasible degree of self-reliance? Ghana under Nkrumah attempted an "economic independence" strategy but clearly flopped at it. Import-substitution enterprises established by the government in manufacturing and farming tended to be unproductive because of poor design and/or wasteful management. Neglect of export industries contributed to stagnation in foreign exchange earnings and the country's near international bankruptcy at the time of the 1966 coup against Nkrumah (see "L'experience du Ghana," L'afrique de l'indépendance politique à l'indépendance économique [Paris: Maspéro,

Tanzania has also embarked on a strategy of "self-reliance." Since 1967 the state has taken over virtually all significant foreign enterprises and has attempted to increase production for domestic demand by, among other means, massive resettling of peasants onto cooperative farms. However, the Tanzania chapter in this book does not tell us whether these policies have been successful. Are the nationalized enterprises producing surpluses for reinvestment, or are they, through mismanagement, eating up state revenues? Have the cooperative farms significantly increased production, or have outputs dropped because of peasant opposition to forced resettlement?

To be persuasive, the policy recommendations of this volume need to be supported by more analysis of actual outcomes: for economic growth and income distribution, for political support generated and lost. The subject—which strategy to follow in coping with economic dependence—is important enough for in-depth, comparative studies. This volume makes a helpful start in posing issues and generating hypotheses.

JOHN D. ESSEKS

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Trade Union Foreign Policy: A Study of British and American Trade Union Activities in Jamaica. By Jeffrey Harrod. (Garden City, N.Y.: Doubleday, 1973. Pp. 485. \$3.95, paper.)

In recent years, wide attention has been given to the foreign activities of America's

gigantic multinational corporations. Contrarily, very little has been written concerning the international role played by our powerful trade unions. Jeffrey Harrod's study partially fills this void by focusing attention on certain foreign activities of the United Steelworkers of America. As an illustrative case study, the author gives special consideration to union activities in the Caribbean nation of Jamaica between 1938 and 1967. The American trade union activities in Jamaica are compared and contrasted with those of the major British union, the Trades Union Congress, during the same period.

Over half of Harrod's book consists of "background" chapters which the author considers necessary for understanding his specific Caribbean case study. These include: a theoretical setting; a general study of the international activities of trade unions; an analysis of the functions and structures of American and British trade unions with special emphasis on their policies toward less developed countries; and sections on the geographical, historical, economic, cultural, political, and trade union background of Jamaica.

The effects of the world-wide economic depression of the 1930s were particularly disastrous in Jamaica. Out of conditions of unusual economic adversity emerged the first mass political parties, the first broadly based trade unions, and the first charismatic local leaders. By the time the first legislative elections were held under universal suffrage in 1944, two vigorous political parties were able to present candidates for voter consideration. One, the People's National Party headed by Rhodes scholar Norman Manley, was closely associated with the Trades Union Congress of Jamaica. The second, the Jamaica Labour Party headed by popular Alexander Bustamante, was indistinguishable in organization and membership from the Bustamante Industrial Trade Union. For over 30 years, these two parties and their affiliated trade unions have provided Jamaica with competitive political and trade union systems. Though close ties between political parties and trade unions are not unusual in democratic nations, competition between two such dual organizations is an unusual characteristic of the Jamaican scene,

In order properly to understand political and labor conditions in Jamaica, Harrod made an extensive investigation of the cultural background of the local people. He found that about 80 percent of the population belonged to what he termed an "evolved culture," which was based on the commonly shared ancestoral experience of slavery. The other 20 percent, he found, belonged to an "imitative culture,"

based on attempts to copy ideas, styles, and institutions from Britain. Alexander Bustamante, founder and leader of Jamaica's first mass trade union, appealed mainly to the "evolved culture" group. He provided a highly personalized type of leadership for his organizations. Norman Manley's party and affiliated trade union, contrarily, appealed mainly to the "initative culture" group, and copied both policies and organizational concepts directly from British practice. When first British, and then American, unions became interested in Jamaica, they naturally found themselves most comfortable dealing with the kind of organizations with which they were already familiar. For this reason, the Jamaican unions associated with the minority "imitative culture" received the almost exclusive interest and attention of the foreign unions.

The major cause for American trade union interest in Jamaica came as an outgrowth of the discovery of extensive bauxite deposits on the island in the early 1940s. Within a decade of discovery, large American and Canadian corporations were extensively involved in the exploitation of the deposits, thought to be the most extensive in the world. Jamaica was soon the major source of raw material for the North American aluminum smelting industry. Since most American and Canadian aluminum workers are members of the United Steelworkers of America, this union took an active interest in union conditions in the Jamaican bauxite industry. In order to protect the source of needed raw materials, the American union wanted a stable trade union situation in Jamaica. And through American eyes, such stability would only be possible if the Jamaican trade union associated with the "imitative culture" was successful. Thus American involvement was one-sided, and involved either ignoring, or in some cases outright opposition to, Bustamante's union and party organizations.

Though the foreign unions involved in Jamaica often used high-sounding rhetoric to defend their actions, such as favoring union democracy, opposing Communism, and supporting improvement in working conditions, Harrod's investigations found "self-interest" the motivating force for their activities. Instead of the more typical North American adversary relationship of unions and management, he found that the foreign unions closely cooperated with the foreign aluminum corporations in their respective Jamaican activities. Both shared the common goals of wanting a safe investment climate in Jamaica and desiring unimpeded access to the bauxite ore. If the case study presented by Jeffrey Harrod is typical,

one may conclude that motives of pure humanitarianism are no more likely to be found in the foreign activities of trade unions than in the foreign activities of multinational corporations.

CHARLES KUNSMAN, JR.

San Jose State University

What Government Does. Edited by Matthew Holden, Jr., and Dennis L. Dresang. (Beverly Hills, Calif.: Sage Publications, 1975. Pp. 320. \$17.50, cloth. \$7.50, paper.)

Political escience, according to the introduction in What Government Does, has turned away from its early interest in public policy to study other questions. Believing public policy to be an essential aspect of political science, the editors offer this volume of 12 previously unpublished papers to remedy the discipline's inattention to public policy. The volume's major shortcoming is its lack of structure-the only common theme the editors admit is that all selections deal with "what government does." Given the objective of emphasizing the study of public policy in general rather than a more narrow theoretical or empirical objective, the reader is left awash in a potpourri of studies, unsure why all of them are important. Despite the editors' disclaimer and the tremendous variety in substance, method, framework, time period, and national context, some common threads do emerge from the book. The selections may be categorized by their emphasis on the determinants of public policy, the impact of public policy, the prescription of public policy, the impact of public policy, and the methodological issues in policy studies.

The best contributions are those that analyze the determinants of public policy. Robert Russell's paper on international monetary reform is clearly the class of the volume. Russell outlines three theories of international politics-power politics, class struggle, and transnational elites-and through exhaustive analysis makes and tests dozens of hypotheses suggested by the three theories about international monetary reform. In the only example of the Dye-Sharkansky-Hofferbert school of state policy analysis, David Brady and Richard Murray examine the relationship between malapportionment and public policy. Unlike most other studies they find that changes in malapportionment do affect social welfare policies. The other three selections concerning the determinants of public policy are less sophisticated. Thomas Wolanin and Lawrence Gladieux contend political culture affects U.S. higher education policy, a conclusion marred only by their operational definition of culture as past policy decisions. Lawrence Pierce argues Nixon's decision to use wage and price controls was affected more by political than economic concerns, and Edmund Keller presents an interesting case study of mass participation determining educational policy in Kenya.

Only one paper has its primary stress on the impact of public policy outputs on policy outcomes, Hugh Heclo's study of social welfare policies in England and Sweden. Heclo's analysis reveals, as most students of public policy suspect, that well-designed public policy may not have the impact intended. Jerome Stephen's prescriptive essay on the rights of medical research subjects is the only direct attempt to influence public policy. Stephens uses a journalistic style to show how medical research subjects are inadequately protected. The low point of the volume is two descriptive selections. Craig Worthington attempts to present a framework for analyzing governmental arts policies but ends with two unsatisfying case studies. George Simwinga's chapter on Zambian copper nationalization is based on other analyses with little contribution by the author.

The methodological selections are informative but not major breakthroughs in the study of public policy. Dean Mann rightly criticizes Theodore Lowi's policy typology (distributive, redistributive, regulatory, and constituent) for not being mutually exclusive. Jack Brand underscores the obvious, that social indicators have serious validity problems. Ending the volume with Anthony King's analysis of the role that political scientists can play in policy analysis is especially troublesome. King competently argues that political scientists can play only a minor role in evaluating public policy, a peculiar position to end a book hoping to encourage political scientists to examine public policy.

Because the book's contents are so diverse, it is guaranteed some uses. The volume provides a snapshot of public policy research in the early 1970s; the short time span, however, limits the quality of the articles and renders the volume unsuitable as a class reader. Students interested in the substantive concerns of individual chapters should read those chapters, but only the most rabid student of public policy will read the entire volume.

KENNETH JOHN MEIER

Rice University



The United Kingdom in 1980: The Hudson Report. By The Hudson Institute Europe. (New York: Halsted Press, 1974. Pp. 126. Price not listed.)

This little book represents the Hudson Institute's diagnosis of, and prescription for, what's wrong with Britain (despite the title the United Kingdom dimension is largely absent). The main theme is that Britain is suffering from a serious and unique form of economic sickness. "It is a failure to grow industrially and economically at the same rate at which other neighbouring countries are growing" (p. 3). The result is an "unstable and socially divided nation, economically depressed" (p. 6), with a standard of living and political conditions inferior to other European states. The suggestion is that by 1980 Britain's position will resemble the slow death of seventeenth-century Spain. Three principal arguments are employed to support this theme. First, some specific weaknesses of the economy are identified and examined. These range from low investment rates leading to technical retardation to poor foreign trade performance, a constantly deteriorating balance of payments, and a declining standard of living. Secondly, some popular "myths of salvation"-the Atlantic alliance, European Community membership, and North Sea Oil-are discussed and rejected on the grounds that they represent "a series of major evasions of reality" (p. 70). Thirdly, two policy areas-education (particularly higher education) and the management of regional resources-are singled out for special treatment to illustrate the general point that successive British governments have wasted major assets.

What is to be done to remedy this mess? The Hudson Institute suggests that "Britain once and for all break with its Victorian past, and renew in these closing years of the late twentieth century the process of social and psychological evolution that has been in abeyance for nearly a century" (p. 113). More briefly "a shift in Britain's national style" is required. In policy terms this involves, first of all, a six-year national development plan managed by a planning commission composed of "Britain's best economists and administrators and engineers, all serving the nation in a self-conscious spirit of ambition (for the country) and enterprise" (p. 115). In addition, the Institute suggests a National Investment and Development Corporapractical effect" yet "weighed with symbolic importance" (p. 123).

Evaluating all this is a difficult exercise. On the one hand the report, first published in late 1974, was written primarily to stimulate public debate. As a result it has to be viewed more as a piece of quality journalism than a weighty academic study. On the other hand, the Hudson Institute clearly regards itself as a worthy organization with views which should be taken seriously by academics. If this is the game then the exercise as a whole can be criticized on three counts. First, the analysis is characterized by a kind of inverted Almond and Verba-ry. In the past, foreign commentators on British politics and society were too ready to emphasize the good points in the system. The Hudson Institute goes to the other extreme. Other countries, particularly France (and even Italy), it sees as operating in a fundamentally different and superior league to Britain. Few would deny that there is some sort of crisis in Britain. Whether other countries will avoid similar tribulations in the future is open to debate. Britain, not France, may be the developmental model for Western Europe. The Institute ignores this problem. Secondly, there is a credibility gap between the perceived nature of the crisis and the solutions offered. To fight a tiger of a crisis the Institute has put up a few mice. Moreover, the reform proposals are marked by a constant desire to separate economic management from politics, something which, in the present British context, is next to impossible. Finally, it may be that in emphasizing the economic nature of the problem the Institute has mistaken a symptom for the crisis itself. The argument that the British sickness is largely political is never seriously considered. However, it is generally accepted that a variety of possible solutions to our economic problems exist-what is lacking is the political will and capacity to carry them out. The absence of a serious political dimension to the report's analysis is its major weakness. Fortunately other, and less expensive, studies are beginning to fill this gap.

JIM BULPITT

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Metropolitan Growth: Public Policy for South and Southeast Asia. Edited by Leo Jakobson and Ved Prakash (New York: Halsted Press_

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policy oriented, with this later volume concentrating on management and planning for urban growth. In the short introduction by the editors, the theme of the first essay by Arch Dotson and Henry Teune is endorsed for the book as a whole, supposedly providing a basis for integrating the diverse contributions to the volume. The Dotson and Teune paper, "On the Consequences of Urbanization: Contributions to Administrative Capacity and Development," employs statistical analysis of aggregate data to support the authors' thesis that urbanization contributes to development, and that higher levels of urbanization produce higher levels of development on a wide range of indicators, such as administrative capacity, health, education, industrialization, and communications, among others.

If this is, indeed, the main theme of the book, one would expect the remainder of the contributions to propose policies designed to promote urbanization and a pattern of development modelled after the more advanced industrial countries of the West. While a few contributors make tacit acknowledgment that urbanization may contribute to national development, greater attention is given to counter themes. Several essays, including the final contribution by the editors, emphasize the necessity of retarding the pace of urbanization, accommodating nondeveloped sectors of the economy and preserving the symbiotic relationship between modern and traditional social structures and economic systems.

The contrast between the initial theme and the subsequent themes in this volume points up a critical issue never directly faced by any contributor, but nonetheless lying at the center of the volume. Is the South and Southeast Asian city substantially different from cities elsewhere in the world, such that it requires unique policies and plans to solve its problems? Most of the contributors appear to be answering this question in the affirmative. In the last six papers, which concentrate on Pakistani and Indian experiences with urban planning, the failures of Western-oriented planning and urban development schemes are highlighted. Time and again the message seems to be that policy must fit resources and be adapted to administrative and government capabilities. Because the South and Southeast Asian city, with a few exceptions, has massive overcrowding, overtaxed services, slums and bustees, a dreadfully inadequate tax and revenue base, low administrative capability, and a complex mix of traditional and modern cultures and economic systems, the process of urbanization poses problems which are insoluble by reference to Western models.

We learn that slums must be preserved, but made marginally livable, that paralyzed urban services must be incrementally improved, and that priorities should focus on minimal criteria for urban services and infrastructure investments. In short, objectives and priorities must be scaled down, planning should be sensitive to cultural factors, and should be cumulative and flexible, but also long-range and coordinated.

The dilemmas of the cities of South and Southeast Asia are highlighted by the fact that the most modern city in the area is not chosen as an appropriate model for other cities to emulate. There are two contributions on Singapore. The first, by Richard L. Meier, discusses Singapore and Bangkok as "pacemaker" cities for the areas they serve, and presents, in an impressionistic fashion, some evaluations on the positive influences of multinational corporations and the modern business community, along with views on the importance of English as a medium for the transmission of aspects of modern technological civilization. In the second paper on Singapore Peter A. Bush explores the problems of legitimacy and national allegiance in that multiethnic society. Neither paper attempts to generalize for the urban planners of the less developed cities of Southern Asia.

The major contributions of the book are those which recount and analyze the urban planning experiences and strategies of the major cities of South Asia. Among those deserving special mention are the articles by Khalid Shibli on "Metropolitan Planning in Karachi" and by Arthur T. Row, "Metropolitan Problems and Prospects: A Study of Calcutta," along with the final three papers written by the editors. Rather sophisticated and abstract heuristic models are utilized in several of the papers, including one devoted to a complex calculus for evaluation of alternative criteria for investment in water supplies, and two excellent papers which discuss the complex issues associated with formulating appropriate housing and urban development policies. Typological models of the urban economy, the social system, and the budget resource allocation system are utilized to illustrate the complex interrelationship of factors which must be taken into account in urban planning.

Unfortunately, the theory and models utilized in the volume are not always related to the practical choice situations of the Asian urban planner. While there is frequent mention of administrative capabilities as a factor to be considered, corruption is ignored as a major impediment to urban planning strategies. Furthermore, nearly all contributors seem to ignore politics and the process of public participation

in plan formation and implementation, apparently assuming that such considerations are irrelevant. For most states in Southern Asia, urban planning must first come to terms with both corruption and politics.

This book will, no doubt, be read with interest by expatriate urban planners who have assisted the major cities of the area with their urban planning problems. However, one cannot help but wonder whether there is an audience for this volume among the indigenous urban civil servants of the medium-sized cities of the region which suffer from unplanned urban growth and have no such expatriate advisory staff. It is more likely that this volume will be utilized as a source book for those urban studies programs in North America which seek to become more cosmopolitan in scope. For such purposes, it is an excellent collection and a most welcome addition to the expanding collection of works devoted to urban problems in Southern Asia.

GORDON P. MEANS

McMaster University

The Devious Dalang: Sukarno and the So-Called Untung-Putsch. Eyewitness report by Bambang S. Widjanarko. Translated and edited by Rahadi Karni and Antonie Dake. (The Hague: Interdoc Publishing House, 1974. Pp. 211. Guilders 17.)

Any documentary material that could shed new light on the controversial question of the true authorship of the October 1, 1965 "coup" in Indonesia, which led to the downfall of President Sukarno, the physical destruction of the huge Indonesian Communist Party, and the installation of the Suharto military regime, is prima facie of real importance. This book claims to provide the "verbatim testimony" of one Colonel Bambang S. Widjanarko, a longtime adjutant to Sukarno, in response to questions posed by the Central Interrogation Team of the Operational Command for the Restoration of Security and Order on the subject of the "coup" and its antecedents. An Indonesian-language text is given, along with an English translation and brief introductions by Dake and Karni.

The colonel's testimony had been referred to earlier by Dake in his book In the Spirit of the Red Banteng (The Hague: Mouton, 1973), and formed part of the basis for his theory that the real author of the "coup" was Sukarno himself in a complex intrigue to get rid of anticommunist generals who stubbornly blocked the president's leftward course in domestic and foreign

affiars. Both Dake's thesis and the evidence for it came under sharp criticism, and the publication under review here is clearly intended to convince the skeptics. Unfortunately, it is likely to have just the opposite effect.

In the first place, there is no way to determine whether the Indonesian text is authentic or has been tampered with by the Indonesian military (who have a long tradition in this regard). The testimony was given in camera and to this day has not been published in Indonesia. Though the colonel did testify at two show-trials of "coup" participants, and the transcripts of both have long been available to scholars, the editors have not seen fit to include Widjanarko's public testimony for comparison and cross-checking. (Furthermore, their identification of the defendants at these two trials as Air Marshal Omar Dhani and General Pranoto is disquieting, since the latter has never been tried!)

Secondly, Dake asserts that the colonel was "for many years close to President Sukarno as no-one else," to stress the central importance of his testimony. As any serious student of Indonesian politics knows, this is plain nonsense. The present text itself deflates such claims. About 88 of the 120 questions put to the colonel deal with matters that he would have known about first-hand if he had really been Sukarno's intimate confident; yet more than half of his answers are based on hearsay. His testimony as a whole gives a characteristic picture of a minor palace functionary hanging about outside closed doors hoping to pick up scraps of high-level gossip. (In addition, less than two-thirds of all his answers have anything at all to do with the "coup" or its background!)

Thirdly, the interrogations were carried out in October-November 1970, over five years after the coup. The dating (and the testimony's rapid arrival in Dake's hands) suggests that its production relates less to the events of 1965 than to the internal military politics of 1970, possibly to efforts by the declining Nasution faction to hit back at the winning Shuarto group by intimating a cover-up of Sukarno's true role in the "coup."

But aside from the equivocal status of the text, the largely heresay nature of the evidence it presents, and the political context in which it was prepared, there are also two major technical drawbacks to this book. In the first place, there are no footnotes of any kind to guide the reader through the maze of personalities, places, organizations, and policies mentioned. Only the handful of scholars specializing in the intricacies of Indonesian palace politics under

the late "Guided Democracy" can be expected to make head or tail of the information presented or to judge its accuracy and relevance. So many names of persons and organizations are misspelled or simply incorrect historically, that one's confidence in the editors' competence in this field is undermined.

The second drawback is the poor quality of the translation. In some instances the errors may be due to carelessness or ignorance of contemporary colloquial speech in Indonesia. But in other cases, one gets the distinct feeling that the mistranslations are designed to buttress claims that Sukarno authored the "coup" or to put the military interrogators in a good light. Space permits me to offer only one example of each: (1) Sukarno is repeatedly said to have urged people to menjingkirkan the anticommunist generals. This word normally means "set aside," "remove from office," but is always translated here as "eliminate" (murder implied). (2) Virtually every question the interrogators pose is introduced in the translation with a gentlemanly "please"-a word completely absent from the brusque Indonesian original. In sum, it is hard to recommend this book except to confirmed "coup" buffs.

BENEDICT R. ANDERSON

Cornell University

Educational Policy-Making: A Study of Interest Groups and Parliament. By Maurice Kogan. (Hamden, Conn.: Linnet Books, 1975. Pp. 262. \$15.00.)

Education has excited perhaps more academic interest than any other area of public policy, and Professor Kogan has added another useful study to the existing literature. This study examines educational policy making for England and Wales between 1960 and 1974. The approach is essentially historical and institutional, although some attention is given to sociological and political science concepts as a means of understanding the historical data.

One of the most interesting aspects of this book is the description and analysis of educational policies and objectives. The 35 policies identified by the author were classified as educational, social, institutional, or economic. In addition, the main events in the formulation of the policy and the actors involved were also catalogued. Although the policies discussed rarely fell neatly into one category or another, this classification does help one understand the changes and pressures for change that have occurred in education. It is particularly in-

teresting to note the continuity of several issues in education—teacher supply planning, comprehensivization, and institutional responses to unionization—which are of current importance. In fact, even two years after the end of the author's time period, the only major issue which would have to be added to his list would be the institutional response to expenditure cuts.

The listing of policies and objectives provides an account of the issues in education, while four chapters on interest groups and one chapter on Parliament provide an understanding of the actors involved in making policy. The listing and description of interest groups is especially thorough, with a better analysis of the role of local authority associations in education than is found elsewhere. Likewise, the discussion of teachers' groups goes substantially beyond the mention of the role of the NUT which is usually encountered. Finally, the discussion of the number of new or tangential groups involved in education policy making is a valuable addition to the mapping of actors in education.

Despite the importance of the script (the issues) and the dramatis personae to the play, they do not in themselves constitute the play. There is still a need for action, and we unfortunately learn little about the action of policy making from Professor Kogan's book. We do have some insight into the processes of policy making and the interplay of actors from two brief case studies: one on the universities and one on comprehensive schools. However, even these appear oriented more toward laying the groundwork for analysis rather than actually examining how policy has been made. This is perhaps especially disappointing given the numerous interviews conducted by the author, his unique knowledge of the subject matter, and the quality of his previous studies of educational policy making. One can only hope that this will be the prelude to a more complete and exhaustive study of the interactions of the several groups and institutions in the formulation of policy.

Perhaps most disappointing in this regard is the failure to deal more directly with the role of the Department of Education and Science (DES) in making policy. Admittedly, there are difficulties in penetrating a department of British government, but some recent studies—most notably that of Heclo and Wildavsky—have shown that it can be done. While Professor Kogan has defined his study as one of interest groups and Parliament, this rather arbitrary restriction seems to exclude much of what is important in modern policy making. The re-

striction seems even more arbitrary as it is pointed out that several of the interest groups react more to DES policy that initiate policy on their own. Even those which seek to initiate policy would appear to direct more of their efforts toward DES than toward Parliament. Thus, it seems that the exclusion of DES from more direct and intensive analysis limits the usefulness of this work in understanding the formulation of educational policy in England.

While this is a valuable and thorough study of the role of educational interest groups and of the issues which have been important in education during the postwar period in Britain, its usefulness as a description of educational policy making is limited by the concentration of the groups involved rather than their actions, and by the exclusion of perhaps the crucial actor in the policy process.

B. GUY PETERS

University of Delaware

Community and Change in European Society: France, Germany, and Italy since 1870. By Martin Kolinsky. (New York: St. Martin's Press, 1974. Pp. 234. \$16.95.)

This is a brief but intelligent overview of political and social development in France, Germany, and Italy since 1870. The book is divided into three parts. Part I deals with the impact of industrialization in these countries in the years between 1870 and the outbreak of World War I-with "the formation of the modern structures and political cultures of Italy, Germany, and France." The years between the World Wars are covered in Part II, which is concerned primarily with the rise of Fascism in Italy and Germany, with the fascist reorganization of Italian and German society, and with the contrasting (although parallel in some fashion) situation in France. Part III looks at the three countries since 1945 and at the development of the European communities.

Kolinsky, who is a member of the Department of Politics at the University of Birmingham (England), did his doctoral thesis at L.S.E. on French social structure, and this volume illustrates his continuing interest in the relationship between social change and political development. In each period, Kolinsky focuses on the social bases of politics in the three countries; in particular, on the relationship between major social groups—labor and business—and the political process.

This basic approach, rather than an overarching theoretical framework, provides the book's

unity. The author does not attempt to open new theoretical frontiers; he touches instead, upon the work of a number of well-known social scientists: Barrington Moore, Ralf Dahrendorf, William Kornhauser, and Talcott Parsons. He introduces basic concepts developed by these authors and attempts to illustrate their use in analyzing social and political development. There is no original research in the book; Kolinsky relies entirely upon secondary sources.

But this should not be taken as a criticism. Criticism should be related to intent, and the author has clearly not designed this book to be a major theoretical contribution, nor does he suggest that it rests upon original research. Thus, it neither is, nor does it pretend to be, a work like the Tillies' recent study of The Rebellious Century, 1830-1930 or Charles Maier's superb volume on Recasting Bourgeois Europe, both of which also appeared in 1975.

Kolinsky's book instead is a highly useful text which would serve well for background reading in a Western European government and politics course. In this light, rather than seen as a major research contribution, it is to be commended as a clearly written history which introduces certain basic social science concepts within an overall framework stressing the interaction of society and politics. For students who take courses in Western European government, particularly—but alas not only—at the introductory level, with little or no background in contemporary European history, this would be a valuable additional text.

Even at this level, the book is not ideal. There is a distinct lack of comparative analysis among the three countries—except when Kolinsky focuses on the social bases of Fascism. In all, given the sweep of events between 1870 and 1970, even in these countries, one might feel that the author devotes too much of the book to his analysis of Fascism. Finally, for what was obviously intended as an introductory text, the bibliography is oddly brief. The book would be more useful if the author had devoted more attention to this.

But my major criticism is not directed at Kolinsky. He has written a book which, if not perfect, still could meet an increasingly important need. As I have attempted to document in a brief volume on Western European Studies in the United States, the Western European content of undergraduate education in American colleges and universities is diminishing rapidly. For good reasons or for ill, most of the requirements which formerly structured undergraduate programs and have now been eliminated on many campuses were courses

with Western European content: "world (i.e., Western) history and civilization," required language courses, and so on. The international content of U.S. undergraduate education unfortunately has not, it appears, increased, but it has changed in character from Western European to "World." I don't mean to argue with this trend, or to suggest it should be reversed. What it means, however, is that those of us with interests in Western Europe can no longer take it for granted that students will acquire a basic background on Western European history and culture just through the normal process of their undergraduate education. Western Europe is increasingly an exotic and esoteric world area to most students, in terms of hard facts and basic academic familiarity.

Kolinsky's book can help fill this need. It could serve as a background text on modern European history in many of our courses for students who have not yet been exposed to these materials. It might, if it weren't a \$16.95 hardback. This is not a volume that research scholars will purchase for their libraries. It isn't designed to be. But it is one that might be used widely in college courses—if it didn't cost almost \$17.00.

STEPHEN BLANK

The Conference Board, Philadelphia

Montaillou, Village Occitan de 1294 à 1324. By Emmanuel LeRoy Ladurie. (Paris, Gallimard [Bibliothèque des Histoires], 1975. Pp. 642.)

It's an ill wind that blows no good. Having picked up LeRoy Ladurie's monograph during a trip to Paris, the chances of reading it were slim until convalescence from pneumonia enforced the necessary leisure. Hopefully, others will not require such compulsion to read *Montaillou*, Village Occitan.

My conclusion is best stated at the outset: this is a very important book. Paradoxically, it is valuable for an understanding of the Western tradition precisely because it is devoted to a single village. Although focused on 30 years (1294 to 1324), this study radically alters one's view of the entire medieval period.

For political scientists who are concerned with socioeconomic development, as well as for theorists who wish to judge social theories ranging from Marx to Aristotle, *Montaillou* is fascinating and rewarding. If you read French, you will find all 625 pages well worth the time; if not, keep an eye out for a translation.

Why this strong endorsement? What is the

general significance of Montaillou, a village of only 200 to 250 inhabitants on the French slopes of the Pyrenees? And how can an ethnological "village study" be done on a community over 650 years in the past?

The answers are closely related to the efforts of the Church to suppress the Albigensian heresy in Languedoc. The Bishop of Pamiersan energetic prelate named Jacques Fournier, later to become Pope Benoit XII-received permission to conduct his own Inquisition from 1318 to 1326; a precise prosecutor and "maniac for detail," his scribes faithfully recorded his careful cross-examination of hundreds of suspects and witnesses. In his diocese. Montaillou was clearly the community most seriously infected with Catharism: in 1308, all its inhabitants had been temporarily incarcerated by the Inquisition of Carcassonne; of the 114 people brought before Bishop Fournier's Inquisition between 1318 and 1326, no less than 25 came from this tiny village.

The copious notes of the Bishop's Register provide LeRoy Ladurie's source. From his analysis, we learn routine and intimate details: what Montaillonais ate, which gestures and swear words they used, how their houses were built, who seduced whom, and what people did before Western technology gave us the flush toilet... We also see the social structure of the village, based on the household (domus in Latin, ostis in Occitan)—a unit which was simultaneously the physical house and the nuclear family, often augmented by one or both sets of grandparents and one or two servants.

The first of the lessons of Montaillou—and it is probably long overdue—is that the classic image of medieval life did not apply everywhere. In this corner of the Pyrenees, Lord and manor, Chateau and Seigneur were not oppressive nor even central to daily life. Second, and closely related, is the irrelevance of simplistic Marxist categories of social class, ideology, or mode of production, which have all too often dominated discussions of Western history. Third, and no less important, LeRoy Ladurie shows that Montaillou cannot be described in terms of Tonnies' Gemeinschaft or "organic community," despite the tendency to apply this concept to all nonmarket or premodern societies.

Religious issues, of course, play a central role in early fourteenth-century Montaillou: the two main axes of life seem to be the *domus* (on earth) and salvation (in heaven). The appeal of Catharism derived primarily from concern with the soul and its fate after death—though the

heresy was reinforced by opposition to taxes imposed by the Church. But in Montaillou, the cleavage between Albigensians and orthodox Christians was more than theological since it generally coincided with the division of the village into two main "cliques" or alliances of households.

It is here that students of comparative political processes will find Montaillou most fascinating. The book treats patron-client relations—with, as patron, a village cure who adopted heretical views and seduced the widow of the local seigneur, family alliances by marriage or friendship, vendettas, and even an outright assassination. Those who have studied the dynamics of traditional societies, whether in the field or in the anthropological literature, will find themselves on familiar ground when entering medieval Montaillou with LeRoy Ladurie as guide.

Many further delights await the reader. The personalities of the village are sketched in rich detail, notably the libidinous curé, Pierre Clerque, and a delightful shepherd named Pierre Maury. The relations of men and women, the content of local folklore, and the contrast of village life with that of the shepherds in transhumance: there is more than we are accustomed to find in a single volume.

Each reader will, of course, respond to different parts of such a complex ethnographic report. This reviewer found the evidence of a persisting "mediterranean" culture particularly striking, not least because it apparently confirms Aristotle's economic theory (see p. 617).

Indeed, when enjoying LeRoy Ladurie's monumental work, one *almost* forgets the origins of our information about Montaillou. Many of the villagers were condemned to wear yellow crosses as a sign of their heresy; some perished at the stake; more were imprisoned, and their property seized, and homes destroyed.

It may well be that, as the saying goes, "it's an ill wind..."; but I cannot help feeling a profound bond of sympathy for the men and women whose lives we can perceive only because Bishop Fournier set out to eradicate the Albigensian heresy in Montaillou.

ROGER D. MASTERS

Dartmouth College

Power and Privilege: Influence and Decision-Making in Pakistan. By Robert LaPorte, Jr. (Berkeley, Calif.: University of California Press, 1975. Pp. xii + 225. \$13.75.) This small book of essentially 183 pages deals with political power in Pakistan from 1947 to 1973. It is based on a review of the pertinent literature and on interviews with approximately 80 American nationals who have had experience in Pakistan. The interviews solicited responses to 78 questions from persons presumed to be knowledgeable about the politics or economics of Pakistan and especially about elite groups, influentials, influence structure, and decision making. Fifty-seven of the 80 interviewees were U.S. officials and 23 were private sector and university or foundation officials.

The result is rather hard going for persons unfamiliar with Pakistan; the author seems to assume some previous background—necessarily so given the time span covered and the length of the book. Yet there is not much in the book that the Pakistan specialist will not already know. However, the book is useful for those readers who fall between these two groups. Although no individual or institution in Pakistan is treated in depth, the reader obtains a perspective on the influential groups in the country and the shifts that have taken place in the last decade.

The first two of the book's eight chapters deal with concepts, research problems, and a general history of Pakistan prior to 1971. The next four chapters focus on elites and decision making during specific time periods. The Pre-Ayub period (1947–1958) is described basically as bureaucratic rule behind a parliamentary facade. Ayub's "modernizing" rule is viewed primarily as an attempt to maintain civil and military support and to a lesser extent support of nongovernment influentials such as important industrialists. The Yahya period is characterized as a junta rule so isolated from political reality that civil war and the separation of East Pakistan was hastened.

The most interesting and longest chapter opens with a good summation of the situation as of December 1971 and continues into 1973. These years saw a decline in the influence of the Civil Service of Pakistan (CSP). Formerly the most powerful and prestigious segment of Pakistan's civil service system, it has been disestablished as a distinct and separate class. Other groups also lost influence: the higher military officers and the leading industrial families in particular. The middle classes rose in influence as they participated in national and provincial politics. Two groups-the peasants and industrial labor-failed to make appreciable gains as Bhutto did not deliver on his electioneering promises. The landless peasant "is still waiting for his twelve acres" (p. 139). Students supported Bhutto but do not seem to have gained influence by doing so.

LaPorte's seventh chapter is quite different. Here he uses his interview results to consider the influence of the United States on decision making in Pakistan-concluding that there has been an influence "but not a dominance" (p. 147). This chapter is primarily an attempt to tap the opinions of Americans who have worked in significant posts in Pakistan. However, Appendix B indicates, among other things, that these Americans had surprisingly little contact with labor unions, students, landlords, and peasants (p. 201). One gets the impression that the author was interviewing American bureaucrats whose contacts-almost exclusively-were with Pakistani bureaucrats in urban areas.

This must have been a frustrating book to write. Most of the written materials on Pakistan predated 1971. The years of 1970 to 1973 were years of rapid change, and at this point it is extremely difficult to determine which changes were basic and important and which temporary. The author may have attempted the impossible; he must be commended for his courage. His subject is an important one; given all the methodological difficulties—which he admits to in a forthright fashion—he has done very well. There is something of interest here for any social scientist who has a real concern for Pakistan.

HENRY F. GOODNOW

University of Colorado

Psychological Challenges to Modernization: Emotional Factors in International Development. By Albert Lauterbach. (Amsterdam and New York: Elsevier Scientific Publishing Co., 1974. Pp. 190. \$10.75.)

The core of this book is a challenge to our traditional views of development and ways of looking at what is going on in the modernizing nations. Lauterbach argues that a developing nation is not simply remaking itself (or being remade) in the image of nineteenth-century Britain, or the U.S.A., or Russia, or even China. In fact most of our familiar and cherished categories for talking about economic systems (such as private versus public ownership) are held to be inapplicable and irrelevant to such a nation. That nation's social movements will have little relation to Western movements with similar names. The attitudes of its businessmen about the family and time will be different. Both the perception of uncertainty or risk and

the actual factors that make for uncertainty and risk will be different from what we are used to. For all these reasons, Lauterbach argues, we need to change our conceptual apparatus. Modernization is a distinct sociocultural system; it is not a transition from "feudalism" to "capitalism," or from "capitalism" to "socialism." Modernization is not just something that happens to the economy, for it also implies "change in cultural attitudes, administrative procedures, and power relations among social groups" (p. 160). Because Americans have a mental block against understanding themselves, it is argued, they are peculiarly unable to understand a pluralistic world except by imposing their own conceptual categories. From these misconceptions have come the suspicion, resentment, and outright failure associated with foreign "aid."

Stated in these terms, Lauterbach's thesis is not unreasonable, although it is not exactly new either. Given the author's considerable professional experience in the modernizing world, the reader would hope that in this book these ideas would receive vivid and rigorous documentation or exciting theoretical illumination. Unfortunately, I don't think that they do. Lauterbach's alternative definition of modernization is one of those notions that keep slipping away into the conceptual distance: "It must be defined in terms of exploring their [a people's] own socio-economic potential, both human and material, in line with their prevailing value system and attitudes..." (p. 143). I am not able to think of any activity in any country I know that could be excluded from such a rubric. Support for the general thesis comes largely from anecdotes that are too brief and abstract to be useful as case histories, and from bits and pieces of psychological theory. I find it incredible that a book of this title could be written in 1974 without mentioning the names (or ideas, or findings) of Hagen, Eisenstadt, Inkeles, Lerner, McClelland, Parsons, and Pye, among others. Perhaps due to this lack, the book abounds with assertions and recommendations that are vague, simplistic, and dubious: "Fascism and communism are different in their origins but similar in their results" (p. 10); "The psychological effects of a woman's work on her children are of considerable importance" (p. 118); "The modernizing nations should not assume that the wealthier ones owe them economic, social, or other improvements; and the wealthier powers should not become impatient if the recipients either hesitate or are unable to imitate successfully their experiences" (p. 147).

Lauterbach's own concept of choice in the

analysis of modernization—freedom versus to-talitarianism—leads to the most troublesome assertion: "Nobody has yet presented a totalitarian system in a modernizing country (or in an industrialized nation, for that matter) that has been more efficient and honest, especially in the long run, than are the admittedly slow procedures of many free societies" (p. 141). That last "many" makes it difficult to decide what this statement could mean; but in any case no evidence is presented to support it. Surely the comparison of post-1950 China with pre-1975 India cannot be made in such simple terms.

Chapter 6, "Society, Mental Health, and Developmental Stages," stands apart from the rest of the book. The author lays out structural and dynamic economic factors that are likely to have effects on psychological well-being, but the focus is mainly on developed countries. Here the discussion is provocative, and some of the hypotheses about debilitating effects of unemployment and economic down-turn have been confirmed by Brenner's recent research (Mental Illness and the Economy, 1973).

Anthropologists and sensitive world travelers have always known that each nation is unique and subtly different from each other nation; that in some ways cultures are all of a piece, while the concepts of economics, politics, and the other social sciences were dimensions that were abstracted and isolated from the rich particular settings of life. Ever since the bloom went off the rose of Point Four in the 1950s, sensitive economists and students of international development have sought illumination from anthropology, sociology, and psychology. Not all social scientists have responded with a commitment to look beyond their own national borders; and most politicians have ignored the whole effort. Yet there have been substantial fruits of this interdisciplinary ferment in the works of those mentioned above and others. What this book seems to show is how far we have yet to go toward a rigorous and coherent diffusion of this research, let alone to an understanding of social change and modernization.

DAVID G. WINTER

Wesleyan University

Personal Freedom and the Law in Tanzania: A Study of Socialist State Administration. By Robert Martin. (Nairobi: Oxford University Press, 1974. Pp. xvii + 224. \$7.25.)

Robert Martin's Personal Freedom and the Law in Tanzania is a book of such poor

intellectual quality that it discredits the scholarly reputation of Oxford University Press. Any prospective reader should be cautioned from the outset that it is not, in fact, a book in the standard sense. Personal Freedom can best be likened to an anthology since it consists overwhelmingly of a series of extended excerpts from political speeches, official statutes, court cases, government documents, radical texts, and secondary materials on East African politics. Martin's own contribution, which consists essentially of a series of brief "notes" at the beginning or end of each chapter, is, by his own acknowledgment, only supplemental and is intended to furnish a general frame of reference within which the various excerpted materials can be understood. The result of this unusual writing device is that the author's ideas are nowhere fully developed or elaborated and must be pieced together from his intermittent commentary.

Martin's decision to entitle his book Personal Freedom and the Law in Tanzania is also misleading since the book is not about personal freedom in Tanzania, but about its absence, The contents demonstrate, if anything, that Tanzanian law consistently sacrifices personal rights and freedoms to the executive power of the state. Indeed, Martin's purpose is to legitimize that policy by rejecting the liberaldemocratic tradition which esteems personal liberties as inappropriate to the developmental needs of a poor socialist state. The concept of personal freedom, Martin asserts, was concocted in Western capitalist societies where it has served historically to protect the economic interests of the propertied classes. This has resulted, so the argument runs, in great socioeconomic inequalities and it is absurd, in Martin's view, even to speak of personal freedom so long as social inequality exists.

Martin repeatedly juxtaposes the concept of individual or personal rights against the notion of rights for a greater collective entity known variously as "the masses" or "the people." The purpose of a socialist state, as he sees it, is "to guarantee the economic, political and cultural rights of the mass of the people" (see Note, p. 5). In order to achieve this purpose, the socialist state must equip its executive branch with unrestricted latitude to repress individuals and groups. Martin's textual commentary shows not the slightest qualm or misgiving about the elaboration of a doctrine which, if taken at face value by Tanzania's leaders, would encourage the grossest forms of political abuse in the name of collective justice. So far as this anthologist is concerned, any preoccupation with the protection of the individual against the state (such, one may suppose, as that in this review) is merely evidence of a bourgeois mentality.

The illogic in this position is so glaring that to identify it is merely to call attention to the obvious. Even if one were to grant Martin's simplistic view of the role of law in the West, it does not follow that there is no place for a concern with protection of the individual under socialism. Socialist states are no less capable of grotesque acts of political repression than capitalist ones. Yet Martin mindlessly catalogues Tanzania's truly awesome legalization of executive power and the absence of court restraints on that power as evidence that collective freedom is being upheld (see pp. 82-132). It is to the credit of the Tanzanian executive branch, and a mute official comment on the seriousness with which Martin's ideas are taken, that the government of Tanzania has, for the most part, proven reluctant to utilize the vast arbitrary powers at its disposal.

Martin's book does have one utility. It should serve as an object lesson to radical scholars engaged in the analysis of socialist societies. A critical perspective toward capitalism does not entail the abandonment of political judgment when noncapitalist societies are at issue. The heart of radical analysis is a general skepticism toward state power and its use; an analysis which obsequiously seeks to legitimize the unbridled use of state power by socialist leaders misses the entire point of the radical tradition.

MICHAEL F. LOFCHIE

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Resolutions and Decisions of the Communist Party of the Soviet Union. General Editor Robert H. McNeal. 4 Volumes. Volume I: The Russian Social Democratic Labour Party: 1898-1917. Edited by Ralph Carter Elwood. Pp. 306. Volume II: The Early Soviet Period: 1917-1929. Edited by Richard Gregor. Pp. 382. Volume III: The Stalin Years: 1929-1953. Edited by Robert H. McNeal. Pp. 280. Volume IV: The Khrushchev Years: 1953-1964. Edited by Grey Hodnett. Pp. 328. (Toronto, Canada: University of Toronto Press, 1974. Pp. 1296, inclusive. \$75.00, set of four.)

The basis of this major new reference work is the authoritarian character of the Leninist Communist party. Decisions adopted in the name of the party's Central Committee (as well as decisions adopted by periodic congresses or

conferences) are binding on the whole party. Whoever is able to speak with the authority of the Central Committee—whether that authority is derived from the Central Committee's plenary meeting, its political bureau, or its secretariat—can command the obedience of the entire party. When confronted with this truly charismatic authority—the ultimate source of value—even so formidable a figure as Leon Trotsky felt compelled to disavow his own reasoned judgment. Moreover, since the party commands the Soviet state, the goal of politics in the USSR, as in all Communist states, is to speak in the name of the Central Committee.

The editors, established scholars at Canadian universities, have collected the most important pronouncements of the Communist party of the Soviet Union, most of them readily available in various Russian language collections published in Moscow. The party's history falls naturally into four periods: from its beginning to the Revolution; the early Soviet period; the Stalin years; the post-Stalin period to the ouster of Khrushchev. One volume has been devoted to each period, with a single editor responsible for introducing the volume and providing the necessary headnotes. Since the individual editors were given considerable latitude, each volume has its own character. This has its advantages, but firmer guidance by the general editor might have eliminated distracting idiosyncracies, particularly in the somewhat inflated second volume. By supplementing the sources previously available in English for the study of Soviet domestic policy and politics (such as the collected works of Lenin and Stalin), the present collection will be helpful to political scientists generally, and particularly to students of comparative Communism who have specialized on regimes other than the Soviet one. Of course, very large gaps remain. There is no published collection in English of state documents for the entire Soviet period and many key articles and speeches also have not been translated, although these deficiencies are less serious for the last three decades because of the availability of the Current Digest of the Soviet Press. The present collection of party documents ends with the designation of Leonid Brezhnev as first secretary in 1964; a fifth volume is planned for the Brezhnev period.

The translations on the whole are well done, and the selection of documents is sensible, granting the element of arbitrariness that is inherent in the task. A few of the editors' decisions, however, are troublesome. One-third of the volume on the post-Stalin period is devoted to the party's 1961 program, even though it is readily available in English in

numerous Soviet and foreign editions. Partly as a result, such major developments as Khrushchev's decisive victory over Georgi Malenkov at the Central Committee meeting in January 1955 and the inconclusive but revealing struggle between planners and economic administrators at the session in December 1956 are not represented. The apparatus provided is modest. The indexes, which appear separately for each volume, have not been coordinated and are not very helpful (for example, faction-one of the key terms of Soviet politics-appears in the index of only one of the four volumes). On the other hand, there is a uniform and highly useful appendix displaying membership in the Central Committee's executive bodies (political bureau/presidium, organization bureau, and secretariat) since their beginnings in 1917.

The editors' introductions and headnotes provide helpful background materials. The serious reader should consult as well the standard works on Soviet history by W. H. Chamberlin, E. H. Carr, Leonard Schapiro, and others. Nevertheless, because of tight Soviet secrecy about higher politics, readers may find that authorities not infrequently disagree, even on crucial questions. The interpretive judgments of the editors of this collection for the most part reflect what consensus exists among specialists. There are exceptions, however. Most observers, I believe, would question the view that the convocation of the Nineteenth Party Congress in 1952 was brought about by "a coalition of Stalin's lieutenants" as "part of a growing rift between Stalin and his Politburo" (III:17). At times, an editor's concise account conveys a misleading impression of how Soviet politics is conducted. From the headnote on the October 1964 plenum of the Central Committee, for example, it might appear that the process by which Brezhnev replaced Khrushchev resembles the one by which unsuccessful prime ministers are replaced by their party colleagues in a parliamentary system (IV:316). (Khrushchev's victory over his enemies in June 1957, however, is thought probably to have required the aid of the army and political police-IV:92.)

The editors are competent guides, but nonspecialists should be warned that in interpreting these documents they are operating on treacherous terrain. Nevertheless, political scientists who do not read Russian should consult this collection, which might help them understand better the obscure politics of the expanding second world.

MYRON RUSH

Consociational Democracy: Political Accommodation in Segmented Societies. Edited by Kenneth D. McRae. (Toronto, Canada: McClelland and Stewart Limited, 1974. Pp. 311. \$4.95, paper.)

Professor McRae has undertaken the task of observing political systems that emphasize the continuous adjustment of sharply diversified and articulated social, political, and economic interests. The volume is divided into four parts: (1) a description of various approaches to "consociational democracy," including a brief explication of the multiple terminologies that have emerged from this literature; (2) a more indepth analysis of the theoretical perspectives of "consociationalism" that includes, among others, previously published studies by Val R. Lorwin, Arend Lijphart, and Gerhard Lehmbruch; (3) applications and illustrations of 'consociationalism" in the Netherlands, Austria, Belgium, and Switzerland; (4) an illustration of various aspects of "consociationalism" in Canada concluding with an epilogue.

McRae's introductory statement seeks to present a broad yet fairly integrated framework for the analysis of social and political behavior in states that have substantial cleavages in social structure arising from socioeconomic, ideological, and religious differences. His review of the consociational literature presents no new insights but is an excellent survey of the basic viewpoints that emerge from the existing literature. While McRae argues that his concern "... is not with smaller European democracies in general, but with certain patterns of social structure and political behavior that have been exemplified most sharply in the Netherlands, Belgium, Austria, and Switzerland" (p. 3), the only other application of consociationalism he provides that does not deal with these four states is the section dealing with Canada. One would like to have seen an additional section of the volume dealing with applications and analysis of states that did not emerge from a Western European social and political tradition. While McRae may be correct in his belief that the smaller European segmented societies may offer valuable insights and prescriptions for other states facing similar difficulties, the evidence has yet to be presented. This limitation aside, McRae is to be congratulated for developing a theoretical framework for the volume that knits the various essays into a meaningful whole.

The theoretical essays by Lorwin, Lijphart, Lehmbruch, Steiner, and Daadler each examine and illustrate the various dimensions of consociationalism: (1) Lorwin analyzes the patterns of social structure in the society and concludes

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that consociationalism "works" according to the degree of segmentation found in the structure; (2) Lijphart and Lehmbruch take the perspective of elite interactions and contend that the success of consociationalism emerges from patterns of elite behavior; (3) Daadler explores the argument that consociationalism results from historical political traditions (i.e., political culture). Each of these essays, particularly the ones by Lorwin and Lijphart, are well known, and represent provocative social science analysis.

The major portion of this volume is a series of case materials. The treatment of each state is somewhat different and includes a range of presentations from simple description to more sophisticated analysis of party and voting data. McRae's introduction to these case materials is brief, and as a result the reader who is unfamiliar with these "classic examples" of consociationalism might find additional readings necessary before any meaningful evaluation of the case materials can be made.

On the whole, McRae has assembled an interesting and at times provocative selection of essays on some of the most important issues confronting many developing and developed states. The volume is a useful vehicle in introducing important theoretical and comparative issues in today's integration literature. A volume such as this has a valuable place in almost any graduate division course on comparative theory and analysis. Its modest goals and the excellence with which it reaches them is unusual for an edited volume of previously published research.

ALBERT F. ELDRIDGE

Duke University

Mapai in Israel: Political Organization and Government in a New Society. By Peter Y. Medding. (New York: Cambridge University Press, 1972. Pp. 326. \$16.50.)

Unlike most other party systems in Western democracies, the Israeli party system is neither a two-party nor a multiparty system; it is what Maurice Duverger calls a dominant party system. A party is dominant when it is larger than any other party and outdistances its rivals over a certain period of time.

The problem to which Medding addresses himself in this book is not how Mapai became the dominant party in Israel but rather how it has maintained this position since the establishment of the state. Duverger attributes the domination of a party primarily to cultural and

ideological factors. "A party is dominant," he explains, "when it is identified with an epoch; when its doctrines, ideas, methods, its style, so to speak, coincide with those of an epoch... Domination is a question of influence rather than of strength; it is also linked with belief. A dominant party is that which public opinion believes to be dominant" (Political Parties, 1954, p. 308). This is not Medding's approach to the problem. He rarely mentions party ideology or its identity with the value system of society, but rather attributes Mapai's dominance to its success in establishing close ties with a large number of economic, social, and ethnic organizations.

The main part of the book describes and analyzes how the party, by using its control over the government and the Histadrut (a peculiarly Israeli institution, controlling most of the country's trade unions and agricultural settlements and owning more than 10 percent of the country's industries), satisfies the material and ideological needs of these organizations in exchange for their continuous support during elections. Bargaining between the party leaders and the different organizations, so Medding says, is primarily conducted within the formal and informal party bodies in which the organizations are represented.

However, I would like to question one of Medding's conclusions: his insistence that the bargaining going on continuously among the different interest groups within the party bodies results in what the author calls "a consensual power relation." The different groups within the party, he claims, reach a consensus which consequently becomes party policy implemented by the party leaders in the government and the Histadrut. Medding believes that this process disproves Robert Michels' famous theory of party organizations which claimed that the power of decision making in political parties was left to a handful of leaders ruling the party as an oligarchy.

Medding devotes a lengthy chapter to the operation of the party machine, its control of the party organization, and its success in denying its rivals both control and influence. The machine tries to satisfy the maximum number of interest groups within the party and outside it; it acts as a broker between the leaders in control of the government and the various organized interests in society. But can this process be described as consensual? Who is actually in control? On the basis of Medding's evidence it would seem that the power to make decisions is in the hands of the central leaders representing the party in the government and of the heads of the party machine. However, he

maintains, there is a division of labor between the two groups: some spheres are reserved for representatives of the party in the government while others are left to the heads of the party machine.

Medding's analysis sheds new light on the political process in Israel and provides a convincing description and explanation of the ways in which Mapai has maintained its dominant position in the Israeli political structure. It also describes the modes of operation of a dominant party and the relations between such a party and the interest groups which operate both inside and outside its organization. In my opinion, Medding's study of Israel's dominant party is important to an understanding of Israeli politics and is both well argued and amply documented.

YONATHAN SHAPIRO

Tel-Aviv University

The North Korean Communist Leadership, 1945–1965: A Study of Factionalism and Political Consolidation. By Koon Woo Nam. (University, Alabama: University of Alabama Press, 1974. Pp. 214. \$7.50.)

At the beginning of the present decade, U.S. "China watchers" were in reality largely "China listeners" committed to close examination of media transcripts and interviews with defectors. This difficulty has since been tempered by increasing access to the mainland for direct observation and meetings, which has led to better, although certainly still carefully limited, information. Unfortunately, students of North Korea in the mid-1970s are still "Korea listeners."

The present volume, which briefly traces the shifting coalitions within the Korean Workers' Party (KWP) from 1945 to 1966, unavoidably meets the "listener" criteria. Some ten pages are' devoted to the pre-1945 origins of the party, and a similar space is given to the 1966-1970 period; the bulk of the volume deals with 1948-1956. Professor Nam's study concerns itself almost completely with a small political elite fiercely competing for power. Moreover, its brevity forces the sacrifice of a detailed analysis of complexities. As an easilyread chronology and name list it serves well, but because it takes little account of both social and economic factors, the picture it presents often overlooks the subtleties. This approach is different from other succinct and important works such as Communist Politics in North Korea by Ilpyong J. Kim

The 100 pages which form the body of the book sketch the struggles among the leadership groups that had been separately formed in Korea, Manchuria, China, and Siberia. Professor Nam cogently reasons that many of the causes for intraleadership cleavages can be found in long-standing factional friendships and political orientations. These three main clusters were made up of those returning from—and supported by—the two adjacent countries' Communist parties (Kim Il Sung's group was a subdivision of the "Soviet faction," and those who had stayed in Korea during the harsh Japanese occupation were the "domestic faction").

The story of Kim's rise to domination in the party with Soviet support is well chronicled. The Korean Civil War encouraged some opposition to his rule, while the presence of the Chinese People's Volunteers gave hope to the China-returnees (the "Yenan faction"). The limitations of Soviet material support to Korea during the war might have encouraged opposition to the Soviet faction. In fact, a leader of that faction was purged in 1951, but he also had been an adversary of Kim's. China did not bolster Kim's opponents within the party, perhaps in part because Kim had enjoyed fraternal relations with the Chinese Communist party in the 1930s. In sum, the book points out that it would not be satisfactory to say only that a Korean leader was supported by either Peking or Moscow. Kim Il Sung's successes were largely built upon the state's encouragement of pride in its nationalism.

The leader of the domestic faction, whose base of support had been in the South, allegedly opposed a conclusion of the war before the South had been taken. He was removed from authority in early 1953; the truce followed. It should be added that both of these intra-KWP conflicts are cautiously and masterfully dealt with in the comprehensive and choice major study by Robert A. Scalapino and Chong-Sik Lee, Communism in Korea.

Kim emerged from the war as a nationalist hero and continued to strengthen his position in the KWP. This included an intensified rejection of the adoption of either a Chinese or a Soviet "model." Kim declared in December 1955:

The important thing is to master revolutionary truth. Marxist-Leninist truth, and to apply it to the actual conditions of Korea. We cannot have an imperative principle of doing [everything] just as the Soviet Union does. Although certain people say that the Soviet way is best or that the Chinese way is best, have we not now

reached the point we can construct our own way? (p. 106)

Khrushchev's February 1956 de-Stalinization speech, in concert with Kim's tightening control over the party, prompted the Yenan and Soviet factions to join together in order to oust Kim from power while he was abroad in mid-1956. Kim returned and reversed his rivals' plan. Since then, Professor Nam relates, the leadership has been solely Kim's.

It should be noted that the study lacks any theoretical perspective (e.g., what is "leadership"?). The book draws upon an impressive bibliography in providing a well-written sketch of the political leadership. Yet beginning here, a student of North Korea would want to read further about the complexities of both the system and its social-economic structure, as well as some parallels to roughly similar situations.

ROBERT R. SIMMONS

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Violence and Crime in India: A Quantitative Study. By Baldev Raj Nayar. (Delhi: Macmillan Co. of India, 1975. Pp. 150. Rs 40.00.)

Many Westerners think of India as a nonviolent nation, following in the pacifist tradition established by Gandhi. As pleasant as this image is, it is, unfortunately, false. Although India certainly is not an unusually violent nation, it also is not an unusually nonviolent society. Indeed, some forms of violence and crime, such as those types associated with Rummel's turmoil and revolutionary dimensions, occur quite frequently in India. It is important, therefore, that the comparative analysis of the political and economic bases for crime and violence include rigorous studies of these events in India. Baldev Raj Nayar has made an able step toward providing students of crime and violence with insights into the appropriateness of their empirical and theoretical generalizations for India.

Professor Nayar has tested two hypotheses, derived from the literature concerned with relative deprivation. The first states that long-term trends in crime are inversely related to both national economic and political capabilities, while the second hypothesis argues that deviations from this inverse association are a function of countervailing tendencies arising either from political or economic capabilities. Overwhelming empirical support is found for these hypotheses. One must, however, be very cautious in inferring any causal relationships from the evidence provided in *Violence and*

Crime in India. Professor Nayar states that "violence and crime are not things that happen at random but, as the record of the post independence period in India compellingly demonstrates, are causally related to system performance in politics and economics" (p. 72). The problem with this conclusion is that crimes are committed by individuals, and those individual criminals may or may not have personally experienced any effect from shifting national capabilities. The ecological nature of the data analysis does not permit inferences about the causes of individual criminal activities. The fact that in the aggregate these activities are correlated with gross shifts in economic and political capabilities in the nation cannot be taken to mean that individual experiences with shifting economic and political capabilities produce the same association. Having said that, and therefore having raised serious doubts about the causal inferences drawn from Professor Nayar's analyses, there is still much in this book that makes it an important contribution to the study of Indian politics.

Professor Nayar has convincingly demonstrated that most important categories of crime in India have followed clear and empirically interesting national trends. This result is important because it provides some basis for concluding that the Indian polity is sufficiently integrated so that national policies do, at the aggregate level, have uniform effects throughout India. The reader is left convinced that those who would argue, as D. H. Bayley has. that "very rarely indeed can evidence be found to support a contention that among most states at any given instant an all India factor has been operative" (p. 133) are wrong. The evidence for this conclusion, and its implications for policy analysis in India, are sufficient reason by themselves for reading Violence and Crime in India.

Beyond the discovery of clear national trends, Professor Nayar has somewhat disaggregated the crime data and revealed interesting state and regional factors associated with various types of crime. The most interesting and politically significant of these findings is the substantially greater frequency of politically meaningful crimes, such as riots, in the eastern states of India. The presence of group crimes, such as riots and dacoity, suggests the possibility of motivations other than greed or momentary passion causing violent behavior. To the extent that these crimes committed by groups are more highly coordinated and socially or politically motivated, the discovery of regional components to these types of crime raises important questions about differences in social

awareness, political integration, and channels of opposition in different parts of India. Professor Nayar's study of crime and violence in India provides an important and informative first step toward investigating those questions.

BRUCE BUENO DE MESQUITA

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Parlamentarische Opposition: Ein Internationaler Vergleich. Edited by Heinrich Oberreuter. (Hamburg: Hoffman & Campe, 1975. Pp. 293. DM. 32.)

Popular mistrust of political parties in Western democracies has increased in recent years. Yet shifts in attitudes have not significantly changed the pattern of party systems in which parties in opposition are competing with those in power for voter support. This study of parties in opposition may be compared in scope to that edited by Robert A. Dahl, Political Oppositions in Western Democracies, published in 1966. Once again only the Western democracies are selected for analysis in order to pinpoint the role opposition parties play in them. If no important political developments had taken place in the eight countries (Great Britain, United States, France, Italy, Austria, Sweden, Norway, and West Germany) covered in both volumes then there would be little reason for the Oberreuter study. But the editor, on the staff of the political science institute at the University of Munich, was cognizant of the political changes that had taken place in the United States and Western Europe in the last decade. What we have essentially is a useful up-date of the Dahl volume, with the inclusion of Switzerland and Denmark which Dahl did not cover, but the exclusion of Belgium and the Netherlands which he did cover. All chapters, except one, represent original pieces written for this volume.

In a brief introduction, Oberreuter deals with the tolerance, legitimization, and institutionalization of political oppositions in democratic polities with strongly entrenched welfare systems. He notes that, since the opposition in each political system has its own individual profile, a comparative theoretical approach is difficult, especially if the institutional variable, sharply different from one country to another, is taken into consideration. He is critical of Dahl's comparison of hegemonic or mixed political systems with polyarchies (in *Regimes and Oppositions*, 1973) because in the first two systems the people cannot criticize or loyally oppose their regime fully. Hence, he argues that

the term "opposition" must be restricted to primarily cooperative and competitive opposition among political parties in democratic regimes if it is to have any operational meaning. By narrowing his definition he must exclude, for instance, revolutionary groups intent on destroying the constitutional system. He admits that even his narrow concept of oppositionproduces definitional difficulties given the various types of governmental organizations, the structure of parties, and the effects of different electoral systems in the democratic polities. Thus, the contributors to the volume received the task of working up their own model of opposition in the country or, in one instance the countries, which they studied. Since Oberreuter fails to sum up similarities and differences which emerge in the country chapters, a new theoretical construct or conclusion is missing in this volume.

Yet there is a good deal of payoff, especially on recent developments, in the individual contributions. Symbolically, the chapter on Great Britain by Nevil Johnson leads off the parade of countries. In a country still proud to grant the loyal opposition maximum freedom to criticize Her Majesty's government, Johnson notes changes on the horizon. A "democratization" of the concept of opposition has taken place. In a number of recent elections, the voters, rather than Parliament, have exerted control over the government. To influence the voters, the opposition party, unable to change government policy in Parliament, must constantly present. nondogmatic alternative solutions in order to become a potential creditable government party. But often the alternatives are based on tactical rather than substantive policy differences-true in other countries as well.

George Romoser deals with "political opposition between consensus and instability" in the United States. He emphasizes the delicate balance that various administrations have had to maintain with Congress regardless of party majorities. While radical politics has ebbed, decentralized community-based groups clamoring for social change have added a new dimension to the American political scene.

Roy Macridis dissects the plethora of oppositions in France (in a reprint from Political Opposition and Dissent, Barbara McLennan, editor). While the chapter is dated—the socialist-communist united front was not yet in existence—the author did predict a reduction in the number of parties leading to a more simple division in Parliament and the electorate between the government and the opposition.

Silvano Tosi's chapter on Italy assumes a topical importance. He foresaw the recent governmental crises and the role the Communist party has played as a loyal opposition party ready to share in the national decision-making process. He notes that the old classical dialectic between government and Parliament has surfaced again, even though the parliamentary control function has suffered. Should a Christian Democratic-Communist government be formed some day, a powerful opposition would cease to exist.

Switzerland represents and Austria represented a coalition government model without significant party opposition. Yet in Switzerland, according to Jurg Steiner, the possibilities for parties in power being critical of specific pieces of legislation at the federal and cantonal levels are numerous. For economic interest groups, small opposition parties, and the average citizen, the initiative and the referendum provide modest means to criticize the government.

In Austria, according to Manfried Welan, economic associations also formed one source of opposition, until the breakup of the government coalition of the two major parties in 1966. While the parties did not bury their differences within the coalition, the role of Parliament atrophied. Even since 1966, the major opposition party has assailed the government with little effectiveness; this is not too surprising since about 80 percent of legislation is approved by both parties—a typical phenomenon of an emergent consensus on a host of government policies.

Nils Sternquist notes that in Denmark, Sweden, and Norway the position of the Social Democrats and other established parties has been weakened in recent years. Unable to gain an electoral majority, the Social Democrats had to form coalitions with either more radical Left parties or with bourgeois parties whenever necessary. As a consequence, programmatic and ideological differences have become more invisible, although new schisms between central and peripheral authorities, and between urban and rural areas have emerged.

Three authors (Dieter Grosser, Manfred Friedrich, and the editor) deal with the Federal Republic of Germany. They concur that the role of the opposition differs from that of Great Britain. Heirs to the Weimar system in which the opposition was split between parties loyal and disloyal to the constitution, the loyal opposition party in Bonn must confront an attitudinal mistrust of its so-called negative role and must confront a powerful executive and bureaucracy. As with many other opposition parties described in this volume, the arena of competition is a parliament in which the

governing parties have the upper hand, and in which the opposition party or parties shift their tactics from competition to cooperation.

Despite the cited shortcomings of this study, it offers a valuable survey of opposition parties struggling to gain power in countries with an entrenched welfare state system. While all parties pay lip service to the welfare system, tactical and ideological schisms between them have not evaporated; they make for some lively political contests.

GERARD BRAUNTHAL

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Zambia: Security and Conflict. By Jan Pettman. (New York: St. Martin's Press, 1974. Pp. 284. \$15.95.)

In January of 1976, in the midst of persistent war in neighboring Angola among competing liberation movements supported by external powers, President Kaunda of Zambia instituted a series of emergency measures designed, he announced, to preserve the security and perhaps the very existence of the Zambian state. Though surely anguished by the turn of events, Jan Pettman must take some satisfaction from the accuracy of her prefatory statement that "Zambia's national existence is precarious" (p. 7). Here is a book which seems to provide students of African politics significant insight into the current situation. Unfortunately, however, Pettman's preference for description rather than analysis reduces the value of her work in enabling observers to sort out why the emergency measures were taken, how those measures relate to the cluster of challenges Kaunda faces, and what resolutions of the apparent crisis are likely, or even possible.

Pettman's goal is the description of Zambia's insecurity. In separate chapters on Zambia's colonial heritage, polity, society, economy, and external relationships, she charts the best-known constellations in Zambia's political universe. Concerned with control, she focuses on challenges to Zambia's leadership and party. Her conclusion is that in the period since Zambia's independence in 1964 "threats to Zambia's security did not lessen ... and Zambia's capacity to cope with them did not increase" (p. 235). The organization of her inquiry does not lead her to explore why.

The major contribution of this book is its survey of personalities and events. Drawn from a wide array of sources, her information is for the most part accurate, though occasional striking errors (like her claim, p. 131, that

primary education in Zambia is now "almost universal") both mar the survey and reduce confidence in other reported information. Having relied heavily on what prominent Zambians have said, she rarely differentiates between claims made for specific political purposes and accurate assessments of events. As a result, there are substantial inconsistencies in her report (for example, her assertion, p. 115, that "the Zambian government is committed to a policy of rapid social and economic change," and her conclusion 20 pages later than "the majority of Zambians in rural areas were completely unaffected by development"). As well, she includes observations from other authors, with little effort to assess them critically or to sort out their contradictions. Her efforts at conceptual clarity are further undermined by her proclivity for ambiguous categories like "top civil servants," "leading members of the government," "personal advisers," and so on.

The major weakness of this book lies in Pettman's preference for description over analysis and in her failure to search out the links among the different dimensions she describes. Zambia's situation is perilous. No less dependent on copper exports now than a decade ago, Zambia's economy is structurally dominated by major transnational corporations. The South African connection of those corporations combines with Zambia's historic incorporation into the southern African economy to render Zambia's capacity for self-directed development-or even for locally desired responses to external threats-both limited and constrained by South African policies. Zambia's regional situation, and the dominant role of the transnational corporations, condition Zambia's internal political structure. And that internal political structure, in turn, both perpetuates the existing relationships and leads Zambia to fear, rather than welcome, liberation movements struggling against white rule, particularly those with a Marxist ideology. These are not separate dimensions conveniently discussed in separate chapters, but interrelated phenomena whose very interrelationships offer clues to, and explanations for, Zambia's insecurity. By failing to explore those interrelationships, by adopting a static orientation, Pettman fails to recognize the roots of Zambia's insecurity in its own political structure, and the roots of that political structure in Zambia's regional and international situation. Finally, throughout the book, Pettman confuses Zambia with its current leadership: though there are very real challenges to Zambia's existence as an independent state, most of what concerns Pettman has to do with the maintenance in power of Zambia's current leaders.

Eschewing analysis, Pettman offers reportage. But reluctant to pursue links between events, Pettman leaves us with a very incomplete survey. And concerned with control, only incidentally does she focus on change.

JOEL SAMOFF

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Property and Industrial Organization in Communist and Capitalist Nations. By Frederic L. Pryor. (Bloomington: Indiana University Press, 1973. Pp. xviii + 513. \$17.50.)

This is a remarkable book by an uncommon economist. The author-who has written on comparative economic systems-seeks to bring definitional precision and (insofar as the data permit) exact measurement to "issues of socialism, inequality, managerialism, bureaucracy, and centralization [that] have raised political passion for many decades" (p. 1). The degree to which he has succeeded will surely depend on the reader's tastes and predispositions, but in the process Pryor has produced a landmark study in comparative economic systems that political scientists would be well advised to notice. Not the least impressive are Pryor's extensive familiarity with the literature in both his own discipline and in many cognate fields of social science, his uncanny ability to scout out quantitative data from many countries and to manipulate them fruitfully, his clear delineation of issues at each step, and the general lucidity (even verve) of his exposition.

Choosing a broad bundle-of-rights definition of property and distinguishing it from mere ownership, Pryor uses the concept (in line with a growing trend in economics) to designate a given set of economic relations between men. Thus, a property right is both control over resources and a source of income, and property rights together with systems of motivation and information define the over-all economic system. His purview is wide indeed, and he reaches out in a number of major directions, carefully defines the concepts and the issues, and measures everything measurable in order to test statistically important propositions with respect to economic institutions. Where there are no usable data the author does not necessarily give up quantitative analysis. Thus, in inquiring into the influence of wealth distribution on income distribution, he turns to simulation models to examine the effects of such variables as the rules of inheritance, differential fertility among income classes, marriage patterns, and the intergenerational savings function (ch. 4). One way or another, Pryor's ultimate test is always: what do the figures tell us about institutions? He is not immodest about his approach, nor too modest about his findings, which reveal a good number of surprises as well as confirmations of conventional wisdom.

Among the empirical studies in the book that this reviewer found most enlightening are: The testing of various hypotheses to explain the degree and distribution of public ownership (nationalization) in different countries, where the causal factors "seem to lie outside the sphere of economics" (p. 65); comparative statistical investigation of the distribution of nonagricultural labor incomes in a number of both East European and Western countries, which he follows up with the finding that "within any modern economic system the inequality of labor income is vastly more important [at least statistically-G. Grossman] than the inequality of wealth holdings" (p. 89); and the concise but highly informative comparative analysis of the East European reforms of the 1960s, couched in terms of changes of property rights and industrial organization (ch. 7). The last is a masterpiece of analytical exposition and is highly recommended for those seeking to make sense of that complex matter. Finally, one should mention the probably somewhat less successful but equally stimulating empirical inquiry into the multidimensional conundra of centralization of control rights (Ch. 8).

Many will find equally helpful the various thorough discussions of major concepts and of the issues connected with them, concepts such as property itself; socialism; managerial control; convergence of systems (which the author views with a large dose of skepticism); and so forth. Often the concepts are further developed in considerable depth in the 60 pages of Research Notes (Appendix A). These sections should make valuable outside reading for courses in more than one discipline. In addition, the long bibliography, a glossary of some 50 terms, and (especially) the liberally annotated reference footnotes in the text, are a bibliographic gold mine for future researchers and students. The 18 statistical appendices provide mathematical explanations and additional quantitative material. There are also a helpful subject index and a name index-and comparatively few printing errors that this reviewer noticed (half of them, unhappily, in a transliterated quotation from Pushkin).

It hardly needs stressing that the application of formal statistical techniques to international

or intertemporal data for the testing of propositions regarding economic (or politico-economic) institutions is a powerful method, yet one fraught with considerable problems. To his credit, the author is quite aware of both strengths and weaknesses; there is nothing naive about his approach. Nonetheless—and leaving aside the mainly technical problems of data and statistical methodology—two general types of problem may be indicated briefly.

First, the statistical method perforce tends to accord less attention to the less easily quantifiable factors, such as ideologies and historical forces. Now, Pryor does not completely omit ideology from his ken; in fact, he devotes an appendix (A-19) to alternative definitions of ideology and furnishes a helpful tabular classification of "world views." But not unexpectedly he does almost nothing else with it. As for historical forces, they are to a degree implied in his various "developmental propositions" (cf. p. 372), i.e., findings relating to temporal changes in such variables as public ownership or centralization of control. These generally rest on the analysis of fairly short time series and refer to "history" in only a limited sense. Hence, it is not surprising that the seminal work of, say, Reinhard Bendix or Alexander Gerschenkron-authors who stress the interplay of systems, ideology, and history-is by-passed in the book under review, though Marxism does receive a good deal of attention.

Secondly, findings of international statistical comparisons of institutional phenomena cannot always be taken by themselves; they must be carefully interpreted in view of the associated institutions. Take, for example, comparative findings on the distribution of income or wealth. The statistical measures mean little unless we also take into account such (often unquantifiable) considerations as: freedom of consumer choice and rationing of consumer goods; the prevalence of illegal incomes (which naturally escape the statistics) and black markets, which are of enormous importance in the USSR and other East European countries; differential access to goods and opportunities among social groups in a given country; the significance of ethnic affiliation, status, prestige, connections, and pull in using one's pecuniary resources; freedom of choice of job or "people control" (as in the PRC); degree of job security; and, on a more general plane, the extent of micro-disequilibria throughout the economy. For these reasons, comparisons of the distribution of labor incomes in Western and East European countries (Ch. 3) are even more difficult to interpret than the author warns us to be the case.

This is a book of first-rate importance for the variety of reasons already adverted. Indeed, it is one of a series of important generalizing works in the field of comparative economic systems to appear within a brief span of years-a series that includes, in addition to the present statistical investigation, such very different works as the collection of conference papers edited by the late Alexander Eckstein (Comparisons of Economic Systems: Theoretical and Methodological Approaches, 1971), attempts at formal mathematical modelling (as by Jacob Marschak and Roy Radner in The Economic Theory of Teams, 1971, and by J. M. Montias in The Structure of the Economic System, 1976), and the partly institutionalist, partly neoclassical Economic Institutions Compared, 1977, by J. P. D. Wiles. Altogether, substantial fare with appreciable borrowing from other social sciences.

GREGORY GROSSMAN

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The Mexican Profit-Sharing Decision: Politics in an Authoritarian Regime. By Susan Kaufman Purcell. (Berkeley: University of California Press, 1975. Pp. iv + 216. \$15.75.)

A case study grounded systematically in a theoretical approach is more interesting generally than unstructured description, and a strength of this book is its attempt to apply aspects of Juan Linz's work (c. 1972) on authoritarian regimes. The decision to introduce a national profit-sharing system in the early 1960s is examined in the context of Mexico as an "inclusionary authoritarian regime." The goal is to understand how political processes in an authoritarian regime are similar to or different from corresponding processes in other types of regimes. Authoritarianism, along with corporatism and dependency theory, is of recent interest to students of Latin American politics due to the dissatisfaction with the "early" (1958-1967) SSRC political development school. Given the likelihood of continued interest in Linz's approach, we might consider its uses and limitations generally and as reflected in this case study. For just as a theoretical approach structures inquiry and provides for cumulative comparative work, it may lead us to expect patterns of behavior, sometimes more in eager anticipation than reasoned choice. Assumptions may become prejudices and hypotheses, points to be proven. We must beware, then, when the handles become manacles.

An authoritarian regime, following Linz, is characterized by limited pluralism, controlled participation, patrimonial rule, and elite consensus. A key premise is that it is useful to conceptualize about authoritarian polities as comprising a distinctive regime type, rather than as imperfect mutants of liberal or totalitarian systems. A second assumption is that the government (as one set of actors) controls societal interests (distinctive and subordinate sets of actors) and can maintain a separate decision capacity. Unlike pluralist systems, the authoritarian decision process is not determined by group interests but by the government, guided by a separate "state interest." The handles allow us to cast off the teleological bias that Mexico over time must somehow approach liberalism or totalitarianism, and facilitate more accurate description and classification. The manacles are its tendency toward static analysis (although this is not inherent), and an emphasis on process dimensions (interest group activity, ideology, style or rule), rather than on substantive policy outputs.

One is struck at the outset of this book by the author's unselfconscious embrace of tautology, the opiate of model-users, as she notes: "... there is now substantial agreement regarding the defining traits of an authoritarian regime, as well as a fairly strong consensus regarding the defining traits of an authoritarian regime, as well as a fairly strong consensus regarding the authoritarian nature of the Mexican government" (pp. 8-9). The problem is not vexing in setting the stage, and-based in the strength of static analysis-one of the best chapters is a characterization of the political environment. Here the accumulated literature (accurately and comprehensively surveyed) is synthesized in the authoritarian framework. Particularly good is the description of interest associations, and there is clear awareness of the varying degrees of autonomy and influence enjoyed by the several sectors.

The framework's process bias (style of rule and interest group behavior) begins to cramp when attention is turned to conceptual basics and an analysis of the decision itself. In a rather densely reasoned section on regime definitions, Guillermo O'Donnell's alleged distinction (in Modernization and Bureaucratic Authoritarianism) between "inclusionary" (he does not use this term) and "exclusionary" systems according to the government's orientations toward popular demands is significantly altered, O'Donnell classifies Mexico and Brazil as exclu-

sionary regimes because the lower classes are excluded from material and status benefits as a deliberate strategy at a given stage of industrialization. Professor Purcell sees the exclusion from material rewards to be less significant than the role of coercion in how they are excluded: "The fact that the lower classes in Mexico do not share equally in the country's riches does not mean that they are excluded from the political system.... An exclusionary authoritarian regime is one that relies heavily on coercion to forcibly exclude one or more mobilized groups from participation in the system.... An inclusionary authoritarian regime does not forcibly exclude any mobilized group from participation" (pp. 7-8). Mexico, thus defined, is an "inclusionary regime," and O'Donnell is left standing on his head. But the significance is that O'Donnell is interested in what the policies are, why they are pursued, and who benefits. Purcell is ambivalent on these questions, in part because the adopted framework leads her to stress how the policy was formulated.

Turning to the case study, one wonders whether the authoritarianism framework is well suited to the task of policy analysis. It is perhaps useful in describing the controlled nature of labor and peasantry and characterizing how the policy was formulated, and the best sections of the book describe the sequences and maneuvers in reaching the decision. But the author must resort to eclecticism to account for why the profit-sharing scheme was launched and who benefited from it.

The principal hypothesis derived from the framework on why the decision was adopted is that there exists a separate state interest; the state controls the principal groups; thus if there was a profit-sharing scheme, it was because the state willed it. In Mexico in recent years the government and the business sector have been joined in an "alliance for profits." The fact that the government wanted a profit-sharing scheme prevented the business groups from rejecting it, and the fact that the scheme was not really redistributive means that "it is fair to conclude that the government is not yet interested in profoundly altering the status quo" (p. 147). One gathers that the temporary congruence of state interest and business was largely coincidental. Should the government alter its stand, the business community would surely knuckle under. In the authoritarian imagery, then, we are dealing with a political economy of the "invisible fist."

An alternative hypothesis of merit begins from a different assumption, that governments are constrained by the nature of their supporting coalition. In Mexico, business is a key pillar of economic growth, and the business-government fate is closely intertwined. Thus, the profit-sharing device was adopted in 1961 because: (1) it cost very little and would not retard foreign investment or economic growth; (2) it was a powerful symbolic gesture which aided the government in reducing class tensions and reinforcing the moderate elements of the organized labor movement against radicals who were gaining strength; (3) it was a complicated measure (based on income tax returns!), and thus nicely suited for selective enforcement to reward friends of "cosa nuestra" and punish outsiders. There is ample evidence throughout the book to support this hypothesis, and one senses that the author is powerfully attracted to

But rather than seriously questioning the assumptions of the framework or stating coherent alternative hypotheses on Why profitsharing? and Who benefits?, we find either assertions taken on faith from the framework or eclectic reasoning. A catch-all list of factors is suggested to account for the decision: ideology, the president's personal interest and prior experience, symbolic quiescence, labor unrest. In the process, unfortunately, the framework tends to get in the way of explanation. For example, an insight: "The 1957-1959 labor disturbances ... resulted from the persistent neglect of organized labor by Mexican presidents. The profit-sharing reform of 1961 was part of a 'pay-off' to organized labor once the labor movement had been demobilized" (p. 135). But the framework emphasizes controlled pluralism and presidential preeminence; thus, "the labor unrest of the late 1950s . . . was not followed by new legislation benefitting the labor movement immediately, but approximately four years later. The delay not only reinforces the president's patrimonial image, but avoids giving the impression that violent disruption is rewarded. To give such impression would encourage a higher level of mobilization and would indirectly threaten the stability of the authoritarian regime" (p. 137; my emphasis).

Who benefited from profit sharing? The author is ambivalent on this, and the framework provides little assistance. Ultimately, she concludes, the government benefited, because it got what it wanted. A point of dubious validity is that labor also benefited, because government support gave it more than would have been possible in a liberal democratic system (p. 145). In terms of where the money went, we really do not know, but this is not Professor Purcell's fault, because the government has not published data on distributions under the profit-

sharing scheme. The silence on this point seems interesting in itself.

Indeed it is possible that authoritarian regimes function in a distinctive manner, that Mexico is of the species, and that the patterns of decision making there conform to the model. Unfortunately, in this case study the key points in the logic are asserted rather than demonstrated. One feels reasonably comfortable with the framework in certain respects, particularly the description of process dimensions. But the assertion of "state interest" to account for specific policy outcomes is an unsatisfactory way of avoiding some hard thinking about the dynamics of the governing coalition and the realities in which the Mexican policy makers operate.

JOHN J. BAILEY

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Alternative Strategies for Papua New Guinea. Edited by A. Clunies Ross and J. Langmore. (New York: Oxford University Press, 1973. Pp. 263. \$18.25.)

Although it is some years since this book was published, it is useful to review it now as the editors claim that much of the analysis and prescription "will remain relevant for some years to come" (p. vii). For those with knowledge of the Papua New Guinea scene it is also interesting to see to what extent some of the ideas here have been adopted by the now independent PNG government.

The very title of the book is misleading. According to the editors, "If this book has a theme, it is that there are alternatives" (p. vii). Few of the chapters, however, put forward a strategy: they make personal criticisms of and suggested amendments to what was then Australian policy on topics covered here, but they do not outline effective and realistic plans to reach specified ends.

Perhaps the most politically relevant chapters to the present political climate in PNG are those dealing with secession. Yet these are the weakest chapters. These chapters by James Griffin, Robert Waddell and Anthony Clunies Ross are uniformly naive, unrealistic, and internally contradictory. Griffin underrates the determination of a government forcibly to prevent secession and appears to believe that if Australian forces refused to be involved "the Papua New Guinea government may lack the resolution and the force to do so" (p. 130). It is irrelevant and gratuitous to state "an independent Bougainville could also be very peaceful without being as boring as, say, Nauru" (p.

127), and his rosy picture of a rich and prosperous independent Bougainville assumes that secession would be peacefully granted and ignores the difficulties of a totally copper-dependent economy (e.g., present-day Zambia). One also wonders why Griffin devotes 13 pages to Papua when he cannot estimate the extent of secessionist sentiment there (p. 103).

"Secession Without Tears" by Clunies Ross. creates its own myths in its title. It claims that those who separate should do so by consent (p. 133), failing to realize that this is where the argument usually begins. He decries the supposed argument that large states are "better" than small ones (p. 131) and proposes that in PNG potential secessionists "might be given a constitutional means of pursuing their objectives." He also proposes increasing decentralization of decision making to meet potential secessionists' claims (p. 132). While the latter is sensible, the proposal for a "secession formula" ignores the political realities in PNG and overrates the patience of both potential secessionists and the central government.

Waddell's chapter, "Constitutions and the Political Culture," undermines itself by internal contradictions and poor analysis. He notes that "to be successful a constitution must be a natural product of a country's political culture" (p. 88), without specifying what is meant by "successful." He notes that the Burmese "states" had little in common save their contact with the British raj (p. 87), but this applies to a number of countries including PNG and may act for a time as a unifying factor. Like the above two chapters he suggests secession before rather than after independence (p. 97) which ignores the Australian government refusal to tolerate such moves. He argues that a multifaceted PNG would be better off with a one-party state (pp. 94-95). But diverse interests often require diverse parties. Waddell fails to say how one party helps this situation where "regional or religious groups fear neglect or persecution if members of other religions or regions obtain power" (p. 94). He is similarly blind when he suggests "the emergence of a large, broadly-based political party with firm leadership is greatly to be desired" (p. 97). Having noted that PNG is heterogeneous (p. 94) and that neither class nor ideology are likely to be the mainsprings of political parties (p. 96), where does he expect this broadly based party will appear from?

The other contributions to this book are more valuable. Tos Barnett's "Law and Justice Melanesian Style" is a fluent outline of the contradictions between Western law and Melanesian life style. He makes suggestions about

how some of these contradictions could be overcome or at least lessened, but it is worth noting that some of these "remedies" have similar application in Western societies.

Nigel Oram's "Administration, Development, and Public Order" examines the then administrative organization and considers what administrative reforms are desirable for self government and beyond. He notes the overcentralization and overly complicated nature of the public service (p. 7), and argues for "much closer coordination of governmental activities at district level" (p. 17). For this he suggests the establishment of village courts to bridge the gap between administrative and judicial law enforcement (pp. 41, 49-51; Barnett p. 67), the development of an elite corps of district and assistant district commissioners (p. 55), and politically informed higher-level public servants (p. 56).

John Conroy's chapter on educational policy and John Langmore's on public service pay and localization are both useful outlines of those topics, but they are inconclusive in that they state the problems without providing "alternative strategies." Ross Garnaut's "National Objectives and the Choice of Industries" is a difficult paper for noneconomists and shows, without resolving, the dilemma of economists everywhere-whether to pursue growth or welfare objectives (p. 165). Finally, the two last chapters by Clunies Ross are highly pertinent essays on the economic policies which he believes should be pursued by PNG. He argues that localization of management rather than ownership should be attempted, and resolves Garnaut's unanswered dilemma by arguing for "social planning" and goals "which go beyond the mere increase of 'income' per head or the extension of individual choice" (p. 231). This approach "requires political leadership that is united, popular, determined and sensitive" (p. 233).

This is an interesting book, if not always perceptive.

PETA COLEBATCH

Waigani, Papua New Guinea

York: Monad Press, First Edition 1937; Second Edition 1973. Pp. 160. \$6.95.)

The reappearance of From Lenin to Stalin adds one more title to the growing list of works by Victor Serge republished in recent years. New editions are now also available in Eng-

lish-translated from the French-of three of his novels (The Case of Comrade Tulayev, Men in Prison, Conquered City); of his autobiography (Memoirs of a Revolutionary 1901-1941); of two other historical commentaries (Destiny of a Revolution and Year One of the Russian Revolution); and of his biography, written jointly with Natalia Sedova Trotsky (The Life and Death of Leon Trotsky). Publishers presumably have decided that Serge will attract a number of different reading publics-political scientists and historians interested in the Soviet experience, international Communism, and the origins of totalitarianism; the literary readership on the lookout for social novels imbued with an intense moral concern; and the "New Left" in search of non-Stalinist revolutionary heroes on the model of Rosa Luxemburg, Peter Kropotkin, Leon Trotsky.

Certainly, if one is looking for an intellectual of world repute who participated fully in the romance of the revolution while escaping entrapment in the apparat or the Cheka, then Serge fits the bill remarkably well. Born in 1890 in Brussels of parents who were themselves Russian revolutionary emigres-his real name was Kibalchich-Serge became an anarchist while still in his teens and editor of the illegal l'Anarchie at the age of twenty. He spent the years 1912-17 in Belgian prisons; took part in an abortive uprising in Barcelona in July 1917; found himself an internee in France in 1918; reached Russia in 1919; threw in his lot with the Reds; lived through the crisis of the Civil War in Petrograd; published a book in 1921 on the role of the anarchists in the Russian revolution; worked with Zinoviev at the executive committee of the Comintern; spent the years 1922-26 as a Comintern agent in Western Europe; was in Germany for the October uprising of 1923; returned to Russia in 1926 to join Trotsky's Left Opposition; spent the years 1933-36 as a prisoner of the GPU; was allowed to leave for the West following "l'Affaire Victor Serge," an intensive campaign waged for his release by Western intellectuals (among whom even Romain Rolland and Andre Gide played a role); worked for the Trotsky cause in the years 1936-37 and for POUM during the closing stages of the Spanish Civil War. He escaped Vichy France for Mexico at the last possible moment and died there poverty-stricken and lonely in 1947.

From Lenin to Stalin was written in 1936, published in 1937, and, like his Destiny of a Revolution, belongs to the period of collaboration with Trotsky whom he met soon after reaching the West in April 1936. Trotsky relied on Serge's testimony, and was influenced by it,

while then composing his *The Revolution Betrayed*, also published in 1937. In most essentials, Serge's book is a Trotskyite tract. It explains Soviet history along strictly dichotomous lines with the period 1917–23 described as that of "Soviet democracy," the "great years," and the post-1923 period as that of growing "bureaucratic" nightmare. This sudden transition of the body politic from robust health to cancerous degeneration is explained partly in terms of personality (Lenin's illness and death, Trotsky's fall) and partly in terms of sociology (the swamping of the veteran revolutionary elite by the post-October mass of careerists and opportunists).

Yet even here the lines of the impending dispute between Serge and Trotsky can be discerned in various judgments scattered discreetly throughout the text. "It would have been relatively simple to reach a compromise with Kronstadt and avoid useless massacres. . ." (p. 34); "moral criteria sometimes have greater value than judgments based on politics and economic considerations.... It is untrue, a hundred times untrue that the end justifies the means" (p. 58). Clearly, the alliance between Trotsky and Serge-eager to grapple with his personal experience of the GPU system-would not long survive. Serge was soon to produce a much more critical and authentic description of the early post-October period in his Memoirs of a Revolutionary.

Perhaps the most interesting aspect of the book under review lies in the fact that here was the first eye-witness report on the Stalinist terror as it approached its apex, the Ezhovshchina. Serge was the one who by some miracle escaped to tell the tale. In general, his analysis of the Show Trials stands the test and his imaginative reconstruction of Stalin's paranoid monomania likewise retains its interest today, is reminiscent of Solzhenitsyn's portrait in The First Circle and is arguably even more powerful.

Monad Press, then, is to be congratulated on re-issuing this book. Perhaps the full corpus of Serge's works will gradually be published in English. And is it not time for a full-scale biography?

JONATHAN FRANKEL

Hebrew University

A Social Basis for Prewar Japanese Militarism: The Army and the Rural Community. By Richard J. Smethurst. (Berkeley: University of California Press, 1974. Pp. xxi + 202. \$14.00.) Though much has been written about the role of the military in political decision making of prewar Japan, little has been known about the involvement of the Japanese army in the rural community. Richard Smethurst's book, which describes and analyzes the prewar Imperial Military Reserve Association (teikoku zaigō gunjinkai) and three other organizations, fills that gap.

As the author points out, the pre-1937 Japanese military was quite a small force of less than 250,000 officers and men, which every year drafted only about 15 percent of the 20-year-old men of the nation. In order to enhance the influence of the armed forces, spread military values, and strengthen national unity, the army authorities in 1910 sponsored the establishment of a centralized reservists' association. The new body was open to all men between the ages of 20 and 40 who passed the conscription physical examinations. By 1936 the association had a membership of 2.9 million, half of whom had never served in the armed forces. Before 1933 three other organizations were established under the auspices of the army: the National Youth Association, the military-training youth-schools, and the National Defense Women's Association, all of which were under reservist supervision. The reservists and the other groups utilized the members' traditional commitments to the village, in their effort to mold "national villagers" who would support the military and its policies. In this manner the army succeeded in becoming integrated into the rural community no less than the schools. Moreover, the fact that the local leaders of the reservists, youth, and women groups came from the upper strata of the village contributed to the strengthening of traditional social obligations in rural Japan. Smethurst makes the point that unlike the rise of Nazism in Germany, in Japan the military rose to power not as the result of social dislocation, but on the basis of its roots in a well-organized populace in the countryside. The Japanese army, as he puts it, succeeded "almost without trying" in establishing what Hitler dreamed about but could not achieve: a stratified "organic" society.

The book is well researched. It is based on extensive use of primary sources like unpublished archives, documentary collections, and the publications of numerous little-known organizations. Of special interest is the author's in-depth research into four agricultural communities of different size in central Japan. In these four case studies, detailed information was obtained from local archives, hundreds of questionnaires, and personal interviews. The

book contains many tables and figures which provide data on various aspects, such as yearly enrollments of youth-schools or budgets of reservists' branch offices.

One may regret that out of the five chapters of the book, four are about the aims and the structure of the organizations under scrutiny and only the last chapter tells us about their activities. The reader would like to hear more about the involvement of the reservists or the other groups in the various political affairs of the period. Thus only one short passage (on p. 177) is devoted to the famous Minobe controversy of 1935, in which the reservists played a central role (although Smethurst elaborated on it in his contribution to George M. Wilson, ed., Crisis Politics in Prewar Japan, Tokyo: Sophia University, 1970). The book does not mention the task assigned to the reservists in the many "renovation" plans of the period, such as the role they were to play in Kita Ikki's "reconstruction" of Japan.

Another problem is the name of the book. We all know well that publishers prefer high-sounding titles which would appeal to a wide audience, though often the book deals only with a small portion of the announced subject. The name A Social Basis for Prewar Japanese Militarism for a book on the reservists' association and three other related groups may raise expectations which the author did not intend to fulfill.

BEN-AMI SHILLONY

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The Politics of Pluralism: A Comparative Study of Lebanon and Ghana. By David R. Smock and Audrey C. Smock. (New York: Elsevier, 1975. Pp. x + 369. \$12.50.)

Audrey and David Smock, currently with the Projects Office of the Ford Foundation, conducted the bulk of their research for this volume during two and a half years (1969-72) of residence in Ghana and a 15-month period (1972-73) in Lebanon. In this book the authors detail a number of interesting interethnic, country-specific relationships and make policy suggestions which should be of interest to development specialists. Many of the standard "mechanical formulae," which have been posited as offering hope for "national accommodation" in ethnically fragmented societies, are reviewed in this work-for example, the establishment of a "lingua franca" to overcome language diversity; political reform (e.g., the incorporation of minorities into the political

system in a meaningful way, by proportional representation or confederation); the creation of a nationally oriented educational system (one designed to inculcate "national" values which transcend parochial loyalties, to foster appreciation of diversity and promote tolerance, and to develop a sense of history and identity with respect to the state); the use of the governmental bureaucracy to encourage the growth of an elite corps committed to the ideas of efficiency, effectiveness, and the larger political unit; the institutionalization of conflict management techniques.

After tracing the historical development of Ghana and Lebanon and examining social relations in those two states, the Smocks concluded that Lebanon's distinctive features (i.e., the presence of a number of minorities who had come to Lebanon seeking relief from persecution, the existence of both intra- and intersect conflict, extensive mixing of different groups, and systemic flexibility provided by the possibility of conversion) contributed to the evolution of the "Confessional Principle" in the matter of communal representation-the unwritten National Pact of 1943 established the formula for inclusion of the various communities-and helped to produce a system "able to manage communal competition and to reduce tensions even in a society with deep communal divisions" (p. 332). On a more general level the authors argue that "a political system that recognizes the legitimacy of communal interests and incorporates communal groups in a meaningful manner (i.e., as Lebanon does) has a far better opportunity to effect moderation and conciliation than one that refuses to come to terms with communal political actors (viz., Ghana)" (p. 332). While acknowledging that the "will to live together and to make the system work constitute essential ingredients" in state development (p. 333), the Smocks stress the value of institutional and policy approaches to national accommodation. Ghana's inability to institutionalize ethnic conflict management mechanisms, its lack of success in promoting a more national outlook among its people, its failure to satisfy the economic and social needs/demands of constituent groups, and the government's inability to promote efficiency and rationality as contemporary virtues lead the authors to conclude that, unlike Lebanon, Ghana's future will very likely include ethnic conflict and upheaval (p. 252).

Although they based their book on an extensive review of the theoretical literature on "ethnicity," on intimate knowledge gained from their participant-observer research strategy, and on information gathered from inter-

views with students and adults, the Smocks failed, as evidenced by the recent Lebanese civil strife, to assess accurately the significance of the issue of identity. In this failure perhaps lies the Smocks' major contribution to the field of ethnic studies.

While it has become fashionable to ask about effective measures of ethnic-awareness and to attempt to develop indicators of ethnic friction, some scholars of nationalism (notably Rupert Emerson and Walker Connor) have not been sanguine about the success of such efforts. The difficulty, it seems, lies in the nature of the identity bond (i.e., psychological and, hence, abstract). Because it is intuitive, this bond defies easy measurement: "The simplest statement that can be made about a nation is that it is a body of people who feel that they are a nation; and it may be that when all the finespun analysis is concluded this will be the ultimate statement as well" (Rupert Emerson, From Empire to Nation, 1960, p. 102), Hence the field of identity studies is a difficult one for those who seek absolutes. Perhaps Crawford Young's admonition about the ever-changing nature of the "self" should be a first principle of ethnic studies: "The definition of groups is in constant flux; any theory of ethnic conflict must incorporate change as a central element. . . . At any moment, ethnic conflict may appear to eclipse all other factors in the political equation; a few years later, the same cleavage may appear entirely muted, and quite irrelevant to explication of the political process" (The Politics of Cultural Pluralism, 1976. p. 5).

If a beginning is to be made in understanding ethnicity, authors of studies focusing on this phenomenon should seek to be as precise as possible. Discussion of research strategies and methodologies and provision of details would go a long way toward making it possible for scholars working in other geographical areas to replicate studies, to extend findings, and to, perhaps, refine research instruments. The Smocks may have contributed to this effort but there is no way to know for certain. Their study questionnaires are not provided; and information which is found in the appendix of the book indicates that their surveys varied in construction, content sought, populations sampled, and interview situations. Walker Connor ("Nation-Building or Nation-Destroying?," World Politics, 24, April 1972) has treated in detail the dangers inherent in a situation of terminological confusion where the proliferation of reference words suggests a multiplicity of entities when, in fact, most evidence indicates that one entity is being examined. In

this regard the Smocks might have profitably treated the various Lebanese communities as distinct ethnic groups rather than simply in terms of their Muslim and Christian "confessional" identities. However, none of this detracts from the overall value of this book. The questions raised in this work are important ones and are worthy of consideration. For this reason area experts, students of nationalism, and development specialists would be well advised to place this title on their reading lists.

JOHN E. ROBERTSON

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A Survey of Race Relations in South Africa, 1972. By the South African Institute of Race Relations. (Johannesburg: South African Institute of Race Relations, 1973. Pp. 484. \$3.95, paper.)

The South African Institute of Race Relations is an inter-racial organization unrelated to government or party, which engages in research and discussion, as well as more activist pursuits, in the complicated area of race relations in the Republic of South Africa. Aiming at the promotion of understanding and furthering "the social, economic, and political development of all communities" in that heterogeneous country, it periodically issues a Survey, which summarizes significant occurrences in its field of interest during the preceding year.

This Survey of 1972 is a complete, concise summary of racial developments and an excellent starting point for scholars and laymen who wish to study events within a particular time span. It does not interpret or analyze, that is left to the reader who wishes to penetrate more deeply. But one who reads it will obtain a broad sweep of the ways in which South Africa is moving, even though the sudden shifts from one subject to another make for a rather disjointed reading journey.

It is a compilation of items culled from miscellaneous sources like newspapers, accounts of parliamentary debates, and scholarly publications like the South African Journal of Economics; some information appears to have come from government officials directly. Documentation is now always complete, as some compilers have fallen into the habit of citing newspaper accounts of official statements rather than the documents themselves; this is particularly noticeable in references to United Nations actions. Court decision references occasionally suffer from the same defect (see pp. 95–96).

One advantage of this book is that it brings out facts unknown to the average person or even the Africanist whose specialty is not the republic south of the Limpopo, such as information about the education of children of the small Japanese and Chinese majorities, strikes (all illegal) by African workers, or the farming out of African inmates by penal institutions.

The book is not tendentious, however; recital of digested facts is uncolored by emotion and is objective. Since it is concerned with the broad topic of race relations and not with South African affairs generally, one might easily conclude that the compilers have racial axes to grind. Analysis shows this not to be the case. It contains plenty of ammunition for anti-apartheid crusaders, but there is also enough upon which it can be argued that racial relations in that troubled land are really improving.

Events in southern Africa have been moving so rapidly that one regrets that this digest of 1972 events is not more up to date. It was written before the detente with Zambia over Rhodesia, announcement of moderate reforms by Prime Minister Vorster, and South African intervention in the Angola conflict. But the passage of time has not rendered this book less valuable; in fact, it is a useful background for recent events and that is its principal interest to the American reader.

Because the race situation has affected South Africa's foreign relations, the Survey properly has a chapter on foreign affairs and a section on the United States. Although one wishes that much documentation had been more direct than the second-hand South African sources used, one cannot call the section biased. It notes the "soft" policy of the Nixon administration to South Africa, as well as the criticism of it, including a congressman's prediction of a Vietnam-type "holocaust" in South Africa.

A chapter on South Africa's territory to the northwest—called, tactfully enough, "South West Africa (Namibia)," with the United Nations name in parentheses—is a useful guide and could be read as an introduction to a study of what has recently happened there.

Early in 1972 the producer of this book was one of four organizations which the prime minister announced he intended to investigate through a parliamentary select committee—an incident recorded without fanfare on page 52. But he complemented the Institute by calling it "the least dangerous of all these organizations."

EMMET V. MITTLEBEELER

The Dynamics of Right-Wing Protest: A Political Analysis of "Social Credit" in Quebec. By Michael B. Stein. (Toronto: University of Toronto Press, 1973. Pp. xv + 256. \$12.50.)

Social credit, a Canadian political phenomenon, is an economic and political ideology, a protest movement, a political party, and a manifestation of ethnic conflict. While Michael Stein describes social credit in Quebec with respect to each of these attributes, his objective is to point out that social credit in Quebec is one manifestation of right-wing political phenomena. He refers to organizational characteristics such as developmental patterns, leadership styles, and internal factional disputes to distinguish social credit as a right-wing movement from either political parties or revolutionary movements. As evidence of these characteristics, he draws on the writings of the social credit's original theorist, Major C. H. Douglas; on previous studies of the movement's emergence as a political force in Quebec; and on an analysis of interviews with Quebec social credit leaders and activists.

The interview data do add to our understanding of the attributes and beliefs of political activists and do show, once again, that activists on the right are not estranged social misfits clinging to outmoded traditions. The Quebec créditistes, on the whole, have status in their community; are helpful of achieving political power and of having an impact upon public policy. Stein's internal analysis of the data on activists shows that a minority of them do have the characteristics attributed to rightwing extremists in much of the literature on comparable American phenomena of the 1950s and early 1960s, i.e., they are alienated, not well educated, and they join the movement for its social affiliations more than for its political objectives.

A shortcoming of this study is the discontinuity between the focus of the narrative history of the ideology and organization of Social Credit in Quebec and the analysis of the interviews with the movement's leaders. Generational differences in leadership style and goals, a theme of the narrative history, is not discussed with reference to the interviews with leaders. Another shortcoming is stylistic: this study was originally a doctoral dissertation. The conventions of organizing and presenting research particular to a thesis have remained in the book, to the detriment of the flow of the narrative and crispness of the analysis. Nonetheless, The Dynamics of Right-Wing Protest

should be a reference for students of Canadian politics and of right-wing politics.

SHEILAH KOEPPEN MANN

The American Political Science Association

Politics, Power, and Bureuacracy in France: The Administrative Elite. By Ezra N. Suleiman. (Princeton, N.J.: Princeton University Press, 1974. Pp. 440. \$20.00, cloth; \$9.75, paper.)

Every now and then someone submits a doctoral dissertation that does more than demonstrate professional competence. It announces the arrival of a first-rate expert in a substantial field. Ezra N. Suleiman's *Politics, Power, and Bureaucracy in France* does just that. It does it somewhat laboriously with its burden of 639 footnotes, 79 tables, 2 graphs, 8 diagrams, and one questionnaire, plus a bit more social science methodology and apparatus than either the subject or this reader can profitably absorb; but it does it powerfully, with a parade of well-ordered information and ordering insights.

The aim of the study is to examine the ranking civil servants of France, probably the most powerful and possibly the most able of any Western country, as part of the French political-administrative system. Though the ranking French civil servants have been studied more closely in both popular and scholarly literature than Suleiman's introduction would lead one to suspect, the present volume is the most solid in its compilation of information and the most ambitious in its efforts to relate administration to politics. It not only amasses-and controls-an impressive arsenal of empirical materials about their origins, training, attitudes, and behavior (and here "empirical materials" means, most prominently, interview data-the easiest to amass but the most difficult to control), but it also makes sense out of most of that information by relating it to the substance of French politics.

The dust jacket, the introduction, and an earlier piece by the author in World Politics represent the study as though it were an intelligent write-up of information gathered and sorted through the careful application of a scientific method of empirical enquiry. For better or worse, and I think for better, it is nothing of the kind. In all its many strengths, it is the author's book, and not the method's. The author's extraordinary knowledge of French politics and administration and his sharp intelligence are what carry the book along. The method—a series of elegantly conducted inter-

views with 135 very top civil servantsgenerates lots of information. That information is then carefully presented in tables, charts, and quotations, and the 26-page questionnaire is appended. But the survey findings do not begin to tell us how the French bureaucracy works. Suleiman does that, and not on the narrow and rickety basis of interview data. Except for some unsurprising data about the elite origins and backgrounds of the top civil servants, information readily available elsewhere, the interviews generate cautious statements of views, which are supposed to express attitudes and perceptions-all in all, something quite different from empirical findings about fact and process. An empirical examination of the workings of the French bureaucracy, one that examines how decisions are taken, and more interesting yet, how the decision-making agenda is established, would be an invaluable addition to our knowledge of comparative politics. But that is not what Suleiman does in this book. Essentially, the interview technique permits him to play the role of participant-observer; in this case it is more a literary device than a scientific technique. Because of his rich knowledge of French administration and politics, he is able to profit from that position. The survey results are really his notes, his departure points. The book, therefore, is not a pioneering work in the scientific, empirical study of administration. It is not even an application of some previous breakthrough in empirical methodology to the study of the recalcitrant French. It is, instead, an intelligent and concrete study of the workings of the administrative state based on familiarity, understanding, and insight.

In the course of his reflections on the political nature of administration, Suleiman argues with Grant McConnell on the nature of interest group pluralism; with Michel Crozier on the adaptability of the French system; and with Jean Meynaud on technocracy.

The study treats a golden age of French bureaucracy, the fifties and the sixties, a period in which the ranking civil servants seemed to be not just all-powerful but all-successful. They took on the giant task of overseeing the nation's total transformation, and they succeeded. GNP grew by an average of five percent per year, far faster than the U.S., about as fast as the awesome Germans. They celebrated their success. The atmosphere of those years in the French bureaucracy (not a place traditionally known for its joyful ambiance) made the Best and the Brightest phase of Washington seem like a passing wink. Not only did they transform the nation-and almost no part of its escaped improvement at their hands-but they

also provided the commentary on the process. Under various and sometimes amusing pen names, they wrote the books, pamphlets, and articles which praised, explained and even criticized that transformation. Because Suleiman stays so close to the Directors of the Central Administration and the Cabinet staffs (his impressive survey sample) he does not discuss the ranking civil servants who movedwith astonishing agility-all over the complex administrative system: men like Louis Armand, Paul Delouvrier and François Bloch-Laine who best represented what Frenchmen had in mind when they changed the name (but not the dose of irony) for the super civil servants from bureaucrats and Mandarins to technocrats. It is a bit like analyzing the administrative politics of New York sans Bob Moses. Perhaps the best empirical indicator of their hold on the nation's imagination, as well as its levers of control, was the results of a poll taken among students at the school for political science in the early sixties. When asked to list the men they most admired, the future hauts fonctionaires put Francois Bloch-Laine first on their list. John F. Kennedy came in second. De Gaulle and Sartre finished out of the money.

The technocrats developed and modernized France, and they apparently had no trouble knowing what those words meant. No imaginable group of Berkeley students would be blessed with such ideological ease. Suleiman raises this critical question of what ideology guided their actions. His discussion relies on quotations from his interviews and commentaries on the works of other writers; sometimes he pulls their sentences out of context in order to set them up to push them down. But he does not satisfactorily resolve the question of ideology. The failure is not due to some minor technical problem, nor is it of minor importance. And the difficulties compound when he turns to the delicate relation of the State to big business. He discusses it in terms of how the state deals with interest groups. Within that framework he succeeds in confronting American approaches to interest groups-both mainstream liberal-pluralism and its critics, especially McConnell—with French discourses on the St. Simonian nature of the state. But the whole enterprise springs leaks when he reaches the critical distinction that the civil servants repeatedly make between legitimate and illegitimate interests. That distinction is never fully explained: it just sits there like a kind of Cheshire grin mocking, as it obscures, any real understanding of the relation of the state to big business.

In part his difficulties in these important

areas are due to his not having a concrete framework in which to situate the civil servants' actions and their talk. Even a rudimentary "advantages of backwardness" approach, straight out of Veblen, would have added much in the way of analytic power to his explanation of their actions, their ideology, and their unflinching bonne conscience. Without such an ordering structure it is hard to relate what they say to what they do, and what they do to what is going on-to the specific character of French economic development which gives the different parts their meaning. As a result, relatively simple but revealing questions and facts are not noted. For example, would their happy balances of ideology and action, of power and independence have been disturbed had they ever opposed a single merger among big industrial firms rather than having consistently promoted and subsidized them? Unless we can relate their behavior (as well as their origins and attitudes) to what in fact they were doing-to their role in a particular phase and form of development-we are left with many wellinformed but separate lines of argument. We are also unprepared for developments subsequent to the book such as Prime Minister Chirac's dropping the political subtlety of the Gaullist line and successfully taking a big political stick to the top St. Simonians.

Suleiman tells us in his introduction that he is now working on a study of "the interrelation of public and private elites in France in the formulation of public policy." It seems that it will pick up at just the point where this first study leaves off. It is almost sure to display what so distinguishes this first volume, the author's remarkable knowledge of his subject, plus his intelligence and craftsmanship. If the reliance on attitudinal surveys can be reduced (or perhaps replaced by more robust empirical materials) and perhaps a greater dose of irony can be added-something the subject desperately deserves—it should be a seriously interesting study, not just for French experts, but for all those puzzling over the workings of the modern administrative state. The first volume already is.

STEPHEN S. COHEN

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Politics in Zambia. Edited by William Tordoff. (Berkeley: University of California Press, 1974. Pp. xiv + 439. \$14.00.)

Zambia—an enigma in African politics. Is Zambia a relatively wealthy and stable African state, guided by a clear sense of moral purpose toward a humane and egalitarian African socialism? Or is Zambia a dependent, externally oriented African state, constrained by its white-ruled southern neighbors, and pushed by erratic copper prices and a rapacious sectional elite toward a very unequal and unstable African capitalism?

These contrasting images reflect both arguments about Zambia and a much larger discussion about Africa as a whole. Zambia offers a microcosm: wealth and dependence, articulated national ideology and severe sectionalism, urban splendor and periurban shantytowns, industrial base and continued food imports, and perhaps most important, a front-line position in the struggle against minority rule in southern Africa.

The recent book by Tordoff and his colleagues is a major contribution to this debate, but at the same time it is uneven and frustratingly incomplete. Like Zambia's independence, it is an event delayed beyond its time.

As outlined in Tordoff's and Molteno's introduction, Zambia (Northern Rhodesia) was colonized as an outgrowth of interests and activities further south. The extension of company rule rendered Zambia a copper-supplying outpost of the southern African economy. Despite historical ties with East and Central Africa, Zambia was turned southwards. Transport ran south to the coast. Imports and exports passed through Zambia's southern neighbors. Manufactures, skilled labor, capital, and even agricultural produce were imported from the south. Settler efforts to control the whole region and a lack of advanced education and training for Zambians delayed Zambia's independence.

While research on other African countries has progressed beyond the general country-level studies common earlier to analyses of localities, regions, elites, development strategies, and so on, this collection remains in the panoramic sweep genre, attempting to review politics in Zambia as a whole. In its sweep, this volume is very well informed and for the most part carefully researched. Its authors did all extensive research in Zambia; most taught at the University of Zambia. Their work is as much informed by systematic data-gathering as by the more informal collection of information possible only for sensitive residents.

Particularly strong is the treatment of cleavage and conflict. Molteno carefully documents the politicization of regional, religious, and ethnic ties and skillfully relates the sectional base of politics to issues of power and control. He returns to this theme in several subsequent chapters.

The authors have documented in Zambia a number of general findings on Africa. Rasmussen's decolonization chapter stresses the local base of anti-colonial grievances and leadership, even in relatively urbanized and mobile Northern Rhodesia. Molteno highlights the shifting salience and situational determinants of ethnicity. Gupta's analysis of the mineworkers' union argues that in Zambia, as elsewhere in Africa, the dominant party quickly moves to control potential competitors for power, even former pillars of support.

This volume is much better integrated than is commonly the case for collected works, perhaps because Tordoff and Mosteno have co-authored several of the chapters. Major threads are woven throughout the book; later chapters pick up the discussion of issues raised earlier.

Unfortunately, however, the institutional approach of the papers in this volume, and their focus on "constraints" as the major explanatory variable, prevent the authors from progressing beyond a solid political history.

Tordoff's and Scott's discussion of parties is largely a detailed description of origins and electoral behavior. But that behavior is never systematically related to underlying structural features of the situation (or to other potential explanatory factors). The social origins of national leaders help explain their values, strategies, and tactics. The pervasive influence of South Africa not only constrained but directly determined the definition of key issues. One result of this narrow scope is that nowhere in the volume is the dominant party, UNIP, subjected to the searching analysis its role requires.

Tordoff and Molteno locate administrative difficulties in the structure and (lack of) training of the bureaucracy. But if an institution's structure provides an explanation for its behavior, then we must know why that pattern of organization persists. Is elite socialization the key? Does the explanation lie in the interplay of power centers? Is a bureaucratic bourgeoisie an independent actor? Are foreign influences the determining ones? Trapped in their institutional orientation and in a bureaucratic civil service model, the authors are unwilling to consider seriously a politicized administration, a direction pursued in neighboring Tanzania not inconsistent with Zambia's socialism.

Ideology is treated almost entirely in terms of public statements and official writings. But since the craft of politicians involves the manipulation of symbols and the differential presentation of ideology to different audiences, that cannot take us very far. What is required is

a study of ideology as manifested in specific actions, particularly where ideology is itself a tool in the struggle for power. In Zambia, where public leaders are quick to resort to humanism, the official ideology, to condemn opponents, power lies in the ability to define humanism.

Contributors frequently refer to large undifferentiated categories. But surely an analysis is very limited that does not go beyond references to the "government." President Kaunda, public officials, party leaders, and members of Parliament are all the "government," but clearly their interests do not always coincide. Is the "government's" behavior a reflection of conflict between the president and party leaders? Or, of a tacit agreement among certain provincial leaders? or, of a rare assertion of parliamentary authority? Similarly for "Zambians." Although different segments of the population are mentioned and occasionally described-mineworkers, cash-crop farmers, lower-level party officials, aspiring Zambian industrialists-rarely are those differentiations incorporated into the analysis. To what extent, for example, are ostensibly regional conflicts in fact rooted in the differing interests of rural cash-crop farmers and urban mineworkers?

One result of this institutional approach is that major insights remain undeveloped. Molteno's and Tordoff's conclusion drops a perceptive discussion of nascent class conflict in deference to a favorite concept of Zambian leaders: "indiscipline." A second result is that the volume is replete with the moralizing and admonitions of the authors, all too often patronizing and condescending. Finally, a third result is that there is no coherent discussion of development strategy, a very political issue in Zambia. Perhaps some of these gaps will be filled in the promised sequel volume.

Hence, students of Zambian politics are left with the contradictory images. In 1974-75, President Kaunda pursued diplomatic interchange and dialogue with South Africa, a reversal of the dissociation policy reiterated over the past several years. Why? Because of the constraints imposed by Zambia's position in southern Africa? Or because of the efforts of national elites to expand their economic roles and remain in power? Since its independence Zambia has not only failed to reduce its dependence on copper but as well has suffered a drop in agricultural production, policies to the contrary notwithstanding. Has Zambia in fact pursued a viable development strategy intended to maximize earning foreign exchange in order to construct a more solid and selfdirected economy? Or, rather, have foreign influences and leaders' perceptions of national and self-interest led Zambia to pursue a strategy of further integration into the world capitalist economy as a peripheral, dependent state?

Tordoff's well-informed, reasonably well-integrated survey (to its credit, available at reduced price in Zambia) can provide only a foundation for a more penetrating analysis. The realities of the current situation cry for much more

JOEL SAMOFF

University of Michigan

Public Health in the People's Republic of China. Edited by Myron E. Wegman, Tsung-yi Lin, and Elizabeth F. Purcell. (New York: Josiah Macy, Jr. Foundation, 1973. Pp. 354. \$7.50.)

Medicine and Society in China. Edited by John Z. Bowers and Elizabeth F. Purcell. (New York: Josiah Macy, Jr. Foundation, 1974. Pp. 176. \$7.50.)

These two works are the result of interdisciplinary joint conferences held by the Josiah Macy, Jr. Foundation in cooperation with other institutions. Public Health in the People's Republic of China represents a 1972 conference sponsored by the Macy Foundation and the School of Public Health and Center for Chinese Studies of the University of Michigan. Medicine and Society in China is based on a somewhat more recent (1973) forum convened by the Foundation jointly with the National Library of Medicine. Both books are published as part of the Macy Foundation Series on Medicine and Public Health in China.

As the list of contributors to these volumes runs to some 20 authors, I will not attempt to mention all the papers individually. Rather, a brief sketch of each book will be followed by a few comments on some specific selections, which were chosen only to give the reader some idea as to the range and variety of contents in the two volumes.

Public Health in the People's Republic of China begins with a historical overview of the specifically Chinese background to public health problems. These first papers outline the relevant sociological features—the awesome socioeconomic gulf between privileged and deprived classes in traditional China, and scope of parasitological problems, and social obstacles to the introduction of Western medical techniques. Subsequent chapters are devoted to the policy and strategies of public-health organization in present-day China; social-psychological

aspects of public-health reform and mobilization; the sources, training, and roles of health manpower; the present status of disease control, nutrition, and preventive care; and population planning.

Chu-yuan Cheng's paper, "Health Manpower: Growth and Distribution," points up the difficulties of data collection and interpretation which inevitably tend to impose a conjectural quality on studies of the People's Republic. This problem, which in one form or another is evident in many of the contributions, is explicitly discussed by the editors in several contexts.

An especially valuable feature of this volume is the annotated bibliography, which provides citations and rather detailed abstracts of the accessible literature in Chinese, Japanese, English, and French during the period 1949 to 1971.

Medicine and Society in China begins with a summary outline of Chinese traditional medicine, its theoretical basis, and its preservation in classical texts. The following chapters discuss specific phases in the introduction of Western medicine: early Chinese contacts with Portuguese medical men, cultural problems and the confrontation of Chinese and Western medicine in the nineteenth and early twentieth centuries, the role of American medical educators in China, the development of health services in the People's Republic, and the place of public health as an issue in the Chinese political environment.

Robert Bridgman's "Traditional Chinese Medicine" will supply the non-sinologist reader with some basic notions as to the native Chinese medical tradition and the truly formidable textual, linguistic, and philosophical problems involved in any scholarly study of this material. Carl Nathan's "The Acceptance of Western Medicine in Early 20th Century China" provides some fascinatingly concrete details on popular attitudes and prejudices in the China of several decades ago. This paper and several of the other contributions help to develop insight into specific problem areas in cross-cultural relations, and into the remarkable interrelation of scientific, cultural, and political factors.

The editors might have been more careful in their treatment of Chinese names and terms. In an English-language context, some of the spelling conventions are simply incorrect and should have been avoided (e.g., "K'ong Tsi" for Confucius, "Sze-ma Ts'ien" for Ssu-ma Ch'ien); and no one system is used uniformly throughout the book.

The list of participants to the two conferences reveals the broad interdisciplinary basis

of these volumes. Included are sinologists, physicians, historians, political scientists, sociologists, economists, psychologists, demographers, librarians, and others. The group includes a good many scholars with specialized research experience in Chinese matters; some were able to supplement their material with remarks based on personal travel or residence in the People's Republic.

The only real drawback of both these collections is, in a sense, the very problem to which they are addressed: the frustrating lack of normal information flow between China and the West. Time and again readers will wish a more definite picture could be drawn of the subjects discussed here; but the papers themselves will assure readers that it cannot realistically be done. Given the present state of affairs, as general introductions these books are probably as good as can be had.

LLOYD HAFT

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Electoral Politics in the Indian States—Volume I: The Communist Parties of West Bengal. Edited by Myron Weiner and John Osgood Field. (Delhi: Manohar Book Service, Distributed in the U.S. by South Asia Books, 1974. Pp. xvii + 158. \$9.00.)

This volume represents the first published fruits of the massive Indian elections data analysis project that has been in progress for a number of years at M.I.T. under Myron Weiner and his colleagues. At least three more volumes are to follow this one.

The current study consists of a monograph of 68 pages and an appendix of 90 pages, in which detailed results are given for all elections to the West Bengal Legislative Assembly since 1952. This balance may appear to be a bit short on the analysis side, but in fact 68 pages give the authors ample scope for an exhaustive treatment of their topic, the electoral performance of the two communist parties of West Bengal over the 1967 to 1972 period.

The focus of inquiry is an interesting one for several reasons. First, the time period was one of unprecedented success at electoral politics for the Indian communist parties, both at the polls and in forming ministries at the state level. Second, the context was a most unusual one in that four elections took place in West Bengal within a five-year period, all with the same constituency boundaries (usually these boundaries are changed after every second election); thus all four elections could be compared

directly with each other. Third, the authors bring to their subject an intimate familiarity with Bengali culture and politics; it can reasonably be expected that little of importance in the bewildering world of Bengali radical politics will escape them.

The central theme of the study is the changing electoral fortunes of the Moscow-oriented Communist Party of India (CPI) and the more leftist Communist Party of India-Marxist (CPM), as they moved into and out of alliance with each other and the Indian National Congress. The authors develop a number of useful taxonomies and typologies to sort and classify the various constituencies in the context of the different elections. It is doubtful that anything more in this line could be done that would be of use or interest.

The comparisons are all between parties, elections, and constituencies. The authors did not work with survey data, nor did they include any socioeconomic data in their analysis. They report that when they did use such election-related data as turnout, number of contestants and the like, they found no significant relationships. The study, then, does not relate election behavior with what is outside the electoral universe, but rather focuses on explaining voting patterns in terms of the world of parties, electoral alliances, and the voting results themselves. The statistics employed are those of the percentage table, so the analysis is quite easily understood.

The results of the analysis will not be surprising to students of Indian politics—that the two CP's did better at the polls when acting together than when fighting each other, that when one of them (the CPI) ganged up with the Congress, the effects were hard on the other, and that both but particularly the CPM were able to build up core support areas where they were able to pull in a reasonably steady vote in election after election. It is good to have these findings nailed down, though, in a rigorous quantitative sense.

The authors' ultimate goal is to delineate the normal or "institutionalized" support each of the CP's can expect in a West Bengal election. They find themselves unable to pin down this level of support for any party, largely because the "exact nature of the party's standing with the electorate remains unclear" (p. 27). The answer will lie in more analysis of future contests. "In short, several more elections—and much more research—will be required," before dependable party support can be determined (p. 38). What they seem to be saying implicitly is that underlying the shifts that take place from one election to another, there is a

"normal vote" that each party can count on; that is, there is a basic allegiance toward party on the part of the electorate—or a large part of it, at any rate—from which actual voting results represent deviations. Explaining election behavior would amount to accounting for the deviations.

It would appear to this observer, however, that a "normal election" is a chimera, particularly in the Indian context. There has been much heated debate over whether the "normal election" concept is useful in analyzing American voting history, but for India the evidence would appear heavily against the notion's having any validity, especially in volatile West Bengal and most especially during the period covered by this study. Each election has been in a very real sense sui generis; there really is no "normal election," either behaviorally or as an abstract entity, so it is not a surprise that the authors fail to find the "normal vote."

HARRY W. BLAIR

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Inward Hunger: The Education of a Prime Minister. By Eric Williams. (Chicago: University of Chicago Press, 1971. Pp. 352. \$7.95.)

It is rarely the case that Third World political leaders are able to find the time to write a series of scholarly books which are both informative in a general way as well as instructive as to factors which influence their policy directions. This book by Eric Williams, the Prime Minister of the Caribbean English-speaking state of Trinidad and Tobago, represents a further effort in a series of historical and political works. This work provides a clue as to why this scholar-politician continues to write in spite of the pressures of office which, as recently as 1970, almost culminated in the overthrow of his regime by an army mutiny and mass riots activated by Black Power militants. Dr. Williams remains an academic searching for acceptance and full recognition by the Western academic establishment in spite of having occupied the role of leader of the government of his country's parliamentary democracy for a continuous period since 1956 and the preeminence of being the major architect of nation building in Trinidad in the postwar period.

The book itself is rich with attitudinal and behavioral data with which to grasp the overall process of socialization which shaped the ideological and political development of Caribbean colonial intellectuals in the postwar period. It mirrors the deep assimilation of Western European values as well as the ambivalence and sense of wounded rejection that comes from exposure to the ethnocentricity and double standards of the colonizer.

It reveals the essence of the conservative chatacter of early postwar middle-class nationalism in the Caribbean region, tied as it was to a Western European frame of reference and a deep distrust of the creative culture and capability of the non-European Caribbean masses. Fundamentally, it is a nationalistic perspective that makes power claims on behalf of the intelligentsia but expresses great doubts about the viability of the Caribbean societies in whose name and interest the calls for decolonization were articulated.

Williams reveals himself to be the classic colonial intellectual in the way in which he assumes the mantle of political leadership as maximum teacher and high priest pointing the way forward for the society. He presents no vision of the future society to be built or claim to be articulating the indigenous stirrings of the people of Trinidad and Tobago. Nowhere in his writings is there the semblance of a belief in a Trinidadian or West Indian path to development. Instead, he claims the mantle of teacher and maximum leader to enlighten the masses in the ways of the colonizer. In other words, he becomes by virtue of his socialization an instrument of British upper middle-class cultural imperialism. It is therefore no surprise that this distinguished scholar-politician became the target of attack from the more recent nationalist stirrings generated by the first wave of indigenously-bred middle-class nationalists produced by the University of the West Indies.

One fact that is made clear in this work is that in spite of being a victim of cultural imperialism, Williams has no illusions about the benevolence of the Western metropolitan powers, particularly Great Britain and the United States. Indeed he articulates a view of the need to preserve national and West Indian self-interest in the face of the predatory intentions of the major Western powers which is ideologically consistent with his earlier historical work exposing the economic basis of both slavery and the emancipation movement in the U.K. and expounding on the fascinating thesis of how the slave colonies contributed to early British industrial development. Where Williams falls short is in the failure to follow through on this critique to develop a vaible strategy of development outside of dependency on the major capitalist powers. This partial and incomplete view of the economics of decolonization reveals itself in this work as rooted in his

antisocialist ideological tendency and a view of Trinidad and Tobago and the Caribbean as necessarily tied to the path of Western capitalist development and by that very principle limited to a satellite status vis-à-vis major powers like the United States.

The work is written with a frankness and openness in revealing motives, emotions, and gut feelings that give the reader a close view of the inner forces motivating and influencing Prime Minister Williams at critical points in his academic and political careers. In this respect the book represents a rich source of data on the sociology of Third World political leadership that should be of interest to political scientists who may have no particular specialization in Caribbean political studies.

CARL STONE

University of the West Indies

Population, Politics, and the Future of Southern Asia. Edited by W. Howard Wriggins and James F. Guyot. (New York: Columbia University Press, 1973. Pp. 402. \$8.50, cloth; \$2.95, paper.)

This book is the product of a 1971 conference on population and politics sponsored by Columbia's Southern Asian Institute, with some additional material and updating. The contributors, from several countries and disciplines, agree that population will continue to grow at high rates in most of Southern Asia for the rest of the century; population movement, into. cities and into areas dominated by hostile peoples, will continue to create tensions; demands by the rapidly growing and increasingly "modernized" (the term usually used in the book) youth cohorts will increase and strengthen; and there is very little optimism that governments will be able to achieve the great changes seen as necessary to handle these processes. Thus, the prospects for political stability and successful "modernization" are dim.

As usual, generalization on a more specific level is not achieved. The contributors emphasize the limits of their work, and particularities are evident. Migration in India and Pakistan, described in excellent papers by Shahid Javed Burki and Myron Weiner, differs from migration elsewhere because of the post-Partition upheavals. The famous 1941–51 "notch" in Indonesian age cohorts is unique (and, Nathan Keyfitz demonstrates, partly illusory). The editors attempt in an introductory article to draw the studies together, but the patterns they

identify are so broad and elementary that I was led to wonder what audience they had in mind.

Still, this is a very valuable book, full of information and stimulating suggestions and conclusions. The approaches are various, including analysis of census data, survey research, and nonstatistical reports based on the contributors' experience. Political scientists will find the demographic material useful, and will benefit from cautions about the use of data and the acceptance of popular wisdom. For example, Weiner shows that a significant portion of internal migration in India is composed of women traveling for marital or child-bearing purposes. Sidney Goldstein, finding that twothirds of the population increase of Bangkok during the 1960s was natural, suggests that Bangkok may be an example of a kind of maturation of Third World cities. He also finds that 10 percent of those born in Bangkok lived elsewhere in 1960, which is unexpected. Goldstein and Aporodicio Laquian provide evidence for the proposition that shanty-town dwellers are more likely than not to be satisfied with their living conditions, and often actually prefer shanty-town life to that in low-income housing developments.

Studies of Asian population always provide some chilling illustrations, and there are some good ones here. "Ceylon is often cited for having reduced mortality in a single year by an amount that in Scandinavia took 100 years" (p. 37). Vincent Whitney and Gavin Jones estimate that the world population is growing at a rate of 2.1 percent annually, in contrast with a rate of less than 0.5 percent before the Industrial Revolution. Keyfitz estimates that the provision of modern-type employment for the youths entering urban areas in Java alone would require an annual investment equivalent to one-sixth of the total present national income of Indonesia. Howard Wriggins and C. H. S. Javawardene report that in 1969 Sri Lanka's universities produced 100 arts graduates for every graduate in the agricultural sciences. Several of the authors mention the potential "avalanche" of urban migration that would result from a modest increase in the proportion of rural youth who move to cities.

The papers can be summarized only very selectively. In the first part of the book, dealing with general population trends, Whitney and Jones point out that serious governmental efforts to limit population growth began only in the mid-1960s, and that so far real reductions in growth rates have occurred only among Chinese inhabitants of the region, in Singapore and West Malaysia. They offer tentative predictions of population at the end of the century:

more than a billion in India, 250 million in Indonesia, 75-100 million in the Philippines and Thailand, Goldstein helps to explain the lure of the cities when he estimates that average family income in Bangkok is three times higher than rural family income. Burki uses an ingenious comparison of census data and arrest records to suggest that the erosion of support for Ayub Khan had important roots among migrants from land-owning families in the towns, as opposed to the cities, of West Pakistan. Burki provides illuminating and plausible, if sometimes over-generalized, observations on the differences among cities and among migrant groups. Weiner surveys intercultural migration and "nativist" reaction, finding that hostility to in-comers is almost completely confined in India to middle-class migrants; working-class migrants are ignored (note the difference in Bangladesh). Laquian's paper, mostly a parade of stereotypes about cities and trouble, is the least satisfactory contribution, but it does offer some information about the ethnic basis of Manila street gangs and the attitudes and conditions of slum-dwellers.

The second part of the book concentrates on the burgeoining youth cohorts. Keyfitz shows the extent to which migration is a phenomenon of youth: overwhelmingly, at least in Indonesia, people who move are around the age of 20. He points out, too, that the decine of the rural birth rate in Europe as land became less available has not been paralleled in Java, because in Javanese communal ownership patterns larger families were provided with more land, and also wage labor on sugar plantations was plentifully available during the colonial period. Donald Emmerson draws on limited survey data for some tentative conclusions about differences of attitude beteween Indonesian officials who identify themselves with the revolutionary generation and the present student generation. He argues that these attitudinal differences do not necessarily bode ill for political stability in Indonesia (I found his argument a bit confusing). He also challenges the view that ethnicity is of major importance in student activism. Cora Du Bois admirably summarizes the ills of formal education in India, showing that education is highly valued in the abstract and strongly disliked in practice by everyone involved. It is a hollow system that in nearly every particular works against its own goals. Wriggins and Jayawardene analyze what may be the most surprising event in the region in recent times, the 1971 "youth rebellion" in Sri Lanka. They point to underlying causes (rapid growth of population, stagnation of the economy caused partly by over-spending for welfare in

response to politically expressed demands) and precipitating events (organization of the disaffected in rural areas of unprecedented effectiveness). The paper seems more convincing, though, in its explanation of the improbability of the event than of the event itself. This is not criticism of the authors, who do as well as could be done, but another appearance of the old problem of the difficulty of explaining complex events with limited and questionable data. In a concluding note, Kenneth Thompson wisely stresses the difficulty of dealing with the population problem, observing as an example of its complexity that the difficulties of life in an "isolating and insecure" modern world might increase the importance of the family for many people and encourage the maintenance of the birth rate.

The tone of analysis of the Third World has changed in the past 20 years from heady technocratic confidence to discouragement and even despair. This book provides more documentation of the darkness of the prospects. And yet—these grim conditions have prevailed now for some time, the revolution of rising expectations was identified a quarter of a century ago, the gap between rich and poor grows steadily and visibly. But still most people do not starve and do not die violently; most political leaders enjoy a reasonable longevity in office. Is it possible that our data data and our impressions overlook a human capacity to survive and, if not to prevail, at least to cope?

WILLIAM S. HARDENBERGH

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The Systematization of Russian Government: Social Evolution in the Domestic Administration of Imperial Russia, 1711-1905. By George L. Yaney. (Urbana, Ill.: University of Illinois Press, 1973. Pp. xvi + 430. \$13.50.)

This is a book about government efforts to make contact with and exert some control over Russian society. While this may seem a paradox for an absolute, centralist, autocratic regime, George Yaney comprehensively demonstrates that isolation of capital city society and the central bureaucracy was a critical political problem in Russia and that solutions were forthcoming only with the greatest difficulty. The study's chronological purview extends from 1711, the year of establishment of quasi-representative government. In one sense, therefore, Systematization is a contribution—and an important one—to the small but growing literature on Russian administration which has been heavily influenced to good effect by the Soviet

historian, P. A. Zaionchkovsky. Considered at a merely descriptive historical level, Yaney's effort is impressive. Students of administrative development and the history of Russian politics will find new material on both the central and local governments. For, although Yaney has not made the imaginative and extensive use of archives characteristic of S. F. Starr's Decentralization and Self-Government in Russia, in my opinion he more than makes up for this by what appears to be total command of printed and microfilmed material-works by Russian scholars from the eighteenth century through the 1960s; works, including unpublished doctoral dissertations, by Western scholars; the relevant legislation, "official" histories, and so on. If one wishes to understand the basic structure and operation of central and local Russian government, especially in the nineteenth century, a careful, patient reading of Yaney's chapters 5 through 9 will be rewarding.

In addition to being a historical description of the "evolution" (Yaney's word) of Russian government, Systematization is a study of development administration which is quite remarkable in its breadth and depth of perception. The significance of the author's perspective is manifest: "The Imperial Russian Government was the first in history to attempt-albeit inadvertently-to introduce radical social change into a backward society. Its servitors were the first men to sense the necessity of forcibly uprooting social order to preserve political order" (p. 398). The fact that the imperial servitors failed in their effort has, for too long, obscured from many historians and students of political development the more important fact that their effort gave scope and character to the development of Russian government, literally for centuries. Mr. Yaney's capacity to give voice to this phenomenon, to describe and analyze it, has created a work which is conceptually self-conscious (still unusual for the work of a historian) methodologically imaginative, as well as rich and provocative in its language. This effort gives much added interest to the book but, inevitably perhaps, it has resulted in a prolixity which has made Systematization highly controversial since its appearance.

Yaney's notion of the evolution of domestic administration is dialectical. That is, he is constantly at pains to identify, describe, and explain the significance of the nodal point at which the political-administrative ideal is organized into contact with society. Contact creates, in turn, a synthesis of social-administrative interaction which works in spite of the fact that no one predicted it and no one really

understands it. In his effort to explain the behavior of individuals in the context of this evolutionary process, Yaney makes effective use of the notion of myth, which he understands in Benedict's sense as "a wishful projection of a universe of will and intention.... Again and again Yaney notes that both observers and participants in the evolution of Russian administration have been handicapped by distorted vision of the Russian historical experience. "No regime in Russia should be measured against the liturgical formulas that its servitors and supporters repeated. Russian government in all eras should be comprehended against a background of the problems with which it had, consciously or unconsciously, to cope" (p. 318). These formulas made up a large part of what Yaney refers to as capital-city government's myths about Russian politics and administration. His point is important and valid, I believe, for Russia: to the extent that Russian political theorists and statesmen evaluated policy in terms appropriate to another (Western) culture, they were so far unlikely to evaluate themselves and their society with the necessary degree of realism.

In spite of exertions toward a synthesis of the sociology and politics of administrative development, Systematization underscores rather than erases the difference between alternative disciplinary approaches to the history of social change. Nowhere, it seems to me, is this more clearly evident than in the application of the concept of "system" itself. One searches in vain-either in the text or the large bibliography-for reference to Easton, Deutsch, Forrester, or Parsons. Mr. Yaney may have fallen prey to his own mythos-in this case a peculiar idea about the nature of system in society and politics. For him system appears to be that kind of administrative behavior which is regular enough to be predictable by the development administrator. Thus "systematization" becomes something that can gradually overtake the interactions between state and society, an alternative to some previously existing set of relationships, a characteristic of increasing sociopolitical cohesion, and a kind of bureaucratic-political mobilization of a backward society. Does this argue that traditional behavior of peasant society (as a society and a polity) was nonsystematic? Evidently, in spite of the fact that such a conception would create considerable problems for a social- or opensystems theorist. Such use of a basic concept seems needlessly idiosyncratic.

Systematization is an impressive, provocative work. Inasmuch as it raises fresh questions as readily as it answers others, it cannot be

regarded as definitive. Because it sets out to do so much, its real achievements—in descriptive, synthetic history and in analysis—may be overlooked. Nevertheless, in my opinion the work has established a high standard of historical research in development administration against which any subsequent effort will be measured.

DON KARL ROWNEY

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International Politics

Foreign Policy and Federalism: The Nigerian Experience. A. B. Akinyemi. (Ibadan, Nigeria: Ibadan University Press, 1974. Pp. 217. \$6.00, cloth; \$3.00, paper.)

This short new book is another product of B. J. Dudley and E. U. Essien-Udom's excellent Ibadan social science series and consists of a number of case studies on Nigerian foreign policy from that well-trod ground of the early 1960s. If the material, mostly from the lively Nigerian press, produces no surprises, it offers new insights on problems considered in many other volumes.

Ostensibly Akinyemi's book is a "study of the pressures operating on the government of a federation-Nigeria-on specific issues of foreign policy" (p. 2). If only to shed light on how pressure groups (in this case regional states and parties) influence foreign policy in Third World countries, such an objective would be worthy. The real burden of the book, however, is to affirm that, because of such pressures, "a dynamic foreign policy is almost impossible in a federation" (p. 199). Certainly in the first Nigerian republic such was the case, though one could hardly generalize much beyond, since the pertinent point is less the fact of federation than the fact of the diffusion of power. The USSR is istensibly a federation and has a dynamic foreign policy.

But there is little mention of the fact that pressure groups have either disappeared, or have had to operate in a fundamentally different manner, in Nigeria as in much of the rest of Africa since the early 1960s: the object now is to find favor with military factions, and one does not organize public conferences to do this, as radical students did in 1961 to affect Nigeria's Africa policy. A book such as this one, on the second republic, would have to find a new kind of source.

Akinyemi misses this point because he is really trying to prove another one, not foreshadowed in the introduction. This is to discredit the pragmatists who directed the foreign policy he studies. His evidence leads him to note their success, for example, in creating something of a "Pax Nigeriana" with the founding of the OAU. (This puts it a bit strongly, since Nigeria was left out of the central elite originally running the OAU, though it properly focuses on Nigeria's contribution to the climate of moderation that made possible so much progress in African international relations.)

But Sir Abubakar Tafawa Balewa, the architect of Nigeria's foreign policy in this period, lacked vision, it is argued. He could merely "advance practical solutions" (p. 109). But as Nigeria and Africa were "boiling volcanoes threatening to spew out destructive ashes of revolution" (p. 201), radicalism and idealism, for reasons not specified, were called for. Sir Abubakar was therefore "out of touch with the realities of the Nigerian and African political systems" (p. 210), as the book concludes. Or was he? Those pressures had an outlet, and one is bound to wonder whether the author's rage would not be better directed at leaders who have proved intolerant of those pressure groups that are no longer around to be studied.

One other point. It is heartening to see the initiative in such studies passing from the hands of Western social scientists who once held a monopoly on it, to Africans themselves. It is less encouraging, however, that a book like this, which goes over ground so thoroughly plowed elsewhere, acknowledges so little debt to the writers whose work, on internal evidence, helped him focus his questions. In some cases, sources have been left wholly unmentioned; in others, only the most cursory reference is made—unless, of course, the Western writer is perceived as being wholly in agreement. This makes for little dialogue of a scholarly sort.

W. SCOTT THOMPSON

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The Causes of War. By Geoffrey Blainey. (New York: The Free Press, 1973. Pp. x + 278. \$7.95.)

War Without Weapons: Non-Violence in National Defense. By Anders Boserup and Andrew Mack. (New York: Schocken Books, 1975. Pp. 194. \$6.50.)

These two volumes on war at first are seemingly different, but upon closer examination, turn out to be quite complementary. While the book by Blainey treats the origins of war in the modern period, the Boserup and Mack volume analyzes alternate nonviolent

strategies in response to a frequent consequence of war, namely, invasion by a foreign power and occupation. One analysis is in the mode of explanation; the other is more in the realm of policy formulation in the area of strategy and tactics. As a result, these treatments stand at the two ends of a continuum beginning with the origination of international violence and ending in a major possible response in the form of nonviolence. A point of interest here is that Blainey is an Australian economic historian while Boserup and Mack are European conflict and peace researchers. Both books, then, provide a somewhat different perspective on war from that normally seen by an American social scientist.

The Blainey volume is the more historically oriented of the two, as one would expect, given the greater number of wars historically than nonviolent responses. Most of the instances of war are in the modern period stretching roughly from 1700 to the present. The analyses are not systematic in the sense of formal theory building and hypothesis testing, nor is there a sequential building of a nonquantitative analytical framework. Rather, much of the book examines and then rejects many of the commonplace notions held about the causes of war. For example, the concept of international prosperity as a deterrent to war is effectively treated in the detailing of serious instances of wars which were begun in times of widespread relative prosperity (e.g., World War I). The related expectation of peace under conditions of trade, tourism, and intermingling of cultures is effectively countered not only by the example of World War I, but also perhaps by the American Civil War. Blainey resurrects Macfie's argument that it is in times of relative prosperity and high expectations for the future that wars break out.

Other assumptions attacked by Blainey are that the balance of power leads to peace and that "Pearl Harbors" are all that exceptional in the history of war. Blainey argues rather convincingly that the balance of power is more likely to lead to war, if only that a less decisive condition exists within a state of balance and that a particular country might be tempted to initiate a confrontation. The relatively lengthy peace after the end of a long war in which the victor is shown to be clearly more powerful than the vanquished, serves to illustrate the dangers of the balance. Blainey does an effective job of enumerating the many instances of surprise attacks or declarations of war after the battles had begun, to counter the usual assumption of the rarity of surprise attacks such as Pearl Harbor. Perhaps it was the massivity (and

success) of the Japanese attack rather than its timing which led to the subsequent unsavory reputation. In any event, it is commonplace assumptions such as these which are examined rather effectively by the combination of verbal analysis and use of historical example.

Some surprising and rather useful information is brought forward by this approach. The death-watch-wars, for example, or those begun immediately after the death of a monarch in the eighteenth century, may be an instance of an immediate power loss (uncertainty increase) which leads to the initiation of war at the moment of the monarch's death. This may be another instance of a power loss leading to war, much as an alliance between a major power and a smaller one has been found by this reviewer in his own volume on war to yield a power loss as an uncertainty increase which results in war. In addition to the onset of war, Blainey also deals with the duration and extensity of war. Wars occurring at the power center of Europe are found by him to be longer than the colonial or other shorter wars occurring at the periphery of Europe.

In contrast to the loose, generally anecdotal style of Blainey's book, the Boserup and Mack volume is more self-consciously analytical and systematic. Beginning with a general overview of treatments of both positive and negative forms of nonviolence—the former designed to win over the opponent and the latter aimed simply at thwarting the occupation—Boserup and Mack opt for the latter. The remainder of the book is devoted to the application of several theoretical frameworks to the strategy of nonviolence.

One such framework is that of guerrilla warfare in which the militarily weaker side adopts strategies which avoid the opponent's strengths and aim at its weaknesses. This is said to be one strategic model for nonviolent defense. Group theory is used to show the importance of group cohesion in the effective use of nonviolent tactics against the occupiying force. Clausewitzian strategy is also applied to demonstrate the importance of the defense in any military strategy including nonviolent ones. Indeed, according to Clausewitz, the mode of defense will determine the mode of offense. and the authors use this to show how an appropriate nonviolent defense can force the occupying armies to adopt strategies consonant with the aims of the defenders. Examples are drawn from the twentieth-century European experience such as the Ruhrkampf of 1923, the Czechoslovak resistance of 1968, and nonviolent resistance to military coups as in the Kapp Putsch of 1920, or the French generals'

revolt in Algeria in 1961.

However, despite its virtues, there are several weaknesses to this treatment. First, the Clausewitzian emphasis on the defense as a justification for the use and possible success of nonviolent tactics ignores the interactive quality of international warfare. This is especially the case when the occupying power and nonviolent resistance are frequently in conflict over a relatively long period of time. There are frequent "battles" as it were, and this allows invaders to modify their strategy when faced with a temporarily successful instance of nonviolent defense. Perhaps this explains why the Czechoslovak resistance ultimately foundered, even after a successful initial high level of cohesion among the Czechoslovaks. By the making of apparent concessions and the establishment of alternate sources of legitimacy within the country, the Soviets managed to defeat the resistance within a relatively short time. It is precisely because of the time allowed the invader to engage in prolonged interaction with the nonviolent resistance on the occupier's own terms that foreshadows an eventual demise even for an initially successful effort.

Another problem is the almost exclusively European domain of the analysis. The authors are heavily influenced by the events of the Second World War and the recent Czechoslovak invation and occupation. There is little if any treatment of resistance to colonial authorities in the now independent Third-World Nations, or responses to military coups in these countries.

Despite this emphasis on the recent European experience, there is a surprising and notable omission from the overall treatment. This is the assumption concerning the willingness of the occupying power to subscribe to certain moral standards of conduct. If, as in the case of the Nazis during World War II, the occupying power is willing to engage in the destruction of entire population groups such as the Jews or to destroy whole villages as in Czechoslovakia, then the nonviolent enterprise obviously fails.

In comparing this volume on nonviolence with the Blainey treatment of the origins of war, one arrives at a somewhat anomalous conclusion. While the Boserup and Mack book is much more theoretical, analytical, and conceptually more systematic, the Blainey volume seems to have a more solid set of conclusions. Indeed, these are included in a separate concluding chapter which can serve as a set of hypotheses for future research. One wonders why a book which is essentially historical-anecdotal and much thinner analytically would

arrive at a more substantial set of findings than a more systematic one. The answer would seem to lie in the Problem of N. There have been all too many instances of war in the past two to three centuries, while the cases of nonviolent defense really have only occurred with some regularity in the twentieth century with the rise of large-scale civilian participation in war. As a result, an economic historian such as Blainey who obviously has immersed himself in the various histories of war would have a much larger data base to work with. Boserup and Mack had to rely on a few contemporary illustrations and some considerable theorizing to arrive at their set of conclusions.

Because of the use of a largely theoretical social scientific approach, most American social scientists probably would be more comfortable with the Boserup and Mack treatment. Their concern with a systematic development and the extensive use of concepts such as legitimacy, polarization, and behavioral contagion, lend a more familiar quality to their analysis. Blainey, on the other hand, hardly uses concepts such as these and, in addition, the use of chapter headings such as "Paradise is a Bazaar" or "While Waterbirds Fight" seems to be a rather unnecessary literary indulgence. Nevertheless, the extensity of Blainey's familiarity with instances of war and some little-known (in America) but intriguing observations on war, make the book worthwhile reading.

MANUS I. MIDLARSKY

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In Search of American Foreign Policy: The Human Uses of Power. By Lincoln P. Bloomfield. (New York: Oxford University Press, 1974. Pp. 173. \$6.95, cloth; \$1.95, paper.)

The author sees himself standing with one foot in the camp of American foreign policy makers and with the other in the camp of their critics. The critics are often wrong, but the notion that U.S. policy is not inspired by the "human perspective" has merit. The policy makers, he tells us, mean well and possess qualities of mind and character. Still, Bloomfield's disquisition conveys the impression that American foreign policy is continuously out of date and wrapped up in demagoguery. Intelligence estimates are mishandled and policy tends to be based on "best case" hopes, Furthermore, the State Department's longrange planning functions have been curtailed drastically.

I am afraid the roots of the disaster run deeper. Methodological ignorance of what should be considered—that is, reductionism—has been a potent cause of our troubles. The State Department, the Pentagon, the National Security Council, and the White House are continuously operating on images of truncated reality.

Bloomfield himself is an accomplished practitioner of the reductionist art. For example, he argues military power is relevant • only to "some" national strategies: but who would fight international inflation with tanks? He asserts that the Soviets define the protracted conflict with the United States as not "primarily military." But what role does their conflict doctrine assign to military power? He thinks the risk of nuclear war cannot be tolerated indefinitely, but can the risk of the human condition itself?

Bloomfield talks about arms reduction in terms of naive variations on the overkill theme, somewhat like discussing modern medicine as though microbes and microscopes did not exist. He refers to the need for "agreed verification procedures," but hides that the Soviets refuse even token verification. He proposes unilateral American steps to get arms reduction moving but forgets that the United States already has been following this prescription with painful effects.

Bloomfield knows the USSR is America's foremost security problem. Yet he treats this subject with massive doses of reductionism, Analysis of the USSR is a function of the analyst's position in U.S. internal politics. The sovietologists know "too much" (p. 69). Threat assessments are mutually influenced by mirror images propounded by rightwingers in the United States and "ideological purists" in the USSR. That the leading Soviet "purists" belong to the USSR's top leadership group and that purism has marked the announced policies of the CPSU for nearly 60 years remains as unmentioned as the fact that right-wing analysts hold no power in the U.S.

After minimizing the Soviet threat and dismissing those who would describe it realistically, Bloomfield avers that the Soviet regime is "blindered," "terrified by open criticism, ... brainwashed by its own propaganda, and ... hopelessly immobile," "caught in the dilemma of wanting to loosen up but not knowing how, ... fearing above all to lose control over its empire," and trying with all its might and skill to avoid "all-out war and all-out peace." That the Kremlin communists also work effectively for the world-wide victory of Communism is

eliminated from this caricature of bewildered men.

"Common values are largely absent" in the U.S.-USSR relationship. Yet the U.S. "must work hard toward better relations." "Common values will flow from shared experience" and may result in "feelings of community." Together with the USSR, we should establish "spheres of abstention" all over the world and "shape structures and relations that provide a framework for evolution." Bloomfield evidently believes in politics as the art of the impossible.

• "Common interest abound." Common interests of the U.S. with whom? With the kolkhozniki or the industrial managers? The ruling purists in the politboro or the Young Turks in the Central Committee? Or of presidential candidates with Messrs. Andropov, Grechko, Ponomarev, and Brezhnev?

Pending the emergence of a Soviet policy of abstention, the U.S. must not prop up "undemocratic tyrants" within its spheres of influence. America likes to bed down with purveyors of snake oil democracy, such as the Greek colonels, Haile Selassie, and Ayub Khan, and supports countries whose last "free election" Bloomfield can't remember. While waiting for free elections and the overthrow of "undemocratic tyrants," should we ignore the Arab world, and most of the governments of black Africa, Latin America, and Asia? And if tyrants are off limits, how can we develop common interests and improve relations with the worst of the species? Will the tyrants in the Kremlin help us to humanize the use of power?

"The words of liberalism remain, but the spirit was betrayed by both men and events." What is this liberalism which can't be fitted to the facts? It is support of "change, non-violence, and rational discourse." Change isn't always improvement; nonviolence rarely stops aggression; and rational discourse seldom is relevant in political negotiations. So why are those liberal goals?

To rescue liberalism Bloomfield proposes a "reinvigorated" approach. The following "value premise" should override all interests "except actual survival": "Human beings who live in this country—and for that matter everywhere—represent the irrefragably highest value for American policy." Indeed, in Soviet-ruled Eastern Europe the U.S. sought stability "far too often at the price of democracy or human rights."

This premise is very persuasive. But U.S. policy can be humane only to the extent that it promotes the full enjoyment by all of human, civil, and political rights wherever those rights

are most harshly repressed. If Bloomfield were serious and consistent, he would argue that human rights within the USSR should be a primary and obligatory U.S. concern.

At this crucial point, the reinvigoration runs out of vigor and the goal disappears in the fog. Instead, Bloomfield makes the priceless recommendation that "a reformed foreign policy" should "design around obstacles." In other words, the U.S. must not face up to reality. As Shelley said: "From the contagion of the world's slow stain, he is secure, and now can never mourn... a head grown grey in vain."

STEFAN T. POSSONY

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The United States and India, Pakistan, Bangladesh. By W. Norman Brown. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 462. \$16.00.)

This third edition of the late Norman Brown's well-known study was updated to take account of the bloody emergence of Bangladesh as an independent state. The overall format is as before, with considerable emphasis upon the historical background to the contemporary scene. The book provides an excellent introduction for college students and others approaching the subject afresh. Because of the wide scope of the survey it necessarily cannot explore any facet in detail. Earlier reviewers praised the 'balance' achieved by Professor Brown in discussing issues which are still very much matters of controversy. This balance is maintained in the last revision; indeed, Professor Brown's approach, while always exact, was also unusually restrained.

The appearance of this work in a series entitled "The American Foreign Policy Library" always seemed somewhat fortuitous, and the last chapter on relations between the three South Asian countries and the United States serves to demonstrate how marginal and peripheral that relationship has been in the totality of American foreign policy. Norman Brown was concerned to emphasize American scholarly interests in the area, and their growth since the 1950s. Perhaps his manuscript was completed before it became glaringly obvious that these studies may be past their zenith in all but a few American universities. His last word was to warn against viewing South Asia from the United States in terms of American values and American interests. Americans should "take a genuine and intelligent interest in the people of the subcontinent for their intrinsic human worth rather than as so many millions

of bodies to stand in support of American international policies." Professor Brown always sounded a humane, tolerant, liberal note. It deserves to be recalled when those values are fast fading from the South Asian (and the world) scene.

HUGH TINKER

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Pakistan's Foreign Policy: An Historical Analysis. By S. M. Burke. (London: Oxford University Press, 1973. Pp. 432. £6.30.)

Mainsprings of Indian and Pakistani Foreign Policies. By S. M. Burke. (Minneapolis: University of Minnesota Press, 1974. Pp. 308. Price not listed.)

These two volumes are the fruits of several years of research and reflection by a Pakistani administrator-diplomat-scholar who had had a long and distinguished career as a member of the Indian Civil Service, a senior Pakistani diplomat, and a professor of South Asian studies at a leading American university. Pakistan's Foreign Policy is by far the best volume on this important subject. Mainsprings of Indian and Pakistani Foreign Policies presents, in a more abbreviated way, much the same material relating to Pakistan, continues the story to 1974, and reviews the course of India's foreign policy, especially vis-à-vis Pakistan, as well. While the reader will have no difficulty in identifying the nationality of the author, the treatment of India is remarkably balanced, with some lapses. Compared, however, with the treatment of Pakistan's foreign policy, and especially of Pakistani views on foreign affairs, the material on Indian foreign policy is sketchy and rather superficial.

Each volume begins with a brief survey of the historical background, with emphasis on the "two closed systems" of Hinduism and Islam and on the "unbridgeable gulf" between them. The bulk of each volume consists of an essentially historical narrative. Three phases of Pakistan's foreign policy are discerned: the "nonaligned years" (1947-1953), the "aligned years" (1954-1962), and a period of reappraisal (1963-1970). Three phases for the combined discussion of the foreign policies of India and Pakistan are also described, labeled "the first moves" (1947-1953), "panchsheel vs. defense alliances" (1954-1958), and "warclouds over South Asia" (1959-1974), centering first around the Sino-Indian conflict and then around the Indo-Pakistani conflict.

The main theme in each volume is Indo-Pakistan relations. Important secondary themes are also discussed at some length, including Pakistan's relations with other Muslim countries, India's nonalignment, Pakistan's policy of "bilateralism" in recent years, and the relations of both India and Pakistan with the United States, the Soviet Union, and China. The 1971 crisis in South Asia is treated in a brief postscript in Pakistan's Foreign Policy, and more fully, but just as gingerly, in Mainsprings. The much-criticized "tilt" toward Pakistan of the Nixon administration is characterized as a "realistic handling of the South Asian conflagration."

As historical narrative the two volumes deserve high praise. They are, however, deficient in many aspects which many contemporary students of foreign policy regard as essential, such as the role of ideology (especially relevant in the case of Pakistan), geopolitical factors, linkages between domestic politics and foreign policy, decision making, the organization of the government for the conduct of foreign policy, interest groups, elites and the quality of leadership, styles in foreign policy, and economic aspects. Mainsprings does contain a chapter on Jawaharlal Nehru, stressing his "love for Kashmir" and "his other biases." Surprisingly, there is no parallel discussion of Jinnah or any other Pakistani leader, perhaps because no Pakistani leaders gave as much attention to foreign affairs or was as well known in international circles. If Professor Burke were revising this book today, he might find reason for singling out both Indira Gandhi and Zulfikar A. Bhutto for special attention for their leadership in foreign as well as in domestic policy.

In the final chapter of Mainsprings Professor Burke departs from his historical analysis to speculate on larger aspects of the experience of India and Pakistan since independence. He suggests that both countries have belied "high expectations" and have been no less timid than their foreign predecessors "in tackling socioreligious reforms." He offers some advice to the great powers, and raises a poignantly timely question about the prospects for democracy in India and Pakistan. "The great powers," he writes, "... can help India, Pakistan, and Bangladesh best by ceasing to treat the subcontinent as a part of the arena where they measure their respective capacities for world hegemony and, instead, join hands for the constructive purpose of assisting the three neighbors to compose their differences on a fair and enduring basis" (p. 243). The lessons of history, he points out, reveal that "no modern nation has achieved real democracy before attaining prosperity and literacy," both of

which are conspicuously lacking in South Asia. "Democracy seems to be the delectable reward for successfully tackling the grim toils of national construction. Have India, Pakistan, and Bangladesh the capacity to reverse this established sequence?" (p. 242)

NORMAN D. PALMER

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Social Policy of the European Economic Community. By Doreen Collins. (New York: *Halsted Press, 1975. Pp. ix + 286. \$19.75.)

This book on the evolution of social policy in the European Community is strictly for the specialist. It is, in the author's own words, "essentially descriptive of the social tasks faced during the years from 1958 to 1972" (p. 39). The study is informed by a humane sense of the importance of social policy for the lives of European workers. It is also a comprehensive and well-balanced piece of research. In the final analysis, however, I found the book disappointing, because it is neither analytically revealing, nor of much policy interest beyond its wealth of descriptive detail.

The two parts of the book describe, respectively, the application of discrete portions of the treaty dealing with particular aspects of social policy (employment, wages and working conditions, free movement, and social services) and the more general efforts to harmonize national social policies and/or develop a distinctive European social policy. This approach is reasonably serviceable for the author's descriptive purposes, although it does lead to a certain amount of repetition as substantive policy problems reappear in different contexts. While on editorial matters, it should be pointed out that the writing is rather cumbersome and wordy, with a good many sentences of forbidding length and complex structure. The book concludes with a sober but not gloomy assessment of the Community's accomplishments and with a kind of agenda for the future.

The basic argument developed by Collins is that, while progress on social policy has been unsatisfactory, the Community has recorded some notable achievements. In addition to isolated successes (on free movement of wage earners across national borders, for example), she points to commission-sponsored studies which have provided a reliable data base and set the stage for extensive exchanges of views among interested parties. "Such work is not only the pre-condition for any effective development of social harmonization but is a con-

tribution to a fundamental tenet of social policy, namely that a rational base for action may be established through greater knowledge and understanding" (p. 221). Collins is also pleased by recent signs that the Community is prepared to cease thinking about social policy as merely "a compensatory mechanism for damage done" as a result of economic integration (p. 226). She detects, instead, an increased willingness to see the human aspects of social policy as ends in themselves. Only this humane approach, Collins contends, is compatible with the Community's more sweeping aspirations:

If it [the Community] is to level off on the plateau of a free trade area ... its social activities will find it difficult to be more than the marginal adjustments made necessary to cushion the changes its creation has brought. If, however, the Community is concerned to achieve political unity, it is inconceivable that it could operate without acceptance of the social functions of modern government which both express and give deeper meaning to the human group from which it comes (p. 248).

The main stumbling blocks to more rapid progress on social policy have been, as Collins sees it, the national governments. First, they failed to provide a clear enough mandate in the treaty and, subsequently, they have consistently thwarted the many initiatives undertaken by the Community's executive, the commission. The author is additionally mindful of other obstacles like the technical complexity of these matters, their sensitive ideological implications, and the uncertain melange of human aspirations attaching to social policy. In the end, however, the commission and particularly Commissioner Levi-Sandri emerge as the heroes and the member-states as the principal villains in this presentation.

I do not wish to take issue with these conclusions but would suggest that they do not take us very far. No serious effort is made, for example, to explain why success has proven so elusive in social policy while things have gone forward quite well in other matters. Even the standards employed by Collins for judging success and failure are open to question, because she does not consistently distinguish between formal agreement at the European level and actual policy implementation by the Community and/or the national governments. These analytic problems stem, I think, from the basic research plan which focuses almost exclusively on documentary sources. Typically, analysis begins with the relevant treaty provisions, deals next with commission proposals for carrying out treaty obligations, goes on to mention consultation with various bodies and the statements made by parties to the negotiations, and concludes with the final disposition by the Council of Ministers. Since virtually all of the research seems to come from documents and other more or less public sources, we never get behind the scenes to the forces that influence the positions taken in negotiations. When judgments are occasionally made about the relative importance of various factors in determining the final resolution, these judgments tend to be ad hoc—that is, without sufficient attention to systematic evaluation of the evidence.

My criticisms of this book, thus, go to its lack of conceptual rigor and its failure to penetrate the surface of policy analysis. In fairness to the author, I must repeat that she acknowledges the essentially descriptive character of her project. At that level the book succeeds well enough—being carefully researched and judiciously presented. For the reader trying to learn something about Community policy processes or about the politics of social policy in Western Europe, however, the book will not be very satisfying.

STUART A. SCHEINGOLD

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Public Papers of the Secretaries-General of the United Nations. Volume 4: 1958—1960, Dag Hammarskjold. Edited by Andrew W. Cordier and Wilder Foote. (New York: Columbia University Press, 1974. Pp. 629. \$22.50.)

The publication of Volume 4 of the Public Papers of the Secretaries-General of the United Nations, which covers the period 1958–1960 during the Dag Hammarskjold years, evokes a remembrance of happier times for the United Nations and its chief executive officer. For the papers in this volume represent the work of a secretary-general at the very peak of his personal prestige and influence, and at the pinnacle of the historical development of the office itself as an actor of some significance in international relations.

The political growth of the office of the secretary-general is well known. Begun in 1920 at the League of Nations as an essentially administrative function under Sir Eric Drummond of Great Britain, the office was quickly politicized. Sir Eric's position as a neutral behind-the-scenes adviser and mediator for governments in dispute was highly valued, primarily because of his skilled and imaginative diplomacy. From 1920 to 1933, Drummond built up the international civil service he had

created, while simultaneously demonstrating that the office of secretary-general could be of political value to the world community, although without visibility or public notice of any kind.

Drummond's successor as secretary-general, Joseph Avenol of France, made no further contribution to the development of the office during his tenure from 1933 to 1940. Indeed Avenol weakened the structure well before the League collapsed in 1939—40. Avenol's sympathies for Germany and Italy were quite sufficient to reduce his influence, and his frequent indolence served to diminish the office still further. Sean Lester of Ireland, the League's third and last secretary-general, administered a caretaker operation in Geneva from 1940 to 1947.

Trygve Lie of Norway, the U.N. secretarygeneral from 1946 to 1953, continued the behind-the-scenes advisory and mediation functions of the Drummond years, but he further politicized the office by adding a more public dimension to the secretary-general's activities. Lie spoke out frequently before the General. Assembly and Security Council on political matters; he took public stands on issues of critical importance to the international community, including the superpowers; he negotiated preferred positions in the public spotlight. In brief, he generated a highly visible political role for the secretary-general, while maintaining the international civil service created by Drummond.

Dag Hammarskjold, the brilliant Swedish economist and civil servant, developed the political role of the office to its highest point during his administration from 1953 until his death in 1961. Hammarskjold maintained the Drummond and Lie functions, but went further. His carefully constructed theory of the UN's proper role as a neutral intermediary with interests and policies somewhat independent of the great powers led to a conception of the office as an instrument with certain discretionary powers over significant U.N. activities, including peacekeeping forces, observation teams, and other instances of a U.N. presence between or among powers in dispute. He insisted upon and for a time gained acceptance for the notion that secretary-generals had an independent capacity, and might act on their own initiative even without instructions from the U.N.'s political organs.

Neither U Thant (1961-1971) nor Kurt Waldheim (1971-present) have been able to do more than hold the line where Lie had left it. Soviet and French resistance to the Hammarskjold conception, as well as the rapidly

fading U.N. role in world politics, have combined to retract the office to more modest dimensions.

The 1958 to 1960 period covered by volume 4 of the *Public Papers* represents the peak just prior to the Congo affair in the early 1960s which finally resulted in the insistence by the Soviet Union, maintained ever since, that the secretary-general's role be essentially that of chief administrative clerk of the organization. In today's United Nations, perceived and utilized more frequently as an instrument of political pressure than as a neutral aide to dispute settlement, a minimalist conception of the office has supplanted the activist role of Hammarskjold.

Volume 4 of the Public Papers is a meticulously edited book containing speeches, press conference transcripts, diplomatic communications, official statements and other records detailing the work and views of Dag Hammarskjold from early 1958 until mid-1960 and the start of the Congo crisis. The period was marked by Hammarskjold's successful intervention in the 1958 Lebanon-Jordan affair, which included his enlarging the U.N. Observation Group in Lebanon despite a Soviet veto of a Japanese proposal that would have had the Security Council expand the mission. In 1959 Hammarskjold made a personal trip to Laos and installed a personal representative there whose real task, despite a different public explanation, was to assist in the neutralization of the country. These moves were a prelude, of course, to his crucial role in the Congo from mid-1960 until his death in September 1961.

Volume 4 is a valuable addition to the store of historical data on a key figure in the quest for a peaceful world order.

ARTHUR W. ROVINE

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Soviet Policy Toward India: Ideology and Strategy. By Robert H. Donaldson. (Cambridge, Mass.: Harvard University Press, 1974. Pp. x + 338. \$15.00.)

One of the Soviet Union's most significant foreign policy successes of late has been its relations with India. This study by Robert Donaldson is a detailed analysis of the Soviet background to that success in the sense that it depicts the transformation of Soviet policy toward India from a policy mired in dogma to one grounded in a far more flexible conceptualization of India and of the Soviet role in South Asia.

It should be stated at the outset that Donaldson's study is less diplomatic history than intellectual history, for Donaldson is not prepared to dismiss—as so many have—ideology as merely a means of justifying any and all actions pursued by the Soviet Union. Donaldson sees a far more subtle, more complex relationship between ideology and strategy, one in which ideology—while admittedly often bent to accommodate the needs of strategy—nevertheless poses conceptual challenges to and limitations on Soviet strategy makers.

As the author views the evolution of Soviet policy toward India, that policy has gradually undergone a "de-eschatology" in its concern to adapt to a rapidly changing international power structure. In the Leninist and Stalinist periods, India was largely beyond the focus of Soviet foreign policy-although the Soviets were more aware of India and its problems than they were of other areas of Africa and Asia. As Donaldson shows, the problem for the Soviets during this period was not simply a lack of knowledge about India but, more importantly, a conceptual rigidity which caused policy makers to distort what information they had as well as their decisions as to the data they thought they needed. The result was a foreign policy which neither supported Soviet interests in South Asia nor assisted the indigenous Communist movement in India.

Soviet foreign policy toward India radically changed between 1955 and 1961, a change that Donaldson depicts in great detail. This transformation had little to do with conditions in India. Rather, Soviet attitudes toward India shifted as the Soviets began seeking Indian support for the intensifying "anti-imperialist" posture of Soviet foreign policy. Soviet willingness to accommodate the Nehru government grew as the USSR found itself in competition not only with the West but with China as well.

Donaldson's contribution lies less in his illumination of Soviet policy toward India than in his effort to describe some of the consequences of ideology for the policy makers themselves. To be sure, ideology was bent to strategic needs, but it also functioned to screen out useful information from Soviet academicians who were knowledgeable about India but forced to shift with domestic ideological winds. The relatively recent decisions to give researchers increasingly free rein to experiment with Marxist categories as they applied to Asia may have been brought about by the need of policy makers for new justifications. However, what the policy makers ultimately received was far more-a more flexible base upon which to build a foreign policy.

For all of its positive attributes, the Donaldson book is somewhat limited by its focus. Its concern with the relationship between Marxist-Leninist doctrine and Soviet Foreign policy winds up telling us more about changing doctrine than changing policies. It is easy to see, after reading Donaldson's book, how the interpreters of doctrine and policy makers in the Soviet Union have been influenced to change their perception of reality and how they have gained flexibility as a result. Far more difficult to understand is, having achieved this flexibility, how Soviet foreign policy makers come to make the choices that they do. It is useful to know that the Soviets narrowly missed failure in their India policy because of Marxist constraints. It would be equally useful to have Donaldson's explanation for the generally correct choices the Soviets have made in their dealings with India as of late.

Overall, Donaldson's study is a good one and will be of interest not only to students of Soviet foreign policy and of India but also to all those interested in Soviet social science. In this detailed scrutiny of Soviet academic writing on India—and of the pressure on Soviet academicians—Donaldson has given us insight not only into Soviet foreign policy but also into the problems of doing political science in the USSR.

GERALD A. HEEGER

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Beyond Dependency: The Developing World Speaks Out. Edited by Guy F. Erb and Valeriana Kallab. (Washington, D.C.: Overseas Development Council, 1975. Pp. xii + 238. \$3.95, paper.)

This is a collection of ten articles and seven official and nongovernmental statements dealing mostly with economic and political relations between the developing and the developed countries. According to the introduction by Overseas Development Council President James P. Grant, this is meant to be a companion volume to the ODC's Agenda for Action series, and its principal purpose is "to ensure that the views of those in developing countries are heard more widely in the United States." Therefore, Mr. Grant hastens to add, the book includes only one article by a developed-country contributor, namely, Guy F. Erb.

As it turns out, that is too bad; Mr. Erb's paper is by far the best of them all—and one of the few that makes some sense. Although ninth in the contents, Erb's paper ("The Developing

World's 'Challenge' in Perspective") provides a balanced and concise explanation of what changes have been taking place on the international scene, what is right and what is wrong with the "theory" of dependency, what it is that the developed countries have failed to do, what would be the costs of increased confrontation for both developed and developing countries, and what are some of the realistic choices before the world community.

Then there are two papers that though narrower in scope, do deliver something. The article by Krishna Roy ("Population Policy from the Southern Perspective") is a neat outline of prevalent views on population planning in the developing world and a useful survey of what is being done about the issue in Africa, Asia, and Latin America. The paper by Bension Varon ("Ocean Issues on the International Agenda") is a good summary of the beginnings, evolution, complexities, and prospects for an international approach to seabed and other ocean-related problems.

The remaining seven articles can safely be skipped. Three of them deal with international politics, but what is said here has been said better at the United Nations and at other similarly enlightened gatherings: that developing countries want a self-reliant type of development, that greater attention must be placed on insuring that social justice accompanies growth, that social and institutional changes must receive greater priority, that regional arrangements might succeed in doing what world-wide groupings have not, and that petrodollars should be channeled toward the poor countries. I am referring here to the papers by Samuel L. Parmar ("Self-Reliant Development in an 'Interdependent' World"), Soedjatmoko ("Reflections on Nonalignment in the 1970's"), and Ali A. Mazrui ("The New Interdependence"). Two additional articles by Félix Peña and Constantine V. Vaitsos) deal with multinational enterprises. Of course, such enterprises are guilty of everything under the sun. The Vaitsos article is, on this account, particularly vicious. According to him, for instance, foreign investors are a damnable bunch for catering "primarily to high-income consumers," for being uninterested in investing in the areas of health, nutrition, and housing, and, in general, for attracting "scarce local technological capabilities and other resources into areas that do not represent the basic needs of the developing countries." A paper by Mahbub ul Haq outlines some negotiating principles and strategies for the future and, finally, a refreshingly simple article by Soumana Traoré describes an African

experiment in grass-roots development based on as simple a prescription as showing people who they can make a profit by establishing a cooperative.

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The Politics of Trade: The Evolution of the Superbloc. By Douglas Evans. (New York: John Wiley, 1974. Pp. 128. Price not listed.)

• The Politics of Trade purports to be an analysis of the relationship between power politics and a drift toward protectionism in world trade. The book begins with a brief history of modern protectionism, and then proceeds to a chapter ambitiously titled "The Theory of Economic Multipolarity." What is asserted to be theory, however, is not theory in a positive sense, but rather a series of factual assertions about protectionism, economic superblocs (U.S., USSR, European Economic Community, Japan, and China), and multinational corporations. Evans summarizes his theory as follows (pp. 15–16):

The modern industrialized world has become heavily interdependent and rationally should become more interdependent. However, a small number of great powers (superblocs) have arisen... They currently contain protectionist features which are bound to bring the superblocs into conflicts... The inescapable conclusion is that the present structure and tendencies, by accentuating the self-sufficient character of the superblocs and reversing the trend toward interdependence, contain the seeds of economic warfare.

Unfortunately the logical links of the argument are not spelled out in a manner that would make his 'theory' capable of being tested. Indeed Evans himself sees theory as not distinct from evidence. "It must be said at the outset that ... documentation came first.... Without such detailed evidence there could be neither theory nor conclusions" (p. vi). This is not to say that the book is not useful, but the political scientist or economist seeking to find a meaningful theoretical link between economic and political phenomena will not find it in this volume.

Evans' work is better described as a journalistic investigation into recent international economic and political history woven around the theme of economic multipolarity. It is journalistic in that it is largely descriptive, offering neither theoretical insight, verification, or documentation. Such books can, of course, be useful in the library or the classroom on occasion, but the book's chronology ends before such critically important events as the worldwide increase in relative food prices and the successful cartelization of OPEC, thus limiting its potential value as a guide to contemporary international political and economic relations.

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French International Policy Under de Gaulle and Pompidou: The Politics of Grandeur. By Edward A. Kolodziej. (Ithaca: Cornell University Press, 1974. Pp. 618. \$24.50.)

Studies on French foreign policy since 1958 tend to fall into two categories. A large number of studies have focused upon the personality and perspective of Charles de Gaulle and his heirs and their attempt to implement a very special view of the role of France in global politics. Other studies focus upon the details of French diplomacy in a given issue area, like the building of Europe, the duel with the Anglo-Saxon countries, policy toward the Soviet Union or the Third World. Professor Kolodziej's lengthy study of French foreign policy in the Fifth Republic straddles both of these approaches. He has thus performed a great service not only to students of French politics and foreign policy, but also to undergraduate and graduate students who are just being introduced to this fascinating subject. Kolodziei's study is, moreover, especially valuable now that scholarly interest in the international policy of the Fifth Republic has significantly declined since the death of the first two presidents of the Fifth Republic. It is therefore not likely that any additional or more complete overviews of Gaullist diplomacy will be available for a number of years. Under these circumstances, Kolodziej's study is likely to be one of the major source books on the foreign policy of the first decade and a half of the Fifth Republic.

The study is admirable in several aspects. It fully integrates Gaullist security policy—in Europe, toward NATO and the Soviet Union—with economic, and especially international, financial policy. It integrates policy toward the industrialized West with policy toward the East and the South (and here, it traces the continuities and discontinuities between policies which were developed before, during, and after the process of decolonization). It also links the international policy of Pompidou with the precedents laid out by de Gaulle. All of this is fitting for a study which interprets the most

consistent and grandiose foreign policy in the post-World War II period.

The book suffers from a few weaknesses, most of which stem from the author's effort to focus on foreign policy alone, rather than to dissect the relationship between foreign and domestic policy, or from his emphasis on his own interpretation of the meaning of grandeur in Gaullist policy. Thus, his interpretation of Gaullism is somewhat atemporal, and while the author notes that Gaullism was derived in part from the French president's reaction to events. he minimizes the amount of change which occurred in French policy or style of statecraft. Analysis of domestic politics and its effect on foreign policy is virtually absent, and thus seriously weakens Kolodziej's interpretation of policy under Pompidou. It also leads Kolodziej to neglect the linkage, which began under de Gaulle, between French policy toward the Soviet Union and the government's effort to reduce the influence of the French Communist party in French domestic political life. The author's emphasis on the "illusory" nature of the Soviet threat in the 1960s and his argument concerning the parallelism between French and Soviet goals in the Third World are overstated.

In summary, Kolodziej's study offers no new insights on the French policy of grandeur, but presents a thorough analysis of the major axes of Gaullist foreign policy. Experts will quibble with selective aspects of his interpretation, including his view that "de Gaulle's conception of his authority . . . rested ultimately on his self-defined contract with France, and not the French people" (p. 430). But by and large this analysis should remain a basic reference for some time. Especially valuable for an understanding of contemporary French policy are the chapters on *franc* diplomacy and on Mediterranean affairs.

EDWARD L. MORSE

Council on Foreign Relations, Inc.

The Erosion of a Relationship: India and Britain since 1960. By Michael Lipton and John Firn. (London: Oxford University Press, 1975. Published for The Royal Institute of International Affairs. Pp. xvi + 427. \$32.50.)

The study was planned in 1970 as an essay on the decline of Indo-British relationships since 1960, and became on its completion in 1975 a large reference work on decolonization. The text covers 236 pages; references, notes, and appendices on the tables extent from page

237 to page 303; and the tables, figures, and index end on page 427. This is an important case to study, ponder, and critically assess. It is not a book that can be reviewed in any normal fashion. Furthermore, every year that passes introduces new elements into the argument. For example, the consequences externally of the Indian Emergency of 1975 could not be encompassed in this book, nor have all of the recent trade implications based on oil imports and processing been treated.

The authors started with abundant evidence of a drastic reduction in Indo-British relationships during the 1960s. In 1970, for example, exports from Britain to India were valued at half the amount of 1960. Britons owned about five percent of Indian private capital in 1948, but the proportion had dropped to three percent in 1970, and in the same year 55 percent of India's foreign private capital was non-British. The authors assert that "India is a net donor of technical assistance to Britain" (p. 122), taking into account the cost of training in India of professionals, especially doctors, who ultimately practice in the United Kingdom. Even in military supplies, Great Britain now competes with the USSR and other countries for shares in India. Needless to say, India's diversification of trade, aid, shipping, exchange in invisibles, etc. have altered since the 1960s as have Britain's.

The authors started with a theory, a thesis, a methodological problem, and a determination to be rigorous in seeking evidence. The theory was that decolonization need not lead inevitably to a break or even substantial decline in mutually advantageous, adjusted relationships between a former metropolitan country and a former colony. The thesis is that such a decline did take place between Great Britain and India since 1960, but that it need not have been so, and might yet be reversed by incorporating countries such as India in the longterm benefits of the European Economic Community. The methodological problem was to locate accurate quantitative data on the segments of Indo-British relations (economic, social, academic, professional, media, political) over time, and to construct an equation that might be applied in other cases. The difficulty in finding verifiable quantitative data on many crucial themes, reflected in the text by the heavy use of words such as "about," "approximately," "in general," and other cautious phrases, show that the elusive "hard" equation is not yet ready for presentation because rigorous measurements are not possible at this

This is a book that will be of value to many. The themes include trade, exports (British to India and vice versa), invisibles, private investment, aid and technical assistance, education and research, population exchanges, defense, the media, and political relations. Each part is buttressed by thorough referencing and relevant tables. Economists and political scientists will find each of these sections grist to the mill of contemporary analysis both of Britain and of India.

To this reviewer, the prime utility of the study is as a contribution to the work of what now are called "political economists" in political science. Here is a collection of data, carefully collected and cautiously analyzed, on a major case in decolonization. There is little overlay of ideology or grand theories from the Marxist-Leninist catechism. Where the evidence is thin or absent, the text bluntly says so. Where the data are less than flattering, either to Britain or India, conclusions nonetheless flow from the data.

This is a book that is important enough to warrant a new edition after a few years of battering in the stream of academic and political criticism. There is not a chapter, a subsection, or conclusion unworthy of meticulous reexamination by specialists. The result could be a work on decolonization that might begin to bring rigorous standards to this murky area of pseudo-science in the discipline. Even as it stands, it is a contribution of distinction.

RICHARD L. PARK

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Soviet Naval Policy: Objectives and Constraints. Edited by Michael MccGwire, Ken Booth, and John McDonnell. (New York: Praeger, 1973. Pp. 663. \$32.50.)

One of the great strategic mysteries of our time is why the Soviet Union, the epitome of a continental power, has invested the resources that are necessary to become within a single decade a major, if not the major, navel power. "How serious is the Soviet naval threat?" and "For what purposes will the Soviet fleet be used?" are questions around whose answers hinge a potential naval armaments race, the likely scale of which the world has not yet begun to appreciate.

Soviet Naval Policy: Objectives and Constraints is part of a continuing series of publications by the Political Science Department at Dalhousie University which takes as its mission the analysis of Soviet naval developments.

Though we will not know whether the Dalhousie effort correctly deciphers the objectives and constraints of Soviet naval activities until these objectives and constraints are revealed to us "in due course," one cannot but be impressed by the sophistication of the work to date. Among the many books currently available on the Soviet Navy, the Dalhousie series, and this volume in particular, are the most outstanding.

The collection contains 34 essays and is the product of a seminar held in Halifax, Nova Scotia, in October 1973. It is organized into five sections, each one of which is either set off by a symmary essay by one of the editors or contains one of their own contributions.

The first section is the bravest but of necessity the least successful as it attempts to penetrate the impenetrable, the Soviet decision-making process. In it Matthew P. Gallagher speculates on the role of the military in the formulation of Soviet national policy, Edward L. Warner, III, outlines his view of the bureaucratic politics of the Soviet weapon acquisition process, John McDonnell traces in detail the structure of the Soviet defense industry and the movement of the industry's alumni into the higher reaches of the government, and Philip Hanson analyzes the burden of defense expenditures on the Soviet economy. Although the section's authors are persuasive in arguing that it is likely that the military and related interests form important pressure groups within the Soviet system, they are, as might be expected, ineffective in their attempts to define the extent to which such pressure groups can and do influence policy even in an area as limited as the expansion of naval forces.

The second section explores through case studies the impact of Soviet naval power on the politics of the Third World. Alvin Z. Rubinstein, Uri Ra'anan, Robert O. Freedman, George S. Dragnich, Oles M. Smolansky, and Anne M. Kelly separately examine the situation in the Middle East; Geoffrey Jukes looks at Soviet actions in the Indian Ocean; Charles C. Petersen describes Soviet port clearing operations in Bangladesh; and Robert L. Friedheim and Mary E. Jehn discuss Soviet policy at the Law of the Sea Conference. About all that can be gleaned from this experience is that while the Soviets have thus far gained relatively little from the showing of their naval ensign in distant waters, they are likely to keep trying in anticipation of future benefits.

The third section contains a great deal of background information on the Soviet navy indluding totals for various ship types, a listing of all foreign port visits, and annual statistics for at-sea deployments. It also contains two essays of greater significance. One by Michael MccGwire provides a clear description of Soviet shipbuilding capacity and is particularly useful in sorting out the many classes of Soviet submarines, the backbone of the Red Fleet. The other by Nigel D. Brodeur gives meaning to the data on the size and shape of Soviet ships by comparing the quality and function of Soviet and Western weapon systems. According to Brodeur, the Soviet navy, though inferior on several technological dimensions to its Western counterparts, has added an open sea offensive capability to its traditional territorial waters defensive capability.

The final two sections examine Soviet navel thought and policies. Robert G. Weinland creatively attempts to reconstruct the potential purpose behind Admiral Gorshkov's famous series, "Navies in War and Peace," by analyzing Soviet publication practices. Harlan Ullman weights the counter-Polaris task of the Soviet navy. Bradford Dismukes describes potential roles for the Soviets' general purpose naval forces. And Franklyn Griffiths examines the tactical use of naval arms treaties from a Soviet perspective. But the strength of these sections and in fact, the volume as a whole, lies in the several essays by MccGwire. In one he argues that a major turning point in Soviet strategic and naval policies came in 1961 largely in response to perceived U.S. initiatives. In the second he points out the operational weaknesses of the Soviet navy in fulfilling its various potential missions and especially its need for overseas bases. And in the third he notes that though the Soviets have never embraced the Mahonist notion of the importance of the command of the sea in world affairs, they are certainly willing to use naval power selectively to advantage.

The mystery of Soviet naval intentions remains, however, despite the best efforts of MccGwire and associates. If this volume has a hero it has to be Admiral Gorshkov, not only for presiding over the bureaucratic battles which have given the Soviet navy its opportunity for growth, but also for his extensive writings which are reverently examined here as in other places and which, their bulk not withstanding, reveal so little about Soviet intentions. We know that the Soviet navy serves state purposes, but what those purposes are we still search to discover.

HARVEY M. SAPOLSKY

Massachusetts Institute of Technology

The International Law of Communications. Edited by Edward McWhinney. (Dobbs Ferry, N.Y.: Oceana Publications, 1971. Pp. 170. \$7.50.)

The foundations of international law are indeed political, as this slender volume suggests in so many subtle ways. A nation-state binds itself to and even obeys international agreements to the extent that the conveniences they provide-in the form of agreed-upon technical norms (definitions, measurements, units of exchange), standard operating procedures, and above all predictability in a generally important area of life-outweigh inconveniences caused by constraints imposed upon the state. On matters deemed vital to what decision makers see as "national interests," however, and on which no international consensus has yet emerged, the effort to establish "law" may be premature and fruitless, if not downright deleterious for the continued success of rules on which there is substantial agreement.

The volume comprises 11 papers, many of which were first presented at a colloquium held in the Institute of Air and Space Law at McGill University, Montreal, in May 1970, chaired by the Institute's director, Edward McWhinney. Other authors were currently, or had been, high-level civil servants in international bodies (United Nations, Council of Europe), topranked legal officers in national governments (Nigeria, Brazil, France, India, and the United States), and legal scholars from Canada, Czechoslovakia, and the United States.

Given current trends in international communications and the sponsorship of the conference, it is not surprising that most papers focus on legal developments responsive to or needed because of technological developments in satellite communications. A common theme is the high level of interaction among technicians and political officers in making new agreements. Over the course of the last half-century, technological growth in the communications industry has been astounding: the very notion of space satellites would surely have boggled the minds of early radio pioneers. As in many other areas of regulatory law, both national and international, rulemakers simply must be in constant contact with technologists.

Such interlocking has ramifications for communications law. First, the regulators must perforce rely upon the information of technologists, who can, in effect, define the context within which the rule makers must operate. Still more general policy makers, such as national legislators, often trail far behind in their own conceptions of the issues—an example being the proclivity of United States congressmen to view communications satellites as basically similar to transoceanic cables, as "cables in the sky." Second, the lag in conceptions and prescriptions, which can take several years to overcome in a body such as the International Telecommunications Union (ITU), often hampers technological progress. Technologists complain that they must rely on outmoded equipment or even continue to build incompatible components because regulatory agencies cannot act fast enough in approving new equipment.

Still another consequence is that the interaction of technology and law concentrates information and hence influence in the hands of the more technologically advanced countries, most notably the United States but also the Soviet Union. With one exception, however, the international protocol of the conference did not permit authors to do much more than note the overweening role played by the super powers in creating regulatory bodies (and their agendas) at the international level, express mild resentment at some of the more outrageous attempts by these super powers to swing their weight around, or cite approvingly the super powers' efforts to build up other countries' communications facilities and tie them into their own communications systems-acts that less generous critics, elsewhere, would term the creation of "dependency" relationships.

The exception is Abram Chayes' excellent analysis of "Unilateralism in United States Satellite Communications Policy." It describes how Congress, pushed on by the private communications industry, first turned over control of American communications satellites to a private corporation, Comsat, and then, in the Communications Satellite Act of 1962, tried to create an international system that would be primarily the domain of American business. European resistance forced American negotiators to back down, but the interim Intelsat that emerged nonetheless made Comsat its manager and gave to the United States majority representation that permitted it to veto anything it did not like.

The content of international messages is another area in which the creation of international law has been hampered by national interests. Occasional pieties aside, the only internationally accepted (albeit not necessarily preferred) norms are self-regulation, bilateral and regional agreements to avoid beaming offensive material, and jamming incoming programs. More insidious than outright propaganda, which nation-states may well eschew because they are vulnerable to retaliation, is the transmission of programs or information that

structure people's perspectives (values, images, and predispositons), such as family-situation comedies that predispose people to accept as a norm for their own behavior the high consumption of luxury goods. The control of structural programming—indeed, the question even of whether it should be controlled—is a topic that international legal experts have not yet broached.

RICHARD L. MERRITT

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The United States and West Germany 1945–1973: A Study in Alliance Politics. By Roger Morgan. (New York: Oxford University Press, 1974. Pp. 282. \$22.50.)

The period covered in this study witnessed a diplomatic revolution in German-American relations. Who could have imagined in 1945 that a U.S. ambassador in 1973 would hail before a German audience the "special relationship" uniting his and their country for more than two decades? Time was when a special relationship with Britain had been the center piece of American policy in Europe, and Germany had been the common foe in two world wars. But that was before the Cold War, before the division of Germany, and before the precipitous decline of British power. While British leaders vainly sought to maintain the vestiges of the old bilateral association with the United States, the latter forged a new one with West Germany. According to Morgan, "The most striking theme running through German-American relations in the period of more than a quarter of a century surveyed in this book is the high degree of harmony established between two nations which during the previous generation had been enemies in war." Frequent disagreements notwithstanding, he informs us, "the partnership as a whole showed a degree of mutual confidence rare in the history of relations between two major powers" (p. 247).

Given the title and subtitle of Morgan's book—co-sponsored by the Royal Institute of International Affairs and the Harvard Center of International Affairs—this reviewer welcomed its publication. An innovative, comprehensive exploration of the German Federal Republic's transformation from supplicant to senior alliance partner has long been needed. Most previous studies have either been descriptive accounts of the American presence in West Germany or provided at best only bits and pieces for a more extensive analysis. It is therefore all the more a pity that this book does not live up the promises of its introduc-

tion. There Morgan declares that his study has two objectives. One is to relate the story "as accurately and clearly as possible." The other is to present that story "in such a way as to bring out its significance as a case study in the politics of alliance, some of whose features are likely to be found in other bilateral alliances widely separated in time and space from the particular example selected for analysis here" (p. 1). Regrettably, the book falls considerably short of achieving either aim.

The story Morgan has to tell is essentially a diplomatic history of the relationship between a small group of official foreign policy makers in Washington and Bonn. He allows that "various nongovernmental forces of a psychological or economic character ... obviously had profound effects on the relations between government," but maintains that they must be kept "strictly in their place" (p. 7). As a consequence, what is taken to be self-evident in Morgan's approach is largely ignored in his presentation. His discussion of economic factors, for example, pays little or no attention to the impact of West Germany's trade dependency on its relations with the United States. And his consideration of psychological factors does not go much beyond such curious observations as that Chancellor Adenauer was "obsessed" with the pursuit of German reunification (p. 100). A novel idea, indeed, and so is the notion that the restoration of a united Germany was all along a basic purpose of decision makers in Washington and Bonn (p. 46).

But even strictly on his own terms, Morgan's efforts to tell his story as accurately as possible is flawed. One does not have to subscribe to a revisionist approach to wish that he had made some innovative attempt to re-search and reinterpret the extensive literature on his subject. As it is, we get a summary of the now all too familiar official and quasi-official Cold War version of "what actually happened," along with a detailed who-was-who inventory of governmental leaders and advisers. Morgan thus relies heavily on Adenauer's self-serving *Memoirs* and similar "inside" accounts. For a small part of his story, he has gone to the Dulles papers and oral history interviews at Princeton University; to judge by the results, these were either not especially useful for his purposes or not particularly well used. The book also contains frequent footnote references to "private information" not otherwise identified, but apparently provided by some of the surviving actors in and out of public office. Apart from these sources, Morgan constructs his highly selective tale on a very limited eign policy is a must reading for students of

number of standard secondary and tertiary sources, including newspaper and periodical articles all too often inadequately identified.

Morgan's efforts to tell his story as clearly as possible are hindered rather than helped by his ambitious second objective. Here his intention is "to contribute to the construction of a theory of how alliances operate, by presenting the data of the German-American case within a rigorous framework of concepts." His purpose, as he tells us, "is to demonstrate the explanatory value of these concepts in elucidating the forces at work in the particular case analyzed here, in the belief that the same method will prove valid for the study of other bilateral alliances too" (p. 5). However, Morgan never gives us a clear definition of his concepts nor states his underlying premises. His inductive approach to theory construction involves a good deal of circular reasoning and leads him to adhere rigidly to an organizational scheme that does not come off too well. Each episodic chapter of his chronological narrative disaggregates interrelated data in a sequential presentation of internal politics, bilateral relations, alliance politics, and relations with "the adversary" (i.e., the Soviet Union). Why Morgan chooses to follow this particular sequence is not entirely clear. He arrives at the not very startling conclusion that a common foe was the most decisive factor in German-American relations, which suggests that a reverse order might have served Morgan's purposes better. As it is, his disaggregative schema not only obscures rather than illuminates the interrelationship between his four levels of analysis, but produces a host of repetitious and "as we shall see" statements.

The book ends with a 1974 postscriptdesigned to bring it up to date, but in effect dating it. There is little point in trying to catch up with the latest headlines in such a study and it is anyhow a hopeless undertaking. By all indications we have entered upon a phase of German-American relations very different from those in the era of Morgan's study and nothing is gained from viewing the 1970s in terms of the fifties and sixties.

LEWIS J. EDINGER

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Foreign Policy and Interdependence in Gaullist France. By Edward L. Morse. (Princeton, N.J.: Princeton University Press, 1973. Pp. 336. \$14.50.)

This provocative exploration of French for-

Gaullist foreign policy and, more broadly, of international relations and comparative politics. It argues that French foreign policy represents a special case of a fundamental transformation that has occurred in the relations of states and in the relations of the governed and governing. These systemic changes in interstate and intrastate relations arise from the accelerated modernization of societies around the world, especially in the post-World War II era. Economic, scientific, and technological interdependencies so link national societies that the achievement of any regime's foreign policy objectives are a function increasingly of the policies of other states and actions of other societies and groups.

These transnational forces foreclose the pursuit of an independent foreign policy. The distinction between foreign and domestic policy and politics is blurred; the goals and strategies that a state may pursue relative to its own population or to those abroad are significantly restricted; policy objectives are altered and ultimately transformed in response to these intertwining eco-scientific-technological transactions and the incipient international patterns of interstate behavior and institutions they foster; the domestic foreign policy process is pushed from legislative and cabinet rule to executive and bureaucratic domination; and the gaps that develop between these transnational interdependencies and national control mechanisms to regulate them pose the conditions for crisis diplomacy between and among states pursuing conflicting purposes. French foreign policy tried to be different, to escape these limitations, to be independent; but failed, as evidenced in the May events and in de Gaulle's subsequent resignation after losing on regionalization and Senate reform.

Morse's work is significant on a number of counts. It is one of the few foreign policy works that systematically argues that low politics—involving nonsecurity or diplomatic factors—is the principal determinant of the foreign policy of a modernized state. In looking beyond the anecdotal in French foreign policy behavior and departing from current interpretations of Gaullist behavior, the theoretical importance of the work cannot be ignored. A measure of the book is that analysts who dissent from its diagnosis and conclusions about Gaullist foreign policy are forced to frame their critiques in equally theoretical terms.

There is room for doubt whether the Gaullist case is as suitable as Morse believes for his purpose. He has to posit de Gaulle's search for independence as genuine from the start, and not partly, and importantly, a stratagem itself.

A good prima facie case can be made that de Gaulle acutely understood the limits of state action from the start. He was engaged simultaneously on a number of policy levels (economic, technological, military, and diplomatic), in a number of geographic settings (Western and Eastern Europe, the Atlantic Community, and the Third World), and in a variety of international arenas (United Nations, Atlantic Alliance, European Economic Community) to increase France's (and his own) bargaining power within a refracted and discontinuous system of global politics in which foreign and domestic actors constituted a single field of action potentially subject to the French president's manipulative skills. De Gaulle's ostensible search for personal and national independence and grandeur obscured what he recognized from the beginning was his (and France's) ensharement in the politics of interdependence. Morse attempts to demonstrate what de Gaulle might be assumed to have accepted at the outset of his political career: first as the man of June 18, 1940, who called for allied help and, second, as the beneficiary of the spring coup d'etat in 1958 in which temporary domestic allies were accepted (the defenders of the empire and the remnants of the colonial army) only to be later repudiated.

De Gaulle's conception of the politics of interdependence appears to be broader and deeper than Morse suggests. First, there was the French president's interdependence with the French people and nation. De Gaulle never claimed independence from an electoral test although he repudiated the election paraphernalia of the Third and Fourth Republics. Second, there was recognition of France's dependency on its European partners for modernization. EEC restrictions were accepted in a partial trade-off for the common agricultural policy (CAP). German capital was expected to make French farms and industry more competitive. Third, and most important, de Gaulle tried to turn strategic inferiority and dependency into interdependency which implied increased influence over allied policies. The Algerian War was liquidated and former domestic allies foresworn; the force de frappe was not only counted upon to influence Washington, Moscow, and Bonn but also relied upon to gain control over the army and (from Morse's perspective) to accelerate the modernization and competitive strength of the French economv.

If French foreign policy changed after 1968, one can doubt whether it was due to the transnational movements that Morse identifies in trade, monetary flows, and investment and

their impact on public opinion or to other, more basic factors. There is no mention of the Russian invasion of Czechoslovakia, of the limits set by Bonn and accepted by de Gaulle in his handling of detente with the Soviet Union, or of the tortuous distinctions made by Paris between NATO and the Atlantic Alliance which aimed at maximizing French diplomatic initiative and advantage without a corresponding loss in access to the American security guarantee. There is irony, too, in the price paid by de Gaulle's government for its criticism of Israel and for its subsequent arms embargo after the Six Day War. France's industrial development and continued modernization hinged on Arab oil. While French diplomacy worked for economic advantage abroad, it ran afoul of public opinion at home for its pro-Arab stance, a factor that played a partial role in de Gaulle's defeat in the spring referendum of 1969.

Part of Morse's problem is definitional and conceptual. For Morse, modernization leads to interdependence among states and societies, and interdependence prompts cooperative strategies. It is not fully clear what Morse means by modernization or how it is linked empirically to the transnational processes of interdependence. The linkage Morse makes is more suggested than demonstrated, yet his case study depends critically on establishing the French connection. There is also neither a logical nor an empirically verifiable reason why interdependence should be narrowed to cooperative relations. Both negative and positive interdependencies, as Rosecrance defines them (in World Politics, 26 [October 1973], 1-29), are an invitation to the domestic elites dominating a state's foreign policy to manipulate these conditions to the disadvantage of other states and groups. Strategies of conflict are often more attractive than those of cooperation. Witness de Gaulle's withdrawal from NATO to facilitate detente, which, paradoxically, has fostered interdependencies among rival political units. Finally, the disputes of states over economic questions, turning on questions of monetary reform, trade, investment, and the cost of raw materials, while certainly shaped by transnational factors, are also subject, often decisively, to the bargaining and rule making of governmental elites. These latter processes are only partially the product of the factors Morse focuses upon. Through feedback, they are shaped by the governmental mechanisms of the nation-state in ways that still escape the researcher's probe and scalpel.

These criticisms should not detract from the service Morse has performed. He focuses our attention away from exclusive concern with

strategic and diplomatic considerations in looking for the determinants of foreign policy. He has enriched the study of foreign policy and of French foreign policy in particular. It is a book that deserves close reading and wide circulation.

EDWARD A. KOLODZIEJ

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Comparing Foreign Policies: Theories, Findings, and Methods. Edited by James N. Rosenau. (New York: Halsted Press, 1974. Pp. 442. \$20.00.)

The comparative study of foreign policy represents a subfield of international politics and political science which has certainly evolved and yet at the same time failed to really progress. Such "misevolution" is evident within Rosenau's latest collection of 17 essays (by 12 primary authors). As the subtitle suggests, the volume is organized around theories, findings, and methods, and may be comfortably reviewed around such divisions.

The first essay in the theoretical section by Patrick J. McGowan introduces the notion of "positive" foreign policy theory. The essay itself is in fact more concerned with advancing yet another set of exposes on the Rosenau framework and the merits of social scientific foreign policy analysis. What is particularly distressing is McGowan's repeated reference to Rosenau's theory of foreign policy behavior. More accurately, Rosenau's "pre-theory" constitutes a framework for the analysis of foreign policy behavior and a device for the subsequent development of a theory of foreign policy. Misnomers of this nature are particularly damaging to a subfield already plagued with severe growing pains.

The subsequent essays in the first section by McGowan, O'Leary, and Thorson are all devoted to the exposition of yet another concept of Rosenau's national adaptation. These essays are sometimes interesting yet shed no light upon the more fundamental problems associated with foreign policy theory building.

Section two is devoted to findings. The essays by Rosenau and Hoggard; Powell, Andrus, Fowler, and Knight; Moore; Moore; East and (Charles) Hermann; and Harf are all devoted to assessing the impact which national attributes exert upon foreign policy behavior. What the essays fail to note is that attribute variables cannot exert a causal/explanative impact upon foreign policy behavior because by their very nature they are static and therefore unable to produce explanations which are

 necessafily dynamic. The essays thus fail to employ attribute variables solely as means to the classification of nations and the description of foreign policy.

The essays by Harf, Hoovler, and James, Jr., and (Margaret) Hermann are more interesting and much more productive. Harf, Hoovler, and James, Jr., draw an interesting distinction between dynamic external and systematic variables and employ it convincingly. While they are certainly not the first to draw a distinction between external variables of substantially different natures, they are among the first to apply the distinction quantitatively.

Similarly, Margaret Hermann's essay represents one of the very few valuable quantitative studies concerned with the relationship between idiosyncratic variables and foreign policy behavior. Much like Harf, Hoovler, and James, Jr., she correctly conceptualizes dynamic variables as causal in search of explanations of foreign policy behavior.

The volume's third and final section is erroneously entitled "methods." Like the misnomered first section which deals with only one "theoretical" concept (adaptation), the third section deals only with the methodology surrounding the generation and use of events data. The essays by Hoggard, Burrowes, and McGowan are concerned with the generation of valid events data, while Salmore and Munton are concerned with the development of a typology of foreign policy events. These essays are interesting-although highly specializedand constitute a logical end to a volume which increasingly moves away from the general and substantive and steadfastly toward the specialized and abstract.

The specialized and abstract nature of the volume serves to narrow the audience which might find it interesting and comprehensible, and virtually eliminates any possibility of the essays contributing to the formulation and implementation of actual policy. The quest for policy relevance is thus hardly advanced by this latest Rosenau reader.

The real value of the volume lies in its ability to make us look—once again—at the subfield of foreign policy analysis. Where is it heading? Where has it been? Clearly, while volumes like the one at hand are interesting to a decreasingly identifiable group of scholars, they do not advance or well publicize the subfield. If little else, they illustrate the "coherence and consensus" of but a few, and fail to exploit creatively the disarray which other subfields manage to deal with productively. While many of the essays might have served usefully as isolated articles or research notes, together they

do not generate a whole greater than the sum of its parts.

STEPHEN J. ANDRIOLE

Decisions and Designs, Inc., McLean, Virginia

The End of Nowhere: American Policy toward Laos since 1954. By Charles A. Stevenson. (Boston, Mass.: Beacon Press, 1972. Pp. 367. \$8.95.)

This account of the vicissitudes of U.S. policy toward Laos opens with the first Geneva Conference and ends in the fall of 1971 when the clandestine American-supported military operations against the Lao Communists were gradually emerging into the limelight of public discussion. The author showed prescience when in judging the final outcome of the American efforts he speculated (p. 269) that "by insisting on a whole loaf, the United States may wind up with only a charred crust." The anti-Communist Royal Lao Government, maintained in power only thanks to intensive American bombing and massive economic aid, has since fallen and American influence has vanished from Laos. On December 3, 1975, the Pathet Lao announced the abolition of the monarchy and the establishment of a Lao People's Democratic Republic. This study attempts to explain, how American policy leading to such results was formulated and why it eventually failed to attain its objectives.

The author has painstakingly pieced together the available information on the evolution of the American role in Laos. He has combed with great care through the open literature and studied the extensive congressional hearings on the subject. Many U.S. officials involved in the making of Laos policy granted him interviews. The 86 respondents included such key figures as Richard Bissell of the CIA, Chester Cooper of the NSC, Admiral Arthur Radford, William Bundy, and Averell Harriman as well as most diplomats who served as U.S. ambassadors in Vientiane.

Yet, the reader may wonder whether a history based on the public record can do justice to the complexities of American involvement in a remote country where a secret proxy war was being waged under CIA auspices and where American officials were under instructions to avoid any open violation of the Geneva Accords. The publication of the Pentagon Papers provides an answer to this question. The papers appeared after Stevenson completed his manuscript, but he had occasion to peruse them before it went to the publisher. In a postscript

to his study, the author summarizes what he learned from reading these documents and how they modified his interpretation of events. On the whole, Stevenson believes that his conclusions stand up rather well, but admits that this new evidence forced him to modify his judgments in some instances. The reader might find it useful therefore to read this study in conjunction with the Pentagon Papers.

The eight chapters detailing the history of U.S. involvement in Laos between 1954 and 1971 (the appendix contains also a helpful chronology) are preceded by a brief introduction and followed by a more substantial chapter presenting the author's conclusions. As suggested by the book's title, the focus of the study is on American policy rather than on developments in Laos. (For information on the latter, Arthur Dommen's Conflict in Laos remains indispensable.) As a result, the reader without background knowledge about Laos never gets a feel for the country and for the people the U.S. policy makers were attempting to influence and direct. A broader and more penetrating introduction to the Lao setting of U.S. policy might have helped to fill this gap.

Stevenson's study is both an historical account and a case study in U.S. foreign policy and decision making. He sees the American failure in Laos as rooted primarily in the exaggerated strategic importance attached by the U.S. government to that country and the "obsessive anti-communism" (p. 240) of American policy. The United States thus was never able to adjust to what he terms the political realities of Laos. But he assigns much responsibility also to the behavior of contending U.S. bureaucracies-the State Department, AID, the Pentagon, and the CIA. Each had its own perspective, shaped by its primary interests, and each acted accordingly. The author's analysis is rich in observations regarding the ways in which such differences in bureaucratic perspectives tended to be reflected in the policy-making process and how they prevented the United States from altering its course. Despite the author's heavy emphasis on the analysis of the bureaucratic process and its consequences, he appears to conclude that in the end it was primarily not change of U.S. policy machinery that was required in Laos, but a change of minds. Writing in 1971, he suggested that greater American sensitivity to the conditions of Laos and a greater willingness to accept Pathet Lao participation in the government might have obviated the need for the kind of destructive warfare that charred Laos and turned a large portion of its population into refugees.

PAUL F. LANGER

Rand Corporation

Nuclear Proliferation Problems. By the Stockholm International Peace Research Institute. (Stockholm: Almqvist and Wiksell and Cambridge, Mass.: The MIT Press, 1974. Pp. 312. Swedish Kroner, 58.00.)

After the People's Republic of China exploded its first nuclear device in October 1964, a spate of books and articles were written which predicted that the club of nuclear weapons states would rapidly increase to 10, 20, or more. It was rather surprising, therefore, that it took almost a decade before India became the sixth nuclear nation by conducting a "peaceful nuclear explosion" in May 1974.

Why were these predictions in error? In retrospect, a number of considerations relevant to nuclear proliferation were not well appreciated. Security guarantees provided assurances to some states while constraining their movement toward an independent nuclear capability. The domestic opposition to the nuclear option within certain countries was underestimated. The economic and technological wherewithal necessary to embark on a nuclear weapons program was beyond the reach of many "threshold" states. Some governments did not value highly the political payoff of going nuclear. And, for most nations, the weaponsgrade material required to make bombs was simply unavailable.

The Indian explosion and the prospect of the rapid spread of nuclear energy-related technologies, stimulated in part by the 1973 OPEC oil embargo, have now once again raised serious concern about the spread of nuclear weapons. This concern has been reflected in a wealth of publications within the last two years on the technical, economic, and political dimensions of nuclear proliferation.

Among the first of these publications was this volume produced by the Stockholm International Peace Research Institute (SIPRI). Indeed, the SIPRI volume was planned prior to the Indian explosion. It is the product of a conference convened by the Institute in June 1973 which was intended to speak to the issues discussed at the May 1975 Review Conference of the Treaty on the Non-Proliferation of Nuclear Weapons (NPT).

The volume is divided into four parts. The first is a series of nine chapters written by five

different authors summarizing various aspects of nuclear technology. The second focuses on the issue of NPT safeguards and techniques to deal with nongovernmental nuclear proliferation. The third is a relatively brief discussion of the prospects for cooperation in the peaceful applications of nuclear energy. And the fourth is a collection of analyses on security problems facing non-nuclear weapon states, including separate chapters on Italy, Japan, India, and Israel.

The book has a number of obvious points in its favor. Contained within its covers is an extraordinary amount of detailed information, particularly on the technology of nuclear power. Moreover, the inevitable unevenness of a multiauthor format is more than compensated for by the differing perspectives brought by the contributors, who are drawn from 12 different nations, are authorities in their fields, and represent a variety of independent, national government and international organization viewpoints. The fact that this volume has been widely used as an information source in Congressional hearings and other forums devoted to nuclear proliferation testifies to its overall value (although this value is diminished by the absence of a much-needed index which would enable it to be used more easily as a reference work).

Unfortunately, however, events are now rapidly overtaking much of what this volume has to offer. The last part of the book, and potentially the most interesting, is how hopelessly out of date. The chapter on Italy and the nuclear option was written prior to the Italian ratification of the NPT. The same holds true for Japan, which deposited its instrument of ratification in June 1976. The discussion of Indian attitudes toward the NPT must now be evaluated quite differently in light of India's demonstrated nuclear capability. The chapter on Israel, while containing some useful historical information, necessarily suffers from having been written before the October 1973 war. And a final chapter on European security and the NPT is of limited value because of the introduction of the issue of tactical nuclear weapons into the Mutual Balanced Force Reduction (MBFR) Talks, the ratification of the NPT by the Federal Republic of Germany, and the continued vigor of German and French nuclear exports.

Moreover, the less time-dependent sections dealing with nuclear technology, NPT safe-guards, and the peaceful applications of nuclear energy are nonetheless increasingly dated and incorrect. Substantial revisions have been made in the last two years concerning the estimated demand for nuclear power, the economic viabi-

lity of the fast breeder reactor, and the status of uranium enrichment technologies, particularly with respect to laser isotope separation. The prospect of nongovernmental nuclear proliferation has now been exhaustively treated in the highly controversial work by Mason Willrich and Ted Taylor, Nuclear Theft: Risks and Safeguards, which advances the case far beyond Willrich's brief essay in this book. And the issue of peaceful nuclear explosions, discussed briefly by V. S. Emelyanov of the Soviet Union, has been treated exhaustively in a recently signed agreement between the United States and the Soviet Union related to the Threshold Test Ban Treaty.

The likelihood of nuclear proliferation in the next ten years, unlike a decade ago, is quite high: security guarantees are weakening; domestic opposition in many nations to the nuclear option is diminishing; the economic and technological wherewithal is becoming more readily available; the political payoffs for a variety of states appear to be substantial; and perhaps most importantly, the ability to obtain weapons-grade material is on the increase.

The incentives and disincentives on the nuclear decision require thorough examination, as does the assessment of viable nonproliferation strategies such as nuclear supplier agreements and multinational nuclear facilities. SIPRI would do well to turn next to this task.

MICHAEL NACHT

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World Armaments and Disarmament: SIPRI Yearbook 1974. By the Stockholm International Peace Research Institute. (Cambridge, Mass.: The MIT Press, and Stockholm: Almqvist and Wiksell, 1974. Pp. 526. Sw. kr. 75.00.)

This is the fifth in a commendable series of World Armaments and Disarmament Yearbooks compiled by the members of SIPRI, an independent institute financed by the Swedish parliament. It is a massive undertaking and one must think a discouraging one to the members of the institute who are basically opposed to the arms race and armaments and yet conclude that "unfortunately the events of the previous year have not provided grounds for altering our assessment of the lack of progress in disarmament.... In our view, disarmament-with abolition of nuclear weapons having the highest priority and leading to general and complete disarmament-is essential if nuclear holocaust is to be avoided. It is our basic commitment to

effective disarmament that makes inevitable the 'harshness' of our judgment of the value of existing arms control measures" (p. v, italics in original).

The yearbook is divided into four sections which detail the substantiation for this judgment. "Part I: 1973, the Year in Review" discusses in two chapters military developments in the Middle East and Southeast Asia and then devotes separate chapters to mutual force reductions in Europe, nuclear deterrence strategies, and new trends in strategic nuclear weapons developments. The analyses are detailed and comprehensive, if not always prescient. For example, the scant mention of the development of the cruise missile does not hint at the difficulties this new system has brought to the extension of the SALT agreements. Reading the text, accompanied by graphs of always rising weapons levels and expenditures, one senses SIPRI's growing frustration with the slippery debates of deterrence theorists and with the burgeoning U.S. and USSR nuclear arsenals. Finally, the section concludes: "No nuclear strategy, past or present, based on deterrence is credible to rational persons, particularly when the powers concerned are equally armed. Such strategies must be seen for what they are-mere rationalizations for the development and deployment of weapons made available by military technology" (p. 70). Of course, this is too simple since not all weapons systems which could be built are built. Nonetheless, from the institute's point of view, it is certainly too true since many weapons systems which have no mission soon find one, if only as a bargaining chip. In fact, the submarine-launched cruise missile was itself part of what Secretary of Defense Laird called, in 1972, a "triple play for peace," the three elements being the ABM treaty, the Interim Agreement on Strategic Weapons, and finally the president's national security budget replete with bargaining chips such as the cruise missile.

"Part II: Developments in World Armaments," the lengthiest part of the yearbook, consists of two chapters, "The Dynamics of World Military Expenditures" and "World Armaments, 1973." The first is a brief discussion of the rising world military expenditure. SIPRI estimates that in 1972 \$207 billion was spent on world military expenditures and that this represented 6.5 percent of the total world output. This is slightly smaller than the 7 to 8 percent share claimed in recent Vietnam war years; but as pointed out, it is a much higher proportion "than those which prevailed in any other peacetime period in recent history: in the years preceding World War I, and the inter-war

years, the proportion was some 3-3.5 percent" (p. 123). This expansion is attributed to a variety of reasons, including the rising cost of personnel; but the main reason is ascribed to the "technological nature of the armaments process" (p. 134). One wishes that this claim could be substantiated more completely than is done here.

The chapter "World Armaments" begins with a brief introduction outlining trends in military expenditures. However, the heart of the chapter is in its appendices, including an excellent discussion of the problems of estimating Soviet military expenditures and some 80 pages of tables showing, since 1952, annual military expenditures for each country. The tables are nicely arranged by region and by defense alliance. Most tables appear twice, once with the entries in constant 1970 U.S. dollars using 1970 exchange rates and then in local currencies and current prices. Last are two lengthy registers, one of indigenously designed weapons in development or production in 1973, and one of licensed production of foreign-designed major weapons.

"Part III: Advances in Technology" consists of three chapters covering reconnaissance satellites, antisubmarine warfare, and the automated battlefield. They are brief but elaborate reviews of such systems as are now available to the U.S. and the USSR. Here the coverage is biased, by the availability of data, toward the capabilities of U.S. systems. However, there is a lengthy list of Soviet reconnaissance satellites launched in 1973, and two figures showing ground tracks of Soviet maneuverable and unmaneuverable satellites over the Middle East during the October War of 1973 demonstrate their usefulness and some of their capabilities. The section on antisubmarine warfare emphasizes the current stability of the seabased deterrent forces of the U.S. and the USSR and concludes with a few timid suggestions for enhancing this stability. including the development and deployment of a new long-range missile.

Part IV ends the yearbook with a review of disarmament negotiations in 1973, a review of the status of the implementation of agreements related to disarmament such as the Seabed Treaty, the Nonproliferation Treaty, and the Treaty of Tlatelolco, and finally with a chronology of disarmament-related events of 1973.

The yearbook is an impressive compendium of facts and analysis and an excellent bibliographic tool. It would be slightly enhanced if more of the tabular data appeared graphically; but it should be of considerable value to all who are interested in either the numerology or the nuance of armaments and disarmament. It

is a welcome companion to the preceding SIPRI yearbooks.

MICHAEL D. ROSENTHAL

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Normalization of Japanese-Soviet Relations 1945—1970. By Savitri Vishwanathan. (Tallahassee, Florida: The Diplomatic Press, 1973. Pp. xii + 190. \$15.00.)

"Geographic closeness breeds special interests between nations," Vishwanathan asserts in the preface to this short account of postwar Japanese-Soviet relations. While one may not challenge this assertion, it is important to understand why "special" ties have yet to develop between these two countries—despite their geographic propinquity.

The thesis of the book, written by an Indian scholar at the Jawaharlal Nehru University in Delhi, is that Japan's postwar Soviet policies have been shaped almost entirely by outside forces, and especially by the U.S.-Soviet relationship. Both superpowers, she argues, have consistently adapted their policies toward Japan to fit the more compelling needs of their mutual rivalry: Washington has pressured Japan to limit its political and economic commitments to the USSR; the Soviet Union has continuously sought to enlarge its role in Japan and to loosen the American grip there by assorted, and mostly futile, tactical maneuvers. Japan's latitude in setting its Soviet policies has been a direct function of these contending pressures and of Soviet-American tension levels. Thus, Japanese independence was lowest during the peak Cold War years and most extensive during the recent "era of détente."

The author's preoccupation with external factors leads her to assign a clearly secondary role to domestic Japanese political factors. She discusses some of these domestic elements, such as interest groups, public opinion, and intraparty factional conflict, but not systematically. For example, she does not assign relative weights to the different party factions and interest groups favoring, and those opposing, normalization of relations with the USSR during 1955-56. Also, the positions of the political parties are not defined clearly. Further, she treats Japanese opinion data uncritically and sometimes without giving references (e.g., that Soviet objections to U.S. occupation actions on fishing "antagonized public opinion," pp. 40-41; and that Communist China's participation in the 1951 peace treaty "echoed the sentiments of the Japanese people" p. 48).

A more dynamic approach to the subject would have included not only the description and correlation of these domestic political factors but also the identification of some of the linkages between the domestic and international forces that have influenced Japanese policy choices. Vishwanathan writes persuasively that Japan has benefited from the postwar rivalry between the superpowers, but her related contention-that Japan has sought actively to exploit this rivalty-is far less convincing. For instance, she maintains that Japan chose to align itself with the U.S. in 1952 because of the "Soviet-American split" (p. 49) and the U.S. seemed "stronger" than the Soviet Union (p. 144). Could these have been the reasons? In any case, if the author's portrayal of Japan as a pawn in the superpower struggle is accurate, how could the Japanese government have chosen its principal ally-a truly landmark decision-at the zenith of Cold War tension? And, most broadly, if American pressure and high superpower tension levels have caused Soviet-Japanese relations to be limited, then why has the reduced U.S. pressure and détente of recent years not brought the two countries closer?

The point is simply that Vishwanathan's rather deterministic approach neglects the central element of choice. No doubt superpower rivalry has broadly affected Japanese policy choices, but it has not made any decision inevitable. Japan has not been an American satellite-it has made its own decisions according to calculations of what is best for Japan. In these calculations, moreover, the U.S. and the USSR have not had an equal impact-the coalition of business and conservative political leaders responsible for Japan's postwar foreign policies and the vast majority of the Japanese people have preferred an alliance with the United States. A Soviet "alternative" has not been seriously debated. It should be remembered, too, that the Soviets have not given high priority to seeking a rapprochement with Japan. Moscow's inability to improve its image in Japan has stemmed from its stubborn unwillingness to make the compromises on trade, territory, and fishing rights that might have eased deep-seated Japanese suspicions. At bottom, therefore, neither Japan nor the Soviet Union has worked very hard at building a "special" bilateral relationship.

Vishwanathan uses Japanese and English materials on selected Japanese-Soviet negotiatory encounters on fishing, trade, territory, and the repatriation of Japanese prisoners of war. Much of this material is dated, however, and the book's focus is almost wholly (and curious-

ly in view of the title) upon events before 1960. The book is also in need of a firmer editorial hand—translations of key Japanese groups are sometimes omitted, and there are distracting misspellings and errors in dates and names.

The major shortcoming of this book, however, is that the author allows her obvious desire for improved Japanese-Soviet relations to cloud her perspective of what a more detached observer would surely see as a relationship purposefully kept limited by both sides because of a combination of psychological, historical, and political reasons. Vishwanathan may be right in expecting that detente will usher in a "new era" in Japanese-Soviet affairs, but wishing does not necessarily make it so.

MICHAEL K. BLAKER

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International Economics of Pollution. By Ingo Walter. (New York, Toronto: John Wiley, 1975. Pp. 208. \$17.50.)

The cover blurb on Professor Walter's book informs us that he is a consultant to OECD, which is very appropriate since the chief merits and limitations of his book are those of an OECD report. Walter is a specialist in international trade and finance with a particular interest in environmental issues. His book is competent and thorough. If you wish to compile a list of several dozen ways in which the internal environmental policies of trading nations can subtly influence the international flow of commodities and capital, Walter will provide that for you, carefully detailing each in a way that a modest background in economics will allow you to follow. If you want to bring yourself up to speed on PPP (polluter pays principle), TFP (trans-frontier pollution), or the potentially constructive role MNC's (multinational corporations) could play, then again Professor Walter is your man. In general, if you are doing political research that deals with international economic impacts of environmental policies, you will do well to have a copy of Walter's text on your shelves. It is a very useful handbook.

The book's principal intellectual commitments are to a wholesome environment and to free trade, but with rather more fervor (I sense, but perhaps Professor Walter would strongly disagree) for the latter. The goal of socially responsible governments and corporations should be to protect and enhance the environment in such a way as to minimize unwarranted effects on international trade: specifically, Pro-

fessor Walter is concerned about the prospect that environmental policies would be distorted in ways which, in effect, serve to protect domestic industries—providing the functional equivalent of tariffs, export subsidies, import restrictions, and so on.

Ideally, all actors should pay the full social cost of any damage due to their activities. We may desire, in principle, that the owner of a car pay the costs of pollution emitted during its use, and that the manufacturer pay the costs of pollution due to the production of the car. But any such simple principle runs into numerous practical problems. It is almost always hard to get agreement on the social costs of pollution; ambiguities arise about who is properly responsible for the damage done by pollution; firms, work forces, and communities vary in their ability to adapt to environmental constraints. and consequently in their ability to pay; and so on. This makes it difficult to lay down clear-cut rules for who ought to pay how much for various kinds of pollution. These problems would arise even if every actor was wise and reasonable and clearly motivated by some abstractly definable sense of "social responsibility." But since real actors are not always wise or reasonable, and certainly not always motivated by a common sense of social responsibility (all the more so across international boundaries), the potential exists for a good deal of mischief, aggravated by suspicion about what is motivating others.

As an example, a controversy has lately arisen between Japan and its OECD partners (but chiefly the U.S.) about Japan's emissions requirements for automobiles. The Japanese see their regulations as the proper response to the particularly severe pollution problems of Japanese cities; others, particularly American car makers, see the regulations as a device for making it difficult to market foreign cars in Japan. Similar controversies arise in connection with strict limits on DDT contamination of beef which have had the effect of closing European markets to Uraguayan beef; or regarding the reluctance of England and Germany to require controls on sulphur emissions (or provide compensation) that takes account of the consequent damage done by acid rains over Scandinavia.

What Professor Walter does is provide us with a comprehensive survey of the kinds of difficulties we can expect to arise. He also provides a good deal of normative advice on how reasonable and socially responsible nations ought to behave (or, equivalently in most economists' usage, how rational actors alert to their long-run self-interest ought to behave).

This is much less useful, and even at times a little tedious, probably because it is much easier to lay down admirable principles than to provide operationally meaningful criteria for what is reasonable when admirable principles conflict, as they so often do. This difficulty is aggravated by an almost total absence (either due to lack of space or to a reluctance to say anything controversial) of discussion of concrete cases.

Are the problems that Walter discusses live ones or only abstract possibilities? In other words, are national environmental policies being significantly distorted by real or imagined concerns about adverse effects on a country's international economic position? I gather that Professor Walter does not think such problems are yet getting out of hand; and that (much more casually) is my own impression. But the set of issues no doubt warrants some concern; and the very limitations of a narrowly economic approach to a problem that is richly political suggests that useful contributions can be made by political scientists. A reading of Walter's book would be a good way to begin such an inquiry.

Let me add that a topic of great political interest, but which lies largely beyond the scope of Walter's treatment, is what we might call the "economics of international pollution," or better, "the political economy of. . . ." I mean here pollution which has impacts which approach being, or literally are, global in scale, such as the depletion of the earth's ozone shield, or the potentially far-reaching second order effects of Soviet proposals to reverse the flow of certain Siberian rivers. On this kind of problem anticipated costs and benefits are quite likely to be vastly different for different countries; many (or all) countries will have a stake; and the very scale of the phenomena makes even small risks of large consequence, and/or substantial probability of even very hard to observe effects sufficient grounds for real concern. We can expect such issues to play an increasingly prominent role on the agenda of international politics in the decades ahead.

HOWARD MARGOLIS

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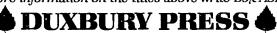
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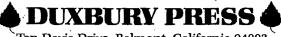
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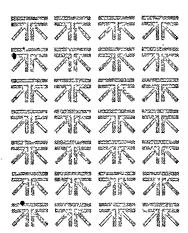
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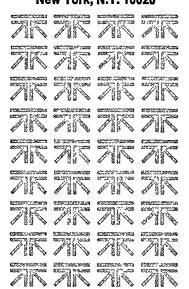
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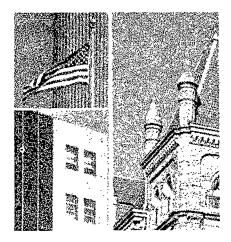
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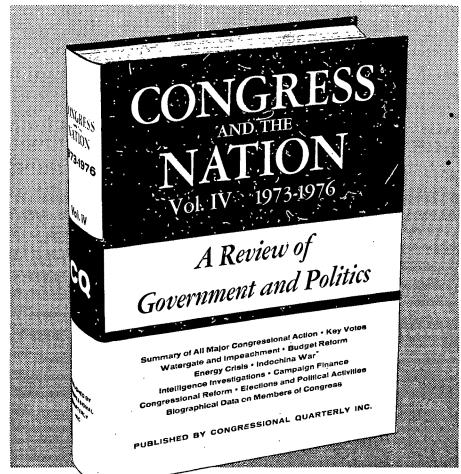
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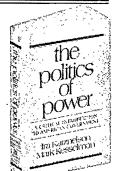
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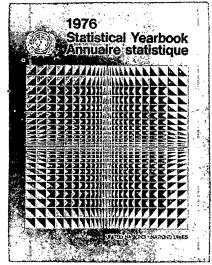
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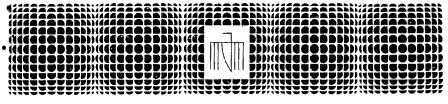
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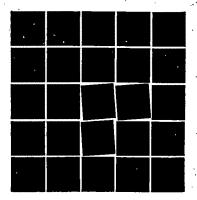
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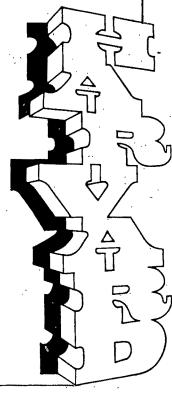
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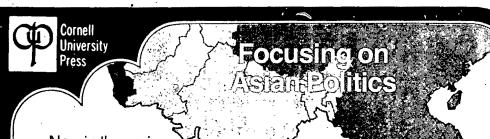
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